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James Joseph Sylvester.

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"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

THURSDAY, NOVEMBER 1, 1888.

GRESHAM COLLEGE.

MR. GOSCHEN—speaking on behalf of the London Branch of the Lecture Society which was started by Prof. James Stewart, of Cambridge, for the purpose of giving remunerative employment to some of the younger graduates of Oxford and Cambridge, and at the same time of affording instruction and amusement of an intelligent character to such audiences as the larger manufacturing towns afford—has publicly urged the claim of the Society to enter upon Gresham's heritage, and by the aid of the funds still in the hands of his trustees, and of such moneys as those trustees may think it incumbent upon them to restore to the Gresham trust, to carry out the purpose of that great founder, who, two hundred and fifty years ago, bequeathed property, now valued at several millions sterling, for the purpose of maintaining a College of Professors in London. There is no question as to what were the intentions of Gresham, nor as to the disgraceful nature of the transactions by which his trustees—the Corporation of London and the Mercers' Company—a little more than one hundred years ago were enabled to seize the property of the trust, and, with the sanction of an Act of Parliament, to assign a mere fraction of it to the payment of half a dozen lecturers, whilst appropriating the bulk of it to their individual and corporate use.

It is beyond question that the existing representatives of the Corporation of London and the Mercers' Company are ashamed of the neglect and spoliation of which their predecessors, in a corrupt age, were guilty. They would be glad to assign the money with which they at present pay so-called "Gresham Professors," and even a large additional sum, representing the misappropriated trust funds, to an institution more truly representing Gresham's purpose than the lecture-room now existing at the back of Mercers' Hall, in the heart of the City, could they be assured that any one of the various plans which have been from time to time urged upon them was really a wise and true method of carrying out that purpose.

We venture to think that Mr. Goschen has merely added to the perplexity in which Gresham's trustees find themselves by his ill-timed proposal that his Lecture Society should be supported by the funds disposed of by those trustees. The lectures given by this Society are, we feel assured, excellent in their way, and we do not doubt that they give a large amount of pleasure and of useful information to the persons who attend them. We are aware that the lectures are more serious in scope than the series of popular lectures frequently arranged by lecture associations, and consist of short courses, in which one teacher is able at some length to explain the outlines of his subject, instead of isolated lectures by numerous individuals on disconnected topics. It is only reasonable that any public or semi-public institution, having a lecture-theatre at its disposal, should encourage so excellent a Lecture Society as Mr. Goschen's, by giving it the use of rooms from time to time. Thus the various Vestry Halls of London may be (and we believe have been) made use of. The London Institution in Finsbury Circus, University and King's Colleges, and the University of London could easily lend a lecture-theatre from time to time to Mr. Goschen's *protégés* as they have to other similar Societies. And it is not unfitting that Gresham's trustees should lend the little-used theatre of the Gresham Professors for the same purpose. When, however, Mr. Goschen and his friends take advantage of this hospitality to urge that not only should Gresham's theatre be lent to them, but that Gresham's money should be assigned to the support of their lecturers, it seems to us that an unwarrantable pretension is put forward, and one which is to be deprecated on very special grounds. Those grounds are as follows.

Gresham's foundation was assigned by him to the support of a body consisting of seven learned men, to whom he proposed to furnish, not a mere fee for a short course of lectures, but a life-provision—in fact, a residence, laboratories, and the means of research, as well as a stipend, at the highest rate at which such persons were paid three hundred years ago, as shown by the payments made to the Professors and College officials of Oxford and Cambridge. Gresham assigned his own palace and garden, situated where Old Broad Street at present runs,

for the dwelling-place of the Professors of his College; and here the first Gresham Professors did reside, and not merely give instruction to the citizens of London by means of lectures, but—what was far more important—carried on their studies and researches. Here the Royal Society met in its early days, and here, in fact, were the head-quarters of learning and science in London.

It is clear enough that what Gresham intended to found, and what actually was constituted by his trustees in the year 1596, was an institution similar to the Professorial Universities of Scotland and Germany of the present day. He distinctly founded seven Professorships—viz. of Physic, Law, Rhetoric, Geometry, Music, Astronomy, and Divinity—and ordered that the proceeds of the rents derived from the shops and houses around the Royal Exchange which were his property should be used in paying each of these Professors £50 a year—no small sum at that time, since the yearly value of Gresham House itself and the gardens attaching to it was, at the date of Lady Anne Gresham's death, in 1596, estimated at only £67!

In view of these facts, it is idle to pretend that the Lecture Society has any similarity to the institution designed by Gresham. Whatever good Mr. Goschen's Lecture Society is doing, it is not doing the work which Gresham intended his College to perform, although Mr. Goschen tells us that he thinks that it is.

To subsidize a series of innumerable short courses of lectures by innumerable young men of small experience is a totally different thing from housing and providing for life seven chosen teachers—the best, the most skilled, the most original in discovery, the most masterly in discourse, worthy to represent science and learning in this great city of London.

By the former course you may diffuse a little knowledge amongst a great many people who will not themselves pay for the pleasure thus presented to them. This is Mr. Goschen's plan. By the second you hold before younger men a prize to stimulate their endeavours; to the matured and chosen teacher you give the leisure and security necessary for research—that is, for the making of new knowledge; to the citizens of London you assure the presence in their midst, and the continual teaching, of the ablest discoverers and philosophers. That is Gresham's plan.

It may be, and, indeed, has been, argued that it is impossible to carry out Gresham's plan, and that the best thing to do with whatever can be got together of his trust funds is to administer it on the principle of *çy-près*, and, accordingly, to let Mr. Goschen's Society have it.

To this we reply that Mr. Goschen's Society has no claim whatever upon this principle, since there are institutions in London—namely, University and King's Colleges—which come near to realizing Gresham's intentions, and if endowed by his funds would actually realize them, whilst Mr. Goschen's Society is as different from Gresham's College as a pic-nic is from a military expedition. A very objectionable use is made of the word "University" in the endeavour to gain support for the Lecture Society. It is spoken of as a "Society for the Extension of University Teaching," and more briefly as "University Extension." The implication is that the teaching is such as is given at Universities, and it is an entirely false implication. The teaching given at Universities depends for its character on two chief factors—firstly, the selection and consequent ability of the teacher; and secondly, the continuous and entire devotion of the student's time to the training and instruction provided for him. In both these factors the Lecture Society differs *toto cœlo* from even the most eccentric University, and has no claim to employ that much misused term. Yet it is by taking advantage of the misconception created by its use in connection with the Lecture Society that a claim has been made for this Society both to take part in the organization of a new University of London and to benefit by Gresham's trust, which it is rightly alleged was intended for the introduction into London of University teaching.

If the present representatives of Gresham's trustees—the Corporation of London and the Mercers' Company—would simply carry out the provisions of his will as nearly as possible—much as they were carried out in the year 1596—all would be well, and the contentions of rival claimants to a share of the pickings still to be got from the bones of Gresham College would be silenced.

The original Gresham College began well enough, and caused the greatest satisfaction to the citizens of London. The lectures were largely attended, the Professors were men of great distinction, and a long and useful career was foreseen for the College. A similar institution—the Collège de France—was founded in Paris by the French King about the same time. The Collège de France exists to this day, and is one of the most effective and valuable institutions in the world for the production of new knowledge.

Our London College perished simply and solely through deliberate jobbery and corruption. The trustees purposely neglected their trust; incompetent persons were appointed by them to the Professorships; they themselves stole the land round about Gresham House, and excused the Professors from lecturing in order to avoid prosecution by the Professors for arrears of salary. In the beginning of the eighteenth century Gresham College was an object of contempt and derision to the citizens of London. The trustees had ruthlessly and systematically plundered the trust-funds and prostituted the Professorships, so that no one raised even a feeble protest when the work of perfidy was consummated, and Gresham House was pulled down, the site handed over to the Excise Office, and the worshipful trustees were spared all responsibility as to their dealings with property worth some millions at the present day, in consideration of a payment of £500 a year.

There are those who maintain that, were Gresham College reconstituted at the present day, it would have the same fate. We are not disposed to believe this. It was, no doubt, a mistake on Gresham's part to place such absolute confidence as he did in the Corporation of London and the Mercers' Company. We have invented, since Gresham's time, methods for keeping a check on erratic trustees; but what is of far greater importance is, that at the present time there is a real and earnest desire on the part of the great City Companies to do service to the State and honour to themselves by employing the funds in their possession for the good of the community. It is not improbable that—were a scheme for the establishment of a thoroughgoing Professorial University in London

(similar in its aims and methods to Gresham's College, and by no means similar to Mr. Goschen's Lecture Society) forthcoming as the result of the deliberations of the Royal Commission now sitting to consider the question of the future University of London—the present representatives of Gresham's trustees would be willing and anxious to redeem the past by endowing in that University seven or more Gresham Professorships, with a sum representing in adequate degree the property long ago misappropriated by their predecessors. Sir Thomas Gresham, the greatest and most generous of merchants who ever desired to benefit the City where he lived and prospered, the man who, above all others, has been most shamefully betrayed by those whom he trusted and loaded with gifts, may yet be honoured and justly dealt with. It rests with the Corporation of London, and the Worshipful Company of Mercers, to give to the future University of London, Gresham's name and Gresham's money.

E. RAY LANKESTER.

BACON.

Bacon. By R. W. Church, Dean of St. Paul's. (London: Macmillan and Co., 1888.)

THE handsome volume before us, which forms the fifth volume of Dean Church's collected works, is a reprint (with, apparently, few or no alterations) of the small book on "Bacon," which originally appeared in Mr. Morley's series of "English Men of Letters." Like every literary composition which falls from the pen of its author, it is a model of candour in treatment, and of gracefulness in style. Other accounts of Bacon may be more profound, more detailed, or more appreciative, but certainly none is likely to be more interesting or attractive to the general reader.

The early chapters, constituting the larger portion of the book, are occupied with Bacon's life, and therefore, by implication, with the never-ceasing controversy about his character, conduct, and motives. On these topics, Dean Church's judgment decidedly inclines to the side of severity; nor does he, as it seems to us, make sufficient allowance for the temptations to which Bacon was exposed, arising largely from his financial embarrassments, the peculiarly difficult positions in which, as in the case of Essex, he was sometimes placed, or the habits and circumstances, so different in many respects from our own, of the times and circles in which he lived. At the same time, the sentence, however decisive, is always delivered in kindly and gentle tones, as that of a judge who regrets, rather than denounces, the faults which he condemns. The judgments of Dean Church, even when we regard them as erroneous, always demand our attention, and perhaps all the more so, because they are entirely free from the asperity and ferocity of tone which mark the utterances of some others of Bacon's more recent critics.

But our business is not so much with the chapters on Bacon's life and character as with the chapter on his philosophy. Here Dr. Church mainly follows the lead of M. de Rémusat, and consequently his account, though reflective and suggestive, and often singularly felicitous in expression, appears to us to be wanting in the definiteness and precision which are requisite in the estimate of a philosophical or logical system. He does not, for instance, bring out with sufficient emphasis the fact that Bacon was what in our own days we should call, not a philosopher, but a logician. His mission, as Bacon himself conceived it, was to bring about a thorough reform in the method of science, and through this new method to reconstitute, or, rather, to enable others to reconstitute, from their very foundations, the whole circle of the sciences—moral, mental, and political, as well as what are more strictly called natural. The inductive method was not conceived of by Bacon as antagonistic to the deductive method, but as its necessary antecedent and complement. Nor did he regard himself, nor would it be right to regard him, as the inventor of the inductive method, any more than Aristotle regarded himself, or it would be right to regard him, as the inventor of the deductive method. What both philosophers alike did, was to analyze, classify, and discriminate, with a view to distinguish between correct and incorrect reasoning, the methods of natural logic already in use. Only, while Aristotle performed this work effectively, and, considering the time at which he taught, with marvellous elaboration, for the syllogistic logic, he did little more than point out the existence and necessity of induction. This want of rules and of a sufficient analysis of the inductive side of reasoning easily accounts for the utterly unscientific character of the inductions with which men ordinarily satisfied themselves throughout the Classical and Middle Ages. What really constituted the most distinctive feature in Bacon's conception of a reformed logic was the profound idea that induction, instead of being the loose, vague, and uncertain process which was then in vogue, admitted of being presented with the force of demonstration, and thereby, if the facts on which it was founded were true, of supplying as firm a basis for the premises, as the premises, if they were true, supplied for the conclusion of the syllogism. "Inductionem enim censemus eam esse demonstrandi formam, quæ sensum tuetur et naturam premit et operibus imminet ac fere immiscetur" ("Distributio Operis"). "Verum ad hujus inductionis, sive demonstrationis, instructionem bonam et legitimam quamplurima adhibenda sunt, quæ adhuc nullius mortalium cogitationem subiere; adeo ut in ea major sit consumenda opera, quam adhuc consumpta est in syllogismo" ("Novum Organum," Book I. Aph. 105). Thus it is hardly an exaggeration to say that inductive logic—that is, the systematic analysis and arrangement of inductive evidence, as distinct from the natural induction which all men practise—was almost as much the creation of Bacon as deductive logic was that of Aristotle. Dean Church rightly calls attention to the wide interval which separates Bacon's "Tables of Instances" from the experimental methods of Mr. Mill; but the latter are, after all, only a corrected version of the former, and, historically, were derived from them through the medium of Sir John Herschel's discourse on "The Study of Natural Philosophy." Moreover, it is remarkable that the two divisions of the "Instantiæ Solitariæ," described in "Nov. Org.," Book II., Aph. 22, correspond respectively with Mill's "Methods of Agreement and Difference," and that the very words "method of agreement" and "method of difference" all but occur in the text. For these and many similar reasons, we certainly cannot

accept the verdict of Dean Church, that "the course which he marked out so laboriously and so ingeniously for induction to follow was one which was found to be impracticable, and as barren of results as those deductive philosophies on which he lavished his scorn." This remark may be approximately true of the method of rejections or exclusions, which proceeds on the false assumption that the whole complex system of the material universe may be resolved into a small and definite number of "simple natures," just as the numerous words which constitute a language may all be resolved into the few and assignable letters of an alphabet; but it is most emphatically not true of the methods which are subsidiary to the method of exclusions, such as the "Tables" and "Prerogatives of Instances." The subsidiary methods have, happily, a value of their own quite independently of the main object which they were supposed to subserve. Nor, as it seems to us, can it be doubted that these methods have been actually fertile in the progress of scientific discovery. Not, perhaps, that the greatest discoverers have often consciously, deliberately, and designedly set to work to employ them; but methods and principles of this kind, when once enunciated and realized, are, as it were, "in the air," and their influence is often no less potent because it is one of which men are only dimly conscious.

The process of fault-finding, especially as applied to a book which we have read with interest and pleasure, is not one which we would gladly prolong; but, to prevent a very grave misconception of Bacon's philosophical position, we feel it incumbent on us to point out a serious error into which Dean Church has been led by too implicit confidence in the authority of Mr. Ellis. "Bacon's conception of philosophy," we are told, "was so narrow as to exclude one of its greatest domains; for, says Mr. Ellis, 'it cannot be denied that to Bacon all sound philosophy seemed to be included in what we now call the natural sciences.'" By "sound philosophy" is meant, it may be presumed, philosophy based on experience, and arrived at by the inductive method. In "Nov. Org.," Book I., Aph. 127, we have the question as to the range of the sciences to which the new method is applicable definitely propounded and definitely answered. "Etiam dubitabit quispiam potius quam objiciet, utrum nos de Naturali tantum Philosophia, an etiam de scientiis reliquis, Logicis, Ethicis, Politicis, secundum viam nostram perficiendis loquamur. At nos certe de universis hæc quæ dicta sunt intelligimus: atque quemadmodum vulgaris logica, quæ regit res per syllogismum, non tantum ad naturales, sed ad omnes scientias pertinet; ita et nostra, quæ procedit per inductionem, omnia complectitur." There are many other passages in the "Novum Organum," the "De Augmentis," and elsewhere, to the same effect. Indeed, it appears to us unquestionable that Bacon, while he regarded his method as primarily, and, perhaps, most easily, applicable to the natural sciences, contemplated its ultimate extension to all branches of knowledge alike. The few passages which seem to point in the opposite direction are, doubtless, ironical, and refer, not to science, or knowledge in the true sense, at all, but to rhetoric and disputation.

The last chapter of the book is on Bacon as a writer. Here the author is thoroughly at home, and the striking and suggestive remarks which he makes on this topic only cause us to regret that there are not more of them. Take, for instance, the following just and forcible sentences on Bacon's English composition:—"His manner of writing depends, not on a style, or a studied or acquired habit, but on the nature of the task which he has in hand. Everywhere his matter is close to his words, and governs, dominates, informs his words. No one in England before had so much as he had the power to say what he wanted to say, and exactly as he wanted to say it. No one was so little at the mercy of conventional language or customary rhetoric, except when he persuaded himself that he had to submit to those necessities of flattery, which cost him at last so dear."

T. FOWLER.

KARYOKINESIS.

Ueber Kern- und Zelltheilung im Pflanzenreiche, nebst einem Anhang über Befruchtung. Von E. Strasburger, o. ö. Professor der Botanik an der Universität Bonn. Mit drei lithographischen Tafeln. (Jena: Gustav Fischer, 1888).

PROF. STRASBURGER intends this volume to constitute only the first of a new series of contributions to our knowledge of vegetable histology. In these 258 pages the phenomena attending indirect or mitotic nuclear division, and the earlier stages in the formation of the cell-membrane, are entered on in detail. During the four years which have elapsed since the appearance of the author's last contribution to this subject ("Die Controversen," &c.) numerous memoirs have been published relating to the nucleus and its division. Prof. Strasburger not only contributes a vast number of new facts, but also reviews the whole nuclear question in a masterly fashion, so that the work may be regarded as a critical text-book of our present knowledge of the subject. It will be seen from what follows, that, although many of his former conceptions have been confirmed, there still remain points which are doubtful, and some positions formerly held by him which are now abandoned.

The book commences with a long account of a renewed investigation of the nuclear processes in *Spirogyra*, the research in question being carried out on a new species, *S. polytæniata*, which presented many facilities for the purpose. This account is full of interest, but difficult to do justice to here, without figures. During the early stages of division, whilst the nuclear fibrils are making their way to the equatorial plane and the nucleolus undergoing solution, but before the breaking down of the nuclear wall, a mass of cytoplasm is formed on the two faces of the nucleus which are directed towards the end-walls of the cell, and in these a striation becomes apparent, representing the commencement of the spindle. Soon the nuclear wall becomes indistinct where the striation abuts upon it, and spindle-filaments appear within the nucleus; these form an undoubted continuation of those which appeared outside. There would appear to be no ground for supposing these later-appearing filaments of the spindle to have an origin differing from those which appeared first of all, but rather they are their direct continuation, and due to the intrusion of cytoplasm into the nucleus.

It is the view of the author that in this, and in all other cases, the spindle has a cytoplasmic origin, and this is in agreement with his former tenets. The occurrence of an almost complete spindle within the nucleus in *S. nitida*, before the break-down of the nuclear wall, is shown to be very probably due to the entrance of cytoplasm through a number of small pores; since the wall, as seen in its polar aspect, shows a sieve-like dotting from which a perforation is inferred. The event in *S. nitida* differs thus only in degree from that in *S. polytæniata*. Throughout the whole process of division the nucleus is enclosed in a cytoplasmic mantle or pocket, which is suspended freely in the cell-lumen by delicate protoplasmic filaments. As the two halves of the nuclear plate separate, a cavity is formed—at first traversed by the uniting-filaments (*Verbindungsfäden*)—which increases in size by a continuous absorption of fluid through its wall, and is regarded by the author as a mechanism by means of which the two young daughter-nuclei are driven apart. For further details the reader is referred to the original, and to the figures on Plate I.

We pass on now to the typical events in the nucleus of higher plants. In the "resting nucleus" (used in the conventional sense only, in contradistinction to "dividing nucleus") there exists a definite, limiting layer, the nuclear wall, which consists undoubtedly of cytoplasm. The nuclear reticulum consists of a number of fibrils so interwoven that it is difficult to say whether they have fused into a genuine network, or really retain their individuality, and are simply in contact with one another. The author is distinctly of the opinion that the latter is the case, and that after a division the nuclear segments or fibrils remain separate, never losing their individuality. The probability of this view is greatly increased by the constancy in number of these fibrils as shown especially by investigation of division-stages of pollen-mother-cells in Liliaceæ. The number of segments is very commonly sixteen, the relatively high number obtaining in developing endosperm-cells being due to the fusion of the two nuclei, which gave rise to the secondary nucleus of the embryo-sac. Thus, in the endosperm of *Lilium Martagon*, Guignard found twenty-four or more segments, though but twelve or sixteen in the daughter-nuclei of the primary embryo-sac nucleus. Although information on this head is limited, it has been shown that where a sudden considerable increase in the number of segments has been observed there has been a previous fusion of nuclei, as often occurs in the young endosperm cells of *Corydalis pallida*. A slight increase, however, may often be due to better nutrition. Absolute constancy in number of segments is only met with in the case of generative nuclei, so far as investigation as yet shows.

Lying between the fibrils, and adhering to them, are one or two nucleoli. Bathing the fibrils and nucleoli is the nuclear sap, which at this period is not stainable. The fibrils consist of a non-staining substance, the nucleo-hyaloplasm, in which are embedded a number of irregular, strongly-staining granules, the chromatin-granules. The author prefers to speak of the nucleo-hyaloplasm, with Schwarz, as Linin. The name nucleo-microsomata for these chromatin-granules is here definitely abandoned, as there exists no true parallel between them and the microsomata of the cell-protoplasm. In the resting vegetable nucleus Prof. Strasburger finds no trace of the faintly staining "bridges" described by Flemming and Rabl as uniting the nuclear fibrils in the Salamander. When division is about to take place a shortening of the nuclear fibrils occurs, accompanied by a definite increase in thickness. The chromatin-granules at the same time run together into plates, separated from one another by linin (nucleo-hyaloplasm). These plates of chromatin grow at the expense of the linin. The fact, that this takes place in *Fritillaria* before the disappearance of the nucleoli, precludes the possibility that the chromatin grows at the expense of the nucleoli. It is probable that this equal distribution of substance in the nuclear fibrils insures completely similar products when the subsequent longitudinal fission takes place.

The dividing nucleus now enters on the "skein-phase," and the arrangement of the fibrils may be seen with distinctness. At this period in many nuclei—as, for instance, in the young endosperm of *Fritillaria imperialis*—the separate segments lie, for the most part, parallel, each segment being loop-shaped with legs of approximately equal length. The points of bending converge on one side of the nucleus—its polar side; the free ends terminate towards the antipolar side. The polar side of the nucleus would appear to bear a definite relation to the point of convergence of the daughter-segments of the previous division, and generally the line joining the polar and antipolar sides will cut the nuclear plate at right angles. It is during this stage that the nucleoli disappear. Hitherto they have occupied an eccentric position, lying it would seem towards the polar side—this being the region least occupied by nuclear fibrils. As the nucleoli disappear, the nuclear sap becomes capable of staining, and the inference is that this is due to the presence in the sap of the dissolved nucleolar matter. The author regards it as improbable that the nucleoli go to nourish the nuclear fibrils. The structures to which Prof. Strasburger formerly gave the name of paranucleoli, he now acknowledges to be simply nucleoli late in disappearing, so that all the theoretical deductions based on the appearance of those structures, by the author and others, fall to the ground.

The nuclear membrane now breaks down, the segments place themselves in the equatorial plane forming the nuclear plate, and the spindle makes its appearance. The author at great length details the evidence in favour of the cytoplasmic origin of this structure, but into this we cannot enter here. The poles of the spindle are determined before the solution of the nuclear wall, but they do not influence the nuclear fibrils in their transpositions before the breaking down of the wall. It must not, however, be concluded from this that the changes within the nucleus are entirely independent of the cytoplasm until the end of the skein-phase. The division of nuclei within the embryo-sac, which is almost simultaneous, would negative such a view. The cytoplasm does not exert any directive influence on the fibrils until the breaking down of the wall. The fibrils now depend for support on the filaments of the spindle, and these are generally equal in number to the segments—one to each, or, after the fission of the segments, one to each segment-pair. The completion of transposition and the separation of the segment halves are carried out under the influence of the spindle, a certain directive action of the poles being exerted; the segments

themselves are not passive, but possess a movement of their own held in control by polar influences.

For details relating to the complicated transpositions of the fibrils, their longitudinal fission, and subsequent separation, the reader is referred to Chapters VII. and VIII.

The two groups of daughter-segments separate, the segments travelling along the filaments of the spindle. Prof. Strasburger considers the hypothesis that the travelling is due to streaming of the protoplasm improbable, as this would involve the running of two opposite currents in each spindle-filament. Further, no streaming, either in or on the filaments, has been observed in the living, dividing nucleus. The fibrils themselves probably possess a capacity for movement, using the spindle-filaments only as supports. What stimulus the segments may receive from the poles is difficult to say—perhaps one similar to the chemical stimulus which causes directive movements in Bacteria, Plasmodia, &c. As the groups of daughter-segments move apart, the spindle-filaments, which are continuous from pole to pole, on the view of Strasburger, are seen stretching over the interval. These constitute the primary uniting-filaments, and there is some diversity of opinion as to their origin. Soon, more cytoplasm makes its way into the equatorial region, and a great increase in the number of the filaments takes place. These additional, or secondary, uniting-filaments are formed from this intrusive cytoplasm, and not by multiplication of the primary ones.

The nuclear sap, and dissolved nucleoli, lie between the uniting-filaments; and even after the collection together of the daughter-segments to form the daughter-nuclei, there remains a considerable residuum of stainable nuclear sap which makes its way to the equatorial region of the spindle and appears to play a most important part.

At this moment a small bead-like thickening appears on each uniting-filament—both primary and secondary—in the equatorial plane, and it is by the fusion of all these thickenings that the cell-plate or primitive cell-membrane arises. Throughout, Prof. Strasburger speaks of this occurrence with the greatest confidence, in opposition to the view of Zacharias and Flemming. These swellings which constitute the elements of the cell-plate, are spoken of as *dermatosomata*, although the same word has been recently used by Weisner with another significance. Fresh uniting-filaments continue to arise at the periphery of the young cell-plate, each bearing a local swelling (*dermatosome*), and in this manner the cell-plate is completed.

In cases of free-cell-formation a temporary cell-plate appears, but is not completed, and subsequently disappears.

It is at this stage that Prof. Strasburger attaches great importance to the part played by the stainable nuclear sap. As above mentioned, a portion of this has collected in the equatorial region, and everywhere bathes the dermatosomata. At the same time a demonstrable change takes place in their constitution; the dermatosomata offer a greater resistance to such a reagent as *eau-de-javelle*, and show an increased refrangibility. In other words, the cell-plate has been converted into the first layer of the new cell-wall. These changes are traced to the stainable nuclear sap which is present. A direct proof of this is very difficult, but the hypothesis is a most taking one; accounting, as it does, for a number of phenomena which have long baffled explanation; and it possesses also the added charm of simplicity. The chief objection to this theory is the difficulty in imagining the continued presence of the nuclear sap in regions where the cell-wall is undergoing a thickening; for will not the process of conversion throughout be identical, whether it be primary or secondary layers that are being formed? Perhaps in a future contribution this will be explained; but for the present this hypothesis must remain a hypothesis, and will—be it hoped—stimulate investigation into a matter on which more light is much needed.

As the daughter-nuclei are formed, a considerable portion of the nuclear sap is taken in, and lies, in the first instance, on the antipolar side of the nucleus, where in some few cases (embryo-sac of *Hyacinthus orientalis*) the nucleoli appear, and the sap loses its staining property. In the majority of cases the nucleoli, as stated above, appear towards the polar side.

In a chapter dealing with the function of the nucleus the view is put forward that it has the same relation to starch-formation as very probably exists between it and the development of cell-membrane. The fact that Klebs found, in plasmolyzed filaments of *Spirogyra*, a formation of starch occurring in masses of protoplasm destitute of a nucleus, Prof. Strasburger considers due to the fact that the pyrenoids physiologically replace the nucleus in this connection. This finds support in the fact that in plasmolyzed cells of *Funaria* the chlorophyll-corpuscles in fragments of cells without a nucleus are unable to form starch.

The book concludes with a chapter on fertilization, in which controversial matters are discussed. The author adheres to his former view that in higher plants fertilization consists of the fusion of an equal number of nuclear segments, as also of the nuclear sap of the two conjugating nuclei. He finds no evidence for the view of Zacharias that the male and female nuclei differ essentially in any way.

It is impossible here to do full justice to this remarkable book, and there are many matters traversed in it to which we have not even alluded. Thus, the detailed comparison drawn between the vegetable and the animal nucleus. It seems that the differences in this respect occurring between lower and higher plants find their parallel in comparable differences in more lowly and more highly organized animals.

A careful perusal of the original will repay the labour so expended, and the style and arrangement of the subject-matter are such as to make us unwillingly lay it aside. Perhaps some idea of the pace at which knowledge in minute cell-histology has progressed may be obtained when we remember that only seventeen years ago a botanist, who now stands in the foremost rank of plant-histologists, was prepared to maintain as a thesis, and to dispute with all comers, "that in the vegetable kingdom nuclear division does not occur." F. W. O.

OUR BOOK SHELF.

Chambers's Encyclopædia; a Dictionary of Useful Knowledge. New Edition. Vol. II. (London and Edinburgh: W. and R. Chambers, 1888.)

THE second volume of the new edition of "Chambers's Encyclopædia," which extends from "Beaugency" to "Cataract," maintains the high standard set by the

first volume. The names of the writers are a guarantee for the excellence of the work; and, where not wholly rewritten, the articles have been revised and brought up to date. Mr. J. Arthur Thomson is responsible for the zoological articles, which in this volume are Bee (in which Sir John Lubbock has assisted), Bird, and Butterfly; Mr. Patrick Geddes is the writer of the articles on Biology, Botany, Bud; Dr. W. Inglis Clark writes on Carbon, Prof. James Geikie on the Carboniferous System, Prof. Wm. Thomson on Capillarity, and Mr. A. Fraser on the Calculus. The articles on engineering and architectural subjects are contributed by Messrs. D. and T. Stevenson, D. K. Clark, and David MacGibbon. In geography, Sir Charles Warren writes on Bechuanaland, Sir Charles Bernard on Burmah, Mr. S. Lane-Poole on Cairo, Mr. Macdonald, of the *Englishman*, on Calcutta, Prince Kropotkine on the Caspian Sea. Amongst other geographical articles are those on Belfast by Mr. T. Macknight, Birmingham by Mr. S. Barnes, Bolivia by Mr. W. Dundus Walker, Bristol by the Rev. W. Hunt, Brittany by Mr. Thos. Davidson, Bulgaria by Mr. A. Silva White, Cambridge by Mr. G. H. Smith, Canada by Mr. J. G. Colmer, C.M.G., Cape Colony by the Rev. J. Mackenzie, and Cashmere by Major Holdich. Five excellent maps accompany this volume—namely, (1) Belgium; (2) Burmah, Siam, and Assam; (3) Canada, Eastern Provinces; (4) Canada, Dominion of; (5) Cape Colony and South Africa. The less important articles are also very satisfactory. For those who desire further information on the various subjects a list of authorities is given. Many of the articles are models of compression. The article on Carlyle is an instance of this. Here the large and growing literature relating to Carlyle, published since his death, is compressed into the space available in a manner that is little short of amazing. In this and one or two articles which we have noticed, the very difficult art of saying much in a little space, of reducing volumes to paragraphs, and even to lines, is exhibited in a high degree of excellence.

Star Atlas, containing Maps of all Stars from 1 *to* 6·5 *Magnitude between the North Pole and* 34° *South Declination, and all Nebulæ and Star-clusters in the same Region, which are visible in Telescopes of moderate powers, with Explanatory Text.* By Dr. Hermann J. Klein. Translated and adapted for English readers by Edmund M'Clure, M.A., M.R.I.A. (London: Society for Promoting Christian Knowledge, 1888.)

THIS is a most important addition to the stock-in trade of the amateur astronomer. The eighteen maps, printed by Funke, of Leipzig, are as clear as they can be, the letters and constellation boundaries being given in red ink.

There are some useful tables given in the introduction, and these are followed by a catalogue of the most interesting objects, which seem to have been very carefully chosen by a practical astronomer, and the editor has done his best to bring the accompanying notes down to the latest date.

In addition to the maps, some excellent illustrations of clusters and nebulæ are given, and no pains have been spared to give as much useful and trustworthy information as possible.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Alpine Haze.

FOR some years past, occurrences in the Alpine atmosphere have frequently reminded me of effects produced in the "experimental tubes" with which I worked some years ago. When the experimental tube was already occupied by a fine "actinic cloud," it was a common experiment to precipitate within the tube an ordinary cloud by dilatation. The melting away of this latter, and the reappearance of the finer and more persistent cloud, which it had for a short time disguised, were curious and instructive effects.

In the valleys of the Alps floats, not unfrequently, a fine haze, much resembling the actinic clouds. This year the haze was more than usually prevalent, being sometimes very curiously distributed. It frequently filled the great Rhone Basin, below Alp Lusgen. Amid the haze, patches of true cloud would appear, extending till they became continuous, and filled the basin. A floor of cloud, usually shining white, would then spread below us. Under a strong sun, the cloud would disappear, leaving the more permanent haze behind. The haze could not have been aqueous. I have seen the dense true cloud disappear early in the morning, and the haze continue through a fervid summer day, until the moon came out at night to illuminate it. The distribution of the haze this year, and the consequent precipitation were often remarkable. Looked at from our eminence, the haze would be seen filling the lower valley, but divided above into long horizontal striæ, which were obviously the edges of haze-layers, foreshortened from our point of view. Mr. Stirling's beautiful observations were frequently brought to mind by the obvious tendency of the aqueous clouds to form in, and to follow, the haze. A highly picturesque distribution of the clouds was often thus produced. What the haze *is* I do not, for a certainty, know; but that it is not water is proved by its persistence in presence of a powerful sun, and above the heated earth-surface. The late Prof. De la Rive would probably have referred it to floating germs (see letter to myself "On the Organic Dust of the Air," *Phil. Mag.*, 1870, vol. xxxix. p. 229). The prevalence of autumn pollen in the air may, perhaps, account for the singularly striking cloud effects invariably observed at Alp Lusgen, at the end of September and the beginning of October.

JOHN TYNDALL.

Hind Head, Haslemere, October 30.

Prophetic Germs.

MY desire in this controversy has been to bring the Duke of Argyll's theory to the test of fact. But I cannot obtain from him any statement of fact which tends to support his belief in prophetic germs. He cites the well-known observation that in the growth of the individual from the egg, organs pass through rudimentary stages, during which they are not used He then says: "On the Darwinian hypothesis this fact applies equally to the birth of species." Does it? It is not worth while posing opinion against opinion. Let us have some facts. Can the Duke of Argyll, or anyone else, adduce an observation of fact which necessarily leads to the conclusion that a given organ in a given animal or plant has passed through rudimentary stages in ancestral evolution in which that organ's rudiment had no use?

I am inclined to think that there are some cases which might appear to be of this nature, but are to be explained as due to "concomitant variation" or "correlation of growth" in a complex highly-elaborated organism, one part developing, though without use, as the necessary mechanical or structural condition of the development of another part which *has* use.

Such cases will not serve the purpose of establishing a general law. Will the Duke undertake to tell us what were the rudimentary stages of the limbs of Vertebrata in which actual use was impossible? Will he give a similar history of the vertebral column, or of the brain and spinal cord, or of the eye? In short, are there any facts in support of the theory of prophetic germs? Unless such facts are cited, your readers will conclude that the theory of prophetic germs is devoid of basis.

E. RAY LANKESTER.

45 Grove End Road, N.W., October 26.

Mr. Romanes's Paradox.

I SHOULD be sorry to have misrepresented the views of Mr. Romanes, especially on so formal an occasion as a Presidential address at a meeting of the British Association. But, if I have done so, I must plead in extenuation that I know of no recent writer whose papers I find so difficult to thoroughly comprehend. With an appearance of lucidity there seems to me to be often an

underlying obscurity of ideas by which I find myself as often completely befogged.

It appears to me that it is sometimes overlooked that what is usually called the "Darwinian theory" is set out in a book which bears as its title the words, not, as they are usually quoted, "The Origin of Species," but "The Origin of Species by Natural Selection." These words I regard as a proposition of which the book itself affords what is intended to be the proof. It seemed to me that Mr. Romanes intended to distinctly traverse this proposition, and, this being so, the careful consideration of his views became a matter of very great importance. Mr. Romanes now denies that he intended anything of the kind. But the denial comes rather late in the day, because the impression which I received from his paper at the Linnean Society was certainly shared at the time by others. For example, though it is unusual for a purely scientific paper to receive an extended notice in large print in the *Times*, Mr. Romanes was so favoured, and here is what the *Times* (August 16, 1886) says on one of the points on which Mr. Romanes complains that I have misrepresented him :—

"The position which Mr. Romanes takes up is the result of his perception, shared by many evolutionists, that the theory of natural selection is not really a theory of the origin of species, but rather a theory of the origin and cumulative development of adaptations." Now, I suppose Mr. Romanes would call this an "absurd misrepresentation." If so, it is singular that, as far as I remember, he took no steps to correct the statement of his views to which the *Times* gave its wide circulation.

But is it a misrepresentation? It is not, I think, difficult to cite a good deal of evidence that it is not. Anyone who will take the trouble to refer to the Journal of the Linnean Society, Zoology, vol. xix. p. 345, will find printed in capital letters across a page of Mr. Romanes's paper, "Natural Selection not a Theory of the Origin of Species." Now, everybody knows that the idea of the evolution of organic nature existed in men's minds long before Mr. Darwin. He did not originate it; what he did originate was the theory that "natural selection" is the mechanical means by which that evolution has been brought about. Mr. Romanes says roundly that it is not, or words have ceased to have meaning. Well, coming from "the biological investigator upon whom," the *Times* tells us, "in England, the mantle of Mr. Darwin has most conspicuously descended," I thought that a "startling paradox," and I said so. There was nothing very novel in this: it only put into other words what Mr. Wallace had already said (NATURE, vol. xxxiv. p. 467), when he took exception to Mr. Romanes's "extraordinary statement that, during his whole life, Darwin was mistaken in supposing his theory to be 'a theory of the origin of species,' and that all Darwinians who have believed it to be so have blindly fallen into the same error."

The next point on which Mr. Romanes complains is that I make him say specific differences are not adaptive, while those of genera are. And he calls this an absurd misrepresentation! It is really too comical, because it is the key of his whole strategic position. When Mr. Romanes read his paper at the Linnean Society, he began by saying that he regarded it as the most important work of his life. And the expression would certainly not have been exaggerated if he had succeeded in establishing what he terms (capitals again) the "inutility," *i.e.* non-adaptiveness, "of specific characters." Even Mr. Romanes could not assert that all specific characters are non-adaptive. But he asserts (NATURE, *l.c.*, p. 314) that "a very large proportion, if not the majority, of features which serve to distinguish species from species are features presenting no utilitarian significance." If this could be proved, it would be quite as effective as proving the proposition universally in inflicting a deadly blow on the Darwinian theory, the very essence of which is that specific differences must be advantageous. I agree with Mr. Wallace (NATURE, *l.c.*, p. 467) "that there is no proof worthy of the name that specific characters are frequently useless."

I am of course prepared to admit that, in regard to plants, about which only I feel competent to speak, there are a vast number of specific differences the adaptive significance of which we are either wholly ignorant of, or, at any rate, very imperfectly understand. But Mr. Darwin has himself led the way in a host of discoveries which have shown in innumerable directions, which had never been previously suspected, the adaptive significance of plant structures. We seem to me justified, then, in drawing the conclusion that all specific differences in plants are probably adaptive. This Mr. Romanes calls reasoning in a circle; to me it seems only a reasonable induction, the validity of which is strengthened every day by fresh observation.

As to the distinction which Mr. Romanes draws between specific and generic differences, I only summed up what he repeats again and again. Here is a specimen :—"It is comparatively seldom that we encounter any difficulty in perceiving the utilitarian significance of generic and family distinctions, while we still more rarely encounter any such difficulty in the case of ordinal and class distinctions. Why, then, should we encounter this difficulty in the case of specific distinctions?" In my opinion the actual state of things is exactly the reverse. But, as I discussed this point at some length in my Bath address, I need not touch upon it further.

I do not undertake to follow Mr. Romanes into all his dialectical subtleties. But the position which I understood him to have taken up in his paper was quite intelligible, and was of very great interest to the biologist. I briefly analyze it as follows :—Mr. Darwin explained the origin of species by natural selection; this implies that specific differences are adaptive; but this is not universally the case; it follows, then, that natural selection is not the explanation of the origin of species except when specific differences are adaptive, which, in point of fact, they are not in the majority of cases. It is clear that this shrivels up the part played by natural selection to very small dimensions, and minimises pretty effectively in proportion the position of the Darwinian theory in the field of biological speculation. The force, however, of the whole train of argument obviously depends, as I have remarked before, on the proof which can be given of the proposition that the majority of specific differences are non-adaptive. When we turn to the part of Mr. Romanes's paper dealing with this vital point, we only find some not very convincing assertions—some of which I think are erroneous—and no facts whatever. This is, however, not very surprising. Mr. Romanes is not a practised naturalist. His method is the very inverse of that of Mr. Darwin. We know that the latter for more than twenty years patiently accumulated facts, and then only reluctantly gave his conclusions to the world. Mr. Romanes, on the other hand, frames a theory which looks pretty enough on paper, and then, but not till then, looks about for facts to support it.

In my view, one is not called upon to give much attention at present to physiological selection. Still, a word or two may be devoted to it. The *Times* took an exception to the phrase of which I am surprised that Mr. Romanes has taken so far no notice. It says :—"How his theory can properly be termed one of selection he fails to make clear. If correct, it is a law or principle of operation rather than a process of selection." In point of fact, what Mr. Romanes calls physiological selection may be more accurately described as reproductive isolation. He supposes that individuals of a particular species arise which from some cause or other are incapable of breeding with other conspecific individuals. They are therefore in one aspect isolated, as if they were on an oceanic island. This being so, any casual variations which they exhibit will be perpetuated, he thinks, whether adaptive or not. And in this way he also thinks that species distinguished by non-adaptive characters have arisen. The idea is interesting, and Mr. Romanes believes that Mr. Darwin would have welcomed it. We know, however, that it occurred twelve years earlier to Mr. Belt, that Mr. Darwin was acquainted with it, and that "he did not regard it with any great favour." I myself have carefully considered it in connection with a variety of facts, and I have arrived at the conclusion that it is not a principle of very much value. It would take too long to set out the grounds for that conclusion here. But I may point out that such an isolated race would get no immunity from the general struggle for existence, while it would lose all the advantages to be obtained from free intercrossing. I am disposed to agree, then, with Mr. Wallace that, far from such races being "unable to escape the preserving agency of physiological selection," they would be very short-lived. Before leaving the subject, I cannot but remark on Mr. Romanes's singular choice of an alternative name for physiological selection—the "segregation of the fit." Segregation, I agree, is an improvement; but "fit" lets in the whole train of adaptive ideas, while Mr. Romanes insists that "the variations on the occurrence of which it [physiological selection] depends are variations of an unuseful kind."

One remark, and I shall conclude all that I propose to say about Mr. Romanes and his theory. What I introduced into my Bath address I had had long before in my mind. While I

was writing it, Prof. Huxley's obituary notice of Mr. Darwin came into my hands. I read it with the keenest pleasure, as everyone must; and I pointedly referred to it with a pardonable anxiety that a piece of work perhaps one of the most remarkable that ever came from that admirable literary workshop should attract a wider attention than from its mode of publication it might possibly receive. Personally, with regard to indifferent variations, I am a little disposed to think that Mr. Huxley is inclined to make too great concessions. I quite admit that correlated variation does give rise to a large class of non-significant characters. But I feel more and more that natural selection is a very hard taskmaster, and that it is down very sharply on structural details that cannot give an account of themselves. I doubt if there is much room in Nature for indifferent variations; and even correlated variations must be anchored, as it were, to an adaptive variation which has to bear the brunt of the maintenance of the whole correlated train.

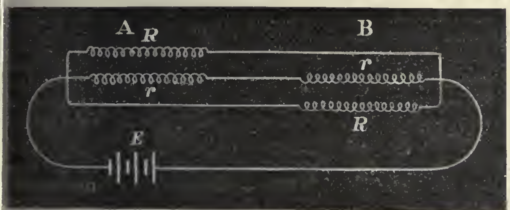
W. T. Thiselton Dyer.

Royal Gardens, Kew, October 26.

Electro-Calorimetry.

In a paper read at the British Association meeting at Bath, Messrs. Stroud and Haldane Gee describe the method used by them for heating the liquids under experiment. Will you allow me to point out that the series arrangement of the coils is electrically in unstable equilibrium, since any difference of temperature between the baths causes less power to be spent in the cooler one, thus tending to increase the difference. With the coils in parallel less power is spent in the hotter bath, but the method is still imperfect from the want of equality of heating at different temperatures.

Coils may, however, be so arranged as to completely overcome these defects in an otherwise very simple and convenient apparatus. In the figure let A and B represent the two baths and the



coils therein, each bath being heated partly by a series coil of r_ω, partly by a parallel coil of R_ω. All four coils should be made of the same metal.

The necessary relation between R and r to secure equal heating may be found by writing $\frac{d \text{ watts in A}}{d\theta} = \frac{d \text{ watts in B}}{d\theta}$, where θ is the difference of temperature between the baths. When worked out this gives $R = 4r$; a result which is obviously true provided the coils have only a small temperature coefficient.

Sydney Evershed.

2 Victoria Mansions, S.W., October 10.

The "Tamarao" from Mindoro (Philippine Islands).

I have only just seen, in Nature of August 16 (p. 363), Dr. Sclater's communication of Prof. Steere's letter concerning the discovery of a new species of *Anoa* (*A. mindorensis*) in the Island of Mindoro. I beg to say that I forwarded a note on this imperfectly-known animal, whose native name is *Tamarao* (not "*Tamaron*," as far as I know), to the Zoological Society of London, and the note was printed in the Proceedings of the Society for 1878, pp. 881–82, under the title, "Letter concerning the supposed existence of the *Anoa* (*Anoa depressicornis*) in the Philippines." Since then, Dr. Hoffmann, formerly Assistant at the Royal Zoological Museum of Dresden, has published the results of his investigations on a skull of the *Tamarao*, which has belonged to the Dresden Museum since 1878, and which was brought by Prof. Semper from his travels in the Philippines (see *Abhandl. und Berichte d. k. Zool. und Anthr. Ethnogr. Museums zu Dresden*, 1886–87, No. 3, p. 26 *et seq.*, Plate 6, *a–f*). He proves, by a comparison of this skull with the skull of *Anoa* from Celebes, and with buffalo skulls from the Philippines and elsewhere, that this *Tamarao* has nothing to do with the genus *Anoa*, but is a true buffalo, viz. either *Bubalus indicus*, Rüt., or an undescribed variety of this species, or, perhaps, a new species of *Bubalus*. Between these alternatives we were unable to decide from the single skull in our hands, which, besides, is not that of a full-grown animal. If Prof. Steere be right in asserting that there exists a true *Anoa* in Mindoro, I can only conclude that the skull brought by Prof. Semper as that of the *Tamarao* of Mindoro, is not the true *Tamarao*.

R. Museum, Dresden, October 17. A. B. Meyer.

Pallas's Sand-Grouse (*Syrrhaptes paradoxus*).

It is obvious that this bird no longer appears to come much, if at all, under observation in Europe, although it was reported from almost every part during the months from April to June (see Meyer and Helm, *Orn. Jahresbericht der Beobachtungstationen im Königreich Sachsen*, iii. p. 117 *et seq*), and even later. I suppose nearly all the specimens have flown into the sea, and been drowned there. As regards its former appearances in Europe, a specimen of *Syrrhaptes paradoxus* is said to have been killed near Grenzdorf, in Silesia, about four years ago; and it is also said to have been observed near Sagan, in Silesia, in the years 1874–78; and in the year 1883 near Münster, in Westphalia. Whether these reports are authentic, I, of course, cannot say, the specimens not being in my hands.

R. Museum, Dresden, October 17. A. B. Meyer.

The Species of Comatulæ.

The writer of the notice of vol. xxvi. of the *Challenger* series, which appeared in Nature of October 11 (p. 561), remarks that the total number of living species of Comatulæ is given on p. 383 as 180, but that from the distribution list itself there would seem to be 188 species, and he adds that "possibly the seven additional species of Antedon and the one species of Actinometra named but not described may account for this discrepancy." If he will look at the list again he will find that though it contains the names of 8 MS. species, three of them belong to Actinometra and only five to Antedon. These, however, do not account for the apparent discrepancy, which is due to the fact that eight species are dimorphic, so that their names appear twice over, as is fully explained in the systematic tables on pp. 54, 58.

It will, of course, be understood that these lists only contain the names of such species as have yet been baptized, some few having received names before they could be described, on account of their serving as hosts to Myzostomida, which have been reported on by Prof. von Graff. But some time must unfortunately yet elapse before it becomes possible to make out a complete systematic and distribution list of all the Comatula-species which are still awaiting description in various Continental Museums. Some very interesting forms were obtained by the German ship *Gazelle* and by the Italian cruiser *Vettor Pisani*. Prof. Semper's Philippine collection, which contains several unusually fine individuals, is as yet undescribed, and I know of many other new types from various localities. At present, however, the fine collections made by the *Blake* in the Caribbean Sea during the years 1877–79 are occupying most of my little working time, and they well repay investigation.

Eton College, October 26. P. Herbert Carpenter.

Voracity of the Haddock.

A small haddock (*Gadus æglefinus*), alive when purchased on the fish quay this morning, was so much distended that curiosity prompted an investigation of the cause. In the stomach were found fourteen young whiting (*G. merlangus*) from 4 to 5 inches long, and a small crab (*Carcinus mænas*), with hard carapace, about 1 inch in diameter, all quite fresh, and digestion barely commencing. The haddock was 17 inches long, and weighed, when gutted, 26 ounces. The weight of the young fry and crab was 6½ ounces, or almost one quarter of the weight of the fish. Doubtless this record is often beaten in the deep, though the evidence of so healthy an appetite among fishes is not often so apparent. Chas. O. Trechmann.

Hartlepool, October 30.

The Queen's Jubilee Prize Essay of the Royal Botanic Society of London.

In your issue of October 18 appears (p. 594) a review of the essay for which I was awarded the medal of the Royal Botanic Society, in which the writer makes a great point of my omitting all reference to drugs. He does not state, for the information of your readers, that the prize was offered for the best (not necessarily complete) essay on the "Vegetable Substances introduced into Britain for use in the Arts, Manufactures, Food, and Domestic Economy during the Reign of Her Majesty Queen Victoria." It is not necessary that one should be either "a member of the medical profession" or have "a wholesome dread of drugs" to know that drugs used as medicines could not with any fitness be introduced into this essay; indeed, inquiry from the Secretary elicited the fact that they had been purposely excluded.

Had your reviewer read the essay with any care, he would have observed that I quote Dr. Forbes Watson to the effect that China grass and rhea fibre are products of the *same plant*, but prepared in different ways; while an unprejudiced reviewer would have mentioned that the quotation having reference to *Phormium tenax* is preceded in the essay by the words, "In one of the authorities consulted it is stated that New Zealand flax . . . was introduced into England about 1840; but the author has found a reference to an unsatisfactory attempt to weave it at Knaresborough at a much earlier period than this, and that it had been experimented upon in the Portsmouth Dockyard about 1819, the ropes made from it being satisfactory."

It was evident that the judges considered that "gun-cotton and its derivatives" *are* "direct products of the vegetable kingdom," or they would not have printed this chapter of the essay.

The limited time allowed for the preparation of the essay (about four months), and the inability of the author to avail himself of any collection of economic botany and of many of the most recent books on the subject, naturally led to many deficiencies in the list of substances mentioned, and of this no one was more conscious than the author himself; and all he claims for his essay is that, in the opinion of the judges (one of whom was Prof. Bentley), it was the best of the half-dozen sent in in competition.

John W. Ellis.

3 Brougham Terrace, Liverpool, October 23.

I have but few remarks to make in answer to Mr. Ellis's letter. First, I cannot follow his reasoning that completeness should not in some measure count as a test of quality, nor can I see anything in the preamble of the offer of the prize to exclude drugs. Mr. Ellis is justified, however, in having done so by receiving direct information from the Secretary to that effect.

On the subject of China grass and rhea, the author, in his essay, distinguishes them under separate heads, describing the first rightly as the produce of *Bœhmeria nivea*, and the second as "the produce of the East Indian *Bœhmeria* (*Urtica*) *tenacissima*, a congener of the species producing China grass." It is after this authoritative statement that he refers to Dr. Forbes Watson's opinion.

Regarding New Zealand flax (*Phormium tenax*), Mr. Ellis, in his essay, follows up the quotation given in his letter by the following paragraph: "Not having been introduced during the period to which this essay refers, any further mention of this interesting fibre—for which it has frequently been attempted to find a place in the British market—is unnecessary;" thus justifying my remarks on this head.

I leave it to anyone who has read Mr. Ellis's chapter on "Gun-cotton and its Derivatives," to say whether they are direct products of the vegetable kingdom.

The latter part of Mr. Ellis's letter, I think, supports the truth of my review generally.

The Reviewer.

October 27.

MODERN VIEWS OF ELECTRICITY.[1]

Part IV.—Radiation.

XII.

We must now mention one or two phenomena which depend entirely upon a modification of ether by the neighbourhood of matter, and which we have reason to believe would not occur in free ether at all. These are the optical phenomena of Faraday and Kerr, and the electric phenomenon of Hall.

Faraday discovered, long before there was any other connection known between electricity and light, that the plane in which light-vibrations occur could be rotated by transmitting light through certain magnetized substances along the lines of magnetic force. To make this effect easily manifest, one uses plane-polarized light and transmits it through a fair length of magnetized substance, analyzing it after emergence, and showing that, though it remains plane-polarized, the plane has been rotated, possibly through a right angle or more.

Now, in a general way it is easy to imagine that, inasmuch as something of the nature of a rotation is going on in a magnetic field round the lines of force, vibrations travelling into such a field along these lines should be twisted round, corkscrew fashion, and emerge vibrating in a different plane. But when one tries to follow out this process into detail, one finds it not quite so simple a matter. It has no business to be a very simple and obvious consequence of the existence of a magnetic rotation round the rays of light, else would it occur in free space, and in the same direction in all media. But the facts are that in free space—that is, in free ether—it does not occur at all, and the direction of rotation is not the same for all media: substances can, in fact, be divided into two groups, according to the way in which given magnetization shall rotate the plane of polarized light passing through them.

Similarly with the electrostatic optical effect discovered by Dr. Kerr, who showed that plane-polarized light transmitted across the lines of force in an electrostatic field could, in certain media, come out elliptically polarized. Now, inasmuch as an electric field is a region of strain, and strain in transparent bodies is well known to make them slightly doubly refracting and able to turn plane-polarized into elliptically-polarized light, it is very easy to imagine such a result in an electric field to be natural and probable. But the explanation is not so simple as that, else it ought to be a large effect, occurring in all sorts of media in the same direction, and likewise in free space. But the facts are that it does not occur at all in free space, and it occurs in different senses in different substances; so that again they can be grouped into two classes according to the sign of the Kerr effect.

Thus, then, the rotatory effect of a magnetic field upon light, discovered by Faraday, and the doubly refracting effect of an electrostatic field upon light, discovered by Kerr, agree in this: that they are both small or residual effects, depending on the existence of a dense medium, and both varying in sign according to the nature of the medium.

The only substance in which the Faraday effect is large is iron, including with iron the other highly magnetic substances. The discovery of the effect in these bodies was likewise made by Kerr. The difficulty of dealing with them is that they are very opaque, and hence that the merest film of them can be used. The film can be used either by way of transmission or by way of reflection, it matters not which, but reflection is perhaps the more convenient. Light reflected from the pole of a magnet has indeed barely penetrated at all into the substance of the iron before being sent back; still, it has penetrated deep enough to be distinctly rotated by the tremendous magnetic whirl which it finds there. All these highly magnetic substances are metallic conductors, and are therefore very opaque.

Whether there is any real connection between high magnetic susceptibility and conductivity is more than I can say. But it is quite natural, and indeed necessary, that the greatest portion of light should be reflected on entering a highly magnetic medium, because in such a medium the ethereal density, μ, is so great, and hence the

[1] Continued from vol. xxxviii. p 592.

velocity of wave transmission must undergo a sudden and immense decrease—a circumstance always causing a great amount of reflection, just as when sound tries to pass from any one medium to a much denser one.

But the opacity of iron and other magnetic substances may be explained by the mere fact of their conducting power, just like other metals, and no noteworthy effect of their large value of μ need be detectable.

If a non-conducting highly magnetic substance could be found, it would probably reflect a great deal of light at its surface, though it would not dissipate that which entered it. Such a substance would be most interesting to submit to experiment, but perhaps its existence presupposes a combination of impossible properties.

As to the phenomenon detected by Hall, it appears intimately associated with that of Faraday, and it will be most simple to omit all reference to it for the present.

A general idea of what is happening in the Faraday and Kerr phenomena can be given thus. A simple vibration, like a pendulum-swing, or any other oscillation in one plane, can be resolved into two others in an infinite variety of ways; just as one force can be resolved into any number of pairs of equivalent forces. The two most useful modes of analyzing a simple vibration into a pair of constituents are these: (1) two equal components, likewise plane vibrations, each inclined at 45° to the original one, as when P Q is resolved into A B and C D (Fig. 49); and (2) two equal circular or rotatory oscillations in opposite directions,

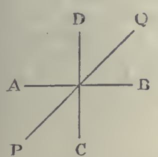


Fig. 49.

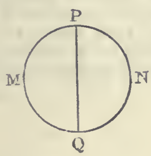


Fig. 50.

as when P Q is resolved into P M Q and P N Q (Fig. 50). The first method of resolution is useful in explaining Kerr's effect, the second in explaining Faraday's.

Of the two component vibrations, A B and C D, into which P Q can be supposed analyzed, let some cause, no matter what, make one gain upon the other, so that in travelling along a line perpendicular to the paper one goes a little the quicker: the effect at once is to change the character of the vibration into which they will recompound. After the gain, they no longer reproduce the original simple vibration P Q: they give rise to elliptic, or it may be to circular, vibrations; this last, if the retardation is equal to a quarter period.

These are matters fully treated in any elementary treatise on polarized light, and they are quite easily illustrated by means of a simple pendulum. One may assume them known.

Similarly with the second system of analyzing the vibration into two opposing circular ones. If the components travel through any interposed medium at the same rate, they will, on emergence, reproduce the original vibration in its original position; but if one travels quicker than the other they recombine into a vibration of the same character as at first, but turned through a certain angle. Thus anything which retards one of the *rectangular* components behind the other changes the character of the vibration from plane into elliptical; while anything which retards one of the *circular* components behind the other leaves the character of the vibration unaltered, but rotates it through a certain angle.

So far one has said nothing but the simplest mechanics. The next point to consider is what determines the rate at which light travels through any substance? This we have discussed at length, and shown to be $\frac{1}{\sqrt{(K\mu)}}$. Anything which increases either the electric or the magnetic permeability of the medium decreases the velocity of light. Now, when a medium is already subject to a violent strain in any one direction it is possibly less susceptible to further strain in that direction and responds less readily. Not necessarily so at all: such an effect would only be produced when the strain was excessive, when the medium was beginning to be overdone, and when its properties began thereby to be slightly modified. There are reasons for believing the specific inductive capacity of most media to be very constant; of some media, perhaps, precisely constant; but if there were any limit beyond which the strain could not pass it is probable that on nearing that limit the specific inductive capacity would be altered—possibly increased, possibly diminished—one could hardly say which. Quincke has investigated this matter, and has shown that the value of K is affected by great electric strain.

Suppose now that a dielectric is subject to a violent electrical stress, so that its properties along the lines of force become slightly different from its properties at right angles to those lines. The value of K will not be quite the same along the lines of strain as across them, and accordingly the rectangular component of a vibration resolved along the lines of force will travel rather quicker or rather slower than the component at right angles, because the velocity of transmission depends upon K as already explained: such a medium at once acquires the necessary doubly-refractive character, and will show Kerr's effect.

Similarly with magnetization. It is well known that for many media μ is not constant. Take iron, for instance. For very small magnetizing forces the susceptibility is moderate, and increases as they increase; at a certain magnetization it reaches a maximum, and then steadily decreases. But not only is it thus very inconstant, its ascending and descending values are not the same. To forces tending to magnetize it more, the susceptibility has one value; to forces tending to demagnetize it, it has another and in general smaller value. This property has been specially studied by Ewing, and has been called by him "hysterĕsis." Slightly susceptible substances cannot be magnetized to anything like the same extent, and hence the property in them has been less noticed, perhaps not noticed at all. Nevertheless it must exist in every substance which exhibits a trace of permanent magnetism, and every substance I have tried appears to show some such trace (see NATURE, vol. xxxiii. p. 484).

An already strongly magnetized medium will be rather differently susceptible to additional magnetizing forces in the same direction than to those in a contrary direction. Nothing more is wanted to explain Faraday's effect. The vibration being resolved into two opposite circular components, one of them must agree in direction with the magnetism already in the medium and try to magnetize it for the instant infinitesimally more; the other component will for the instant infinitesimally tend to demagnetize it. The value of μ offering itself to the two components will be different, hence they will go at different rates, and the plane of vibration will be rotated.

The direction of rotation will depend on whether the value of μ is greater for small relaxations or for small intensifications of magnetizing force; and diamagnetic substances may be expected to be opposite in this respect to paramagnetic ones. Any substance for which μ is absolutely constant, whatever the strength of magnetic polarization to which it is submitted, can hardly be expected to exhibit any hysterĕsis; the ascending and descending curves of magnetization will coincide, being both straight lines, and such a substance will show no Faraday effect. Similarly, any substance for which K is absolutely constant, whatever the electric polarization to

which it is submitted, can show no Kerr's effect. Free space appears to be of this nature; and gases approach it very nearly, but not quite.

In iron, μ is greater for an increasing than for a decreasing force, as is shown by the loops in Ewing's curves; hence the circular component agreeing in direction with the magnetizing current will travel slower than the other component, and hence the rotation in iron will be against the direction of the magnetizing current. The same appears to hold in most paramagnetic substances, and the opposite in most diamagnetic, but the mere fact of paramagnetism or diamagnetism is not sufficient to tell us the sign of the effect in any given substance. We must know the mode in which its magnetic permeability is affected by waxing and by waning magnetization respectively.

Possible Electrical Method of detecting the Faraday Effect.

Thus far we have considered the rotation of electric displacement by a magnetic field as being examined optically, the displacements being those concerned in light, and the rotation being detected by a polarizing analyzer suitable for determining the direction in which the vibrations occur before and after the passage of light through a magnetized substance. This is the only way in which the effect has at present been observed in transparent bodies. But one ought not to be limited to an optical method of detection.

Electrical displacements are easily produced in any insulator, and if it be immersed in a strong magnetic field so that the electric and magnetic lines of force are at right angles to each other, every electric disturbance ought to experience a small rotation. A steady strain will not be affected; it is the variable state only which will experience an effect, but every fresh electric displacement should experience a slight rotatory tendency just like the displacements which occur in light.

Now to rotate a displacement A B into the position A C requires the combination with it of a perpendicular displacement B C (Fig. 51). Hence the effect of the magnetic

FIG. 51.

field upon an electric displacement, A B, may be said to be the generation of a small perpendicular E.M.F., B C, which, compounded with the original one, has the resultant effect A C. It will be only a temporary effect, lasting while the displacement is being produced, and ceasing directly a steady state of strain is set up.

An inverse E.M.F., A D, will be excited by the same magnetic field directly the displacement is reversed.

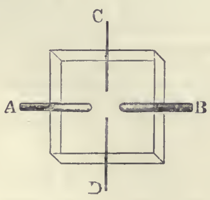


FIG. 52.

And so, if a continual electric oscillation is kept up between A and B in a magnetic field, an accompanying very minute transverse oscillation may be expected, and may be looked for electrically.

Some such arrangement as that here shown (Fig. 52) may be employed. A square of heavy glass, perforated with four holes towards the centre, supplied with electrodes; one pair of electrodes, A, B, to be connected with the poles of some alternating machine, and the other pair, C, D, connected to a telephone or other detector of minute oscillatory disturbance. So soon as a strong steady magnetic field is applied, by placing the glass slab between the poles of a strong magnet, the telephone ought to be slightly affected by the transverse oscillations. This effect has not yet been experimentally observed, but it seems to me a certain consequence of the Faraday rotation of the plane of polarization of light.

Hall Effect.

Although the existence of this transverse E.M.F., excited by a magnetic field in substances undergoing varying electric displacement, has at present only been detected optically in transparent bodies, *i.e.* in insulators, yet in conductors the corresponding effect with a steady current has been distinctly observed electrically. By many persons it had been looked for (by the writer and Prof. Carey Foster, among others, though unfortunately they were not sufficiently prepared for its extreme smallness); by Mr. Hall, at Baltimore, was it first successfully observed.

In conductors it is natural to use a conduction-current instead of a displacement-current. A steady current can be maintained in a square or cross of gold-leaf or other thin sheet of metal between the electrodes A, B, and a minute transverse E.M.F. can be detected, causing a very weak steady current through a galvanometer connected to the terminals C, D, so soon as a strong magnetic field is applied perpendicularly to the plate. Fig. 53 will

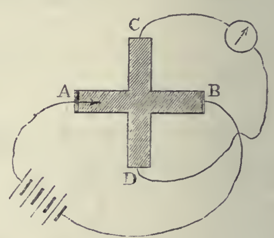


FIG. 53.—The direction of the transverse E.M.F. excited by the earth's vertical magnetic field in this conductor, conveying a current as shown, is C D if it represents gold, D C if it represents iron.

sufficiently indicate the arrangement. The poles of the magnet are one above and one below the paper.

In iron it is easy to see which way the transverse E.M.F. ought to be found. It has been shown that a displacement will be rotated in iron against the magnetizing current; hence, to rotate the displacement A B to A C (Fig. 51), requires in iron a clockwise magnetizing current. Such a current, or, what is the same thing, a south pole below the paper, a north pole above, excites, in the cross of Fig. 53, E.M.F. in the direction D C, and this by Ampère's rule is just the direction in which the conductor itself is urged by the magnetic forces acting on the current-conveying substance. Most diamagnetic substances should exhibit a transverse E.M.F. in the opposite sense. This transverse E.M.F. excited in conductors conveying a current in a magnetic field is the effect known by the name of Hall. It is, as Prof. Rowland and others have pointed out, intimately connected with the Faraday rotation of light.

Unfortunately a pure and simple Hall effect is a difficult thing to observe. Magnetism affects the conductivity of metals in a rather complicated manner, and strain affects their thermo-electric properties. Now, a metal

conveying a current in a magnetic field is certainly more or less strained by mechanical forces, and hence heat will be developed unequally in different parts, by a sort of Peltier effect; and the result of this will be to modify the resistance in patches and so to produce a disturbance of the flow which may easily result partly in a transverse E.M.F. This has been pointed out by Mr. Shelford Bidwell.

The more direct effect of magnetism on conductivity may be negligibly small in many metals, but in bismuth it is certainly large. Both of these spurious effects seem to be large in bismuth, and probably quite mask any true Hall effect there may be in that metal. In all cases the existence of these spurious effects makes it difficult to be sure of the magnitude and sign of the real rotational effect.

But, it may be asked, what right have we to distinguish between a real and a spurious Hall effect? If a transverse E.M.F. can be predicted by reason of known strains and thermo-electric properties, as well as by known rotation of light effects, why should the two things be considered different? Why should they not be different modes of regarding one and the same phenomenon?

In other words, may not the Faraday rotation of light vibration be due to infinitesimal temporary strains and heatings in the medium caused by the fact that minute electric displacements are occurring in a violent magnetic field? This is a question capable of being answered by a quantitative determination of the amounts and direction of the effects to be expected, and a comparison with those actually observed. I do not know of data at present obtained sufficient to enable us to answer it. If the answer should turn out to be in the affirmative, the phenomenon of hysteresis will be at once linked, by an underground path, with those of thermo-electricity and strain.[1]

OLIVER J. LODGE.

(*To be continued.*)

[1] Perhaps I ought to caution students not to accept my connection of Faraday's or Hall's effect with hysteresis as in any way authoritative. Until these views have been criticized it will be wise to place no reliance on them.

IRREGULAR STAR CLUSTERS.

IT is not always easy to distinguish between a casual "sprinkle" of stars and a genuine cluster. The movement-test, by which so many physical have been discriminated from optical double stars, is here inapplicable. The Pleiades are the only considerable group possessing an ascertained common proper motion. All other clusters, debarred as yet from the appeal to this demonstrative argument of their physical nature, have to depend solely upon evidence from probability, with its indefinite variations of conclusiveness according to the circumstances of each particular case. It is, however, in general, amply sufficient. Among five hundred clusters registered as such, there are few indeed as to which there can be any doubt of their forming separate systems; although many real aggregations may exist unrecognized, owing to their loosely scattered character.

Two inferences may be safely derived from the results of recent inquiries into the constitution of the Pleiades. First, that interstitial movements in clusters are likely to be so extremely slow that centuries must elapse before they can become conspicuous; next, that stars showing somewhat marked displacements are presumably mere travellers across, and no genuine components of, the cluster they seem to belong to. An example of this kind of temporary association is almost certainly furnished by an apparent member of a scattered group in Ophiuchus ("Gen. Cat." 1440), the position of which was found, by the comparison of photographs taken by M. von Gothard in 1886 with Vogel's measures of eighteen years previously, to have changed to the extent of 45″, or at the rate of 2½″ annually (*Astr. Nach.*, No. 2777). Its motion, if rectilinear, would carry it from end to end of the collection it is projected upon, in 360 years; and its eventual detachment from it may have become palpably inevitable within ten. The star is of the eleventh magnitude, and is by far the swiftest-moving yet known of so small a size.

Several of the stellar gems surrounding κ Crucis are suspected of considerable mobility. Sir John Herschel, during his visit to the Cape, determined the relative places of 110, all included in an area of about $\frac{1}{48}$ of a square degree ("Cape Observations," p. 17); and the process was, by Mr. H. C. Russell, of Sydney, in 1872, repeated and extended to 130 components (*Monthly Notices*, vol. xxxiii. p. 66). The result was to bring out discrepancies which, if really due to movements of the grouped stars, would be of extreme interest. Herschel's measurements, however, were necessarily too hasty to be minutely reliable; so that changes depending upon their authority need to be confirmed by continuance before they can be unreservedly accepted. The same qualification applies to M. Cruls's discovery of orbital revolution in three double stars within the precincts of the cluster (*Comptes rendus*, t. lxxxix. p. 435).

The stars about κ Crucis are famous for the loveliness of their varied hues. Blue and green, red and sulphur-coloured orbs shine together in a matchless sidereal picture, setting at the same time a problem in sidereal chromatics by no means easy to solve. There is no evidence of change of tint among them since Herschel's time, but there is some, tolerably conclusive, as to change of brightness.

Many irregular clusters seem to be throughout made up of star-streams and reticulations exactly similar to the inflected appendages of globular clusters. A collection (M 24) visible to the naked eye as a dim cloudlet near μ Sagittarii, and regarded by Sir John Herschel as intimately connected with, if not an actual part of, the Milky Way, was named by Father Secchi "Delle Caustiche," from the peculiar arrangement of its stars in rays, arches, caustic curves, and intertwined spirals. Closely adjacent to it, he noted a group of eleventh magnitude stars forming three spokes, as it were, and the nave of a wheel, the axis of which was occupied by a much brighter close pair (*Atti dell' Accad. Pont.*, t. vii. p. 72).

The same kind of radiated structure is apparent in a stellar swarm near the right foot of Castor (M 35), which, with Lassell's 24-inch mirror, showed as so "marvellously striking an object that no one could see it for the first time without an exclamation." A field 19′ in diameter "is perfectly full of brilliant stars, unusually equal in magnitude and distribution over the whole area. Nothing but a sight of the object itself can convey an adequate idea of its exquisite beauty" (*Monthly Notices*, vol. xiv. p. 76). Admiral Smyth described it as "a gorgeous field of stars from the ninth to the sixteenth magnitudes, but with the centre of the mass less rich than the rest. From the small stars being inclined to form curves of three or four, and often with a large one at the root of the curve, it somewhat reminds one of the bursting of a sky-rocket" ("Cycle of Celestial Objects," p. 168, Chambers). A marvellously perfect photograph of this cluster, taken by the MM. Henry, March 10, 1886, exhibits not less than two thousand stars disposed in a roughly-indicated, eight-rayed figure, the branches often connected by drooping chains, and composed in detail of sinuous lines, or "fantastically crossing arcs" of stars (Secchi, *loc. cit.*).

About one hundred connected stars in Ophiuchus ("G. C." 4346) "run in lines and arches" (J. Herschel, Phil. Trans., vol. cxxiii. p. 460); a collection of eleventh magnitude ones in Sagittarius ("G. C." 4323) are scattered along "zigzag lines." The constituents of a large cluster near the Poop of Argo ("G. C." 1649) struck the elder

Herschel by their arrangement "chiefly in rows," by which he gained some insight into the mechanical complexities of such systems. Each row, he observed, while possessing its own centre of attraction, will at the same time attract all the others; nay, "there must be somewhere in all the rows together the seat of a preponderating clustering power which will act upon all the stars in the neighbourhood" (Phil. Trans., vol. civ. p. 269). Speculations, indeed, upon the dynamical relations of "stars in rows," are still premature; nor are they likely, for some time to come, to be accounted as "of the order of the day." But the continual recurrence in the heavens of this mode of stellar aggregation cannot fail to suggest the development of plans of systemic dissolution and recomposition on too grand a scale to be other than vaguely apprehended by us.

The more attentively clusters are studied, the more intricate their construction appears to be. That which challenged Herschel's notice is not singular in exhibiting the federative union of a number of subordinate groups. There is rarely evidence, in the conformation of irregular clusters, of their being governed from a single focus of attraction; there are frequent indications of the simultaneous ascendancy of several. A cluster in Sagittarius ("G. C." 4335) is distinctly bifid. It was remarked by Sir John Herschel at Feldhausen as "divided by a broad, vacant, straight band" ("Cape Observations," p. 116). The fission (as in many nebulæ), no longer in the inchoate state of a "dark lane," is complete. Admiral Smyth's stellar "flight of wild ducks," in Sobieski's Shield (M 11), is perhaps trifid. Father Secchi, at least, perceived in it a three-lobed central vacuity (*Atti dell' Accad. Pont.*, t. vii. p. 75). Sir John Herschel, on the other hand, succeeded by the use of high powers, in breaking up "this glorious object" "into five or six distinct groups with rifts or cracks between them" (Phil. Trans., vol. cxxiii. p. 462). M. Helmert's measures of two hundred of its components referred to a ninth magnitude star conspicuous among them ("Publicationen der Hamburger Sternwarte," No. 1, 1874) will eventually afford the means of detecting their relative displacements. Several of them appear to be variable.

The disruptive tendency indicated by the peculiarities of their distribution is equally marked in "a reticulated mass of small stars" in Cygnus ("G. C." 4511), described at Parsonstown as "a most gorgeous cluster, *full of holes*." The figure published by Lord Rosse shows a winding ribbon of stars inclosing three blank circular spaces, of symmetrically diminishing diameters.

Star-groupings of curiously definite forms are often met with. A triangular swarm ("G. C." 5055) occurs in the tail of Cetus; a rectangular area in Vulpecula ("G. C." 4498) is densely strewn with fine star-dust. Clusters shaped like open fans are tolerably numerous. One situated in Gemini would appear, according to Sir John Herschel, if removed to a sufficient distance, "as a fan-shaped nebula with a bright point like a star at the vertex." Another specimen of an "acutangular" cluster ("G. C." 4902) is bounded by "two principal lines of stars drawing to one" (Phil. Trans., vol. cxxiii. pp. 476, 503). It is 2′ in length, and is to be found in the constellation Cepheus. An oval annulus of stars in Cygnus, 4′ across ("G. C." 4701), centrally surrounds a ruddy ninth magnitude star. A similar elliptical group, with a double substituted for the red star, constitutes a quasi-nucleus for the great cluster in Perseus ("G. C." 512). This superb object, like the still richer group it immediately precedes, has probably galactic affinities. The two together form a telescopic pageant such as, in the wildest flights of imagination, Hipparchus could little have dreamed would one day be unrolled before the eyes of men, out of the "cloudy spot" in the sword-handle of Perseus which he was the first (it is said) to detect. Although the outliers of the two clusters can be brought within the same field of view, they are believed to be really disconnected. The following, and more considerable (known as χ Persei) was micrometrically investigated by Vogel in 1867–70, photographically by O. Lohse in 1884 (*Astr. Nach.*, No. 2650). The result of the comparison of 172 stars was to show their complete immobility in an interval certainly too short for the visible development of such tardy movements as were alone likely to be in progress. A rapid spectroscopic survey executed by Vogel with the Berlin 9-inch refractor, March 30, 1876 ("Der Sternhaufen χ Persei," p. 31), disclosed nothing remarkable in the light of any of the clustered stars, although several of them have been called red, "pale garnet," and even "ruby." Their comparative brilliancy suggests that this magnificent assemblage, as well as its neighbour, may be less exorbitantly distant from the earth than most other objects of its class.

Red and double stars often—we are at a loss to imagine for what reason—seem to dominate in clusters. Compound objects must of course, through the chances of optical juxtaposition, occur most freely where stars are most crowded; yet when they are marked out (as often happens) both by superiority of lustre and by distinction of place, some significance may be attached to their presence. Thus, each of the oblique arms of a "cruciform" group in Auriga ("G. C." 1119), photographed at Paris on January 28, 1887, carries a pair of conjoined stars brighter than the rest (Smyth, "Cycle," p. 140). A "superb cluster" in Monoceros ("G. C." 1637), standing on a background of sky "singularly dotted over with infinitely minute points," has a double star in its most compressed part (J. Herschel, Phil. Trans., vol. cxxiii. p. 386). The central star in Præsepe is double; and there are many examples of more restricted groups gathered round a compound luminary.

Groups apparently ruled by a conspicuous ruddy star are met with in the constellations of the Swan ("G. C." 4676) and Auriga ("G. C." 1067). Another in Cygnus ("G. C." 4701) has already been mentioned.

The nebular affinities of stellar swarms are full of interest, but have as yet been very imperfectly investigated. The discoveries in the Pleiades, however, which may not prove to be the only cluster involved in cosmical fog-wreaths, show what can be done in this direction by the aid of photography. But since nebulæ thus situated are likely to be of the last degree of faintness, the stars probably replacing their original more brilliant knots, their existence can scarcely be made manifest otherwise than by prolonged exposures of plates of the highest sensitiveness. Visual detections of the kind will always be rare. Two rich clusters have nevertheless long been known to include each a nebula of the planetary kind. One in Argo ("G. C." 1801) has a central vacuity conspicuously occupied by a nebulous disk 40″ across; the other (M 46), not far from the head of Canis Major, displays well within its borders a fine annular nebula ("G. C." 1565). It is difficult, if not impossible, to believe either projected casually into such a very remarkable position.

The occurrence of *clusters within clusters* can just as little be set down to the account of chance. In one such instance, a large loose collection in Gemini ("G. C." 1490) involves a neat group of "six or seven stars close together, and well isolated from the rest" (Lord Rosse, Trans. R. Dublin Soc., vol. ii. p. 56). The companion example ("G. C." 1383) is found in the Milky Way, near Orion's right arm.

Researches into the mutual relations of clustered stars are still in their infancy. They will demand for their prosecution a reserve of patience as inexhaustible as the store of problems to be successively confronted. Before these come to an end, the human race itself will perhaps have become terrestrially extinct. But not, we may hope, before much has been attained that is well worth waiting and working for.

A. M. CLERKE.

THE COLOURING MATTER OF THE TESTA OF THE SEED OF RAPE (BRASSICA NAPUS).

THE testa of the seed of this species of Brassica is dark brown in colour, so dark often as to appear almost black. Being curious as to the chemical nature of the colour in this outer seed skin, I made several very simple experiments (which, however, have been thoroughly successful) with a view to elucidate the matter. After trying many solvents I was able to dissolve out the greater part of the colouring material by the use of that very common solvent, viz. a 25 per cent. solution of hydrochloric acid.

I put two or three hundreds of rape-seeds into a large test-tube (boiling-tube), covered them over completely with the dilute hydrochloric acid, and let the whole stand for three days.[1]

At the end of that time the solvent had acquired a very distinct pale brownish-violet (inclining to magenta) colour.

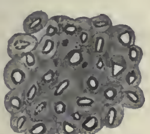
When a little of this dilute hydrochloric acid extract was mixed with as much *strong pure* hydrochloric acid, and gently heated, an intense yellow colour was developed, pointing to the very probable presence in the solution of iron in the ferric condition.

When potassium ferrocyanide was added to another portion, in a test-tube, of the original dilute HCl solution, a pale greenish-blue colour, which gradually darkened, was produced, and, after standing for about a day, the characteristic Prussian-blue precipitate indicative of ferric iron was observed to have settled to the bottom of the tube.

Potassium sulphocyanide confirmed the results obtained above, by giving, when added to the original solution, a well-marked blood-red coloration, showing the certain presence of a ferric compound in the liquid tested.

By these simple experiments I proved the presence of iron. I now wished to ascertain the nature of the iron compound in the testa which gave it its characteristic colour. I adopted the following simple method of investigation:—

I soaked for about a day a hundred or so of seeds. Then I took off the skins, which the soaking had rendered easily removable, placed them on a clean platinum foil, and heated to and kept at a white heat till all the water and organic matters were driven off, and nothing but ash remained. This ash—which was very small in quantity, of course—was reddish-brown in colour, and so was *undoubtedly*, in large measure at least, ferric oxide. When this red ash was treated with moderately strong hydrochloric acid, the intense yellow colour due to the production of ferric chloride was developed, and the potassium ferrocyanide and the sulphocyanide give the characteristic tests recorded above.



Section of testa of rape-seed showing thick corky cell-walls impregnated with hydrated (?) ferric oxide. (× 160).

No doubt, then, was left in my mind by these experiments that the iron existed in the state of ferric oxide (most probably hydrated ferric oxide or limonite) in the testa of the rape-seed. I was next anxious to know how the ferric oxide was distributed in the corky tissue of the testa. It soon occurred to me that this was also a very simple matter to investigate. I embedded several testas in paraffin, and by means of my microtome cut several thin sections, mounted in water, and examined them with a medium microscopic power. The cell-cavities were entirely empty; the thick corky walls were quite red. There, then, in the walls, plainly enough, the ferric oxide was seen to be distributed.

How did the ferric oxide get into its place in these walls? This, I think, is the explanation. The iron was taken in from the soil, by the root hairs of the plant which produced the seeds, chiefly in the ferrous state, probably as ferrous carbonate or chloride. It was conveyed in the water stream through the parent plant to the seed, and there deposited as an accessory substance in the cell-walls of the testa amongst the corky matter while the process of wall-thickening was going on. It was afterwards, or during the process of deposition, oxidized and hydrated (?), and so the seed of *Brassica Napus* acquired its characteristic tough dark brown testa.

ALEXANDER JOHNSTONE.

Edinburgh University.

[1] I know now that it was not necessary to let the seeds remain in the acid so long. By a little gentle and judicious heating for about twenty minutes sufficient of the colouring substance would be extracted to enable one to determine its nature.

THE TAIL-BRISTLES OF A WEST INDIAN EARTHWORM.

I HAVE recently received from Mr. Reginald Windle a small collection of earthworms from Bermuda, among which is a new species showing a remarkable peculiarity of structure which I have not observed, or seen recorded, in any other earthworm.

The posterior extremity, for the length of about half an inch (the worm measures about three inches), is furnished with bristles, which, as in *Urochæta*, are disposed in an alternate fashion; the eight bristles on each segment do not correspond in position to those of the preceding or succeeding segments, but are placed so as to correspond to the intervals between them.

In my specimen the bristles at the end of the body were extremely conspicuous, and, when examined by a lens, appeared to end in a thickened head; the skin felt sticky when touched by the finger. When a portion of the body-wall was teased up in glycerine, and examined with a microscope, the bristles showed the very remarkable shape indicated in the accompanying woodcut (Fig. 1, *a*). The bristle is very large—compared with those upon the more anterior segments (*b*) and those of other

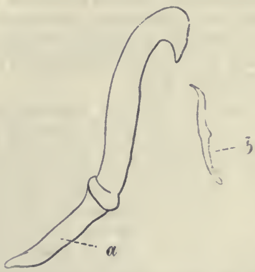


FIG. 1.—*a*, one of posterior setæ; *b*, seta from about middle of body. Both drawn to scale with *camera lucida*.

earthworms—and the free end is bent into a hook, the point of which lies in a direction nearly parallel to that of the shaft. The whole bristle is enormously thicker than those which are found upon the anterior segments, and of a deep yellow colour. At about the middle of the shaft, where a slight swelling is commonly met with in the bristles of other earthworms, is a thickened rim which suggests the attachment of powerful muscles. The hooked end of the bristles accounts for the "sticky" feeling of

the skin, which I was first inclined to ascribe to a secretion of the cutaneous glands.

This curious modification of the posterior bristles has an evident relation to the habits of these creatures. All observers, from the time of Gilbert White onwards, have noticed that earthworms constantly, at night and in wet weather, lie outside their burrows with only the extreme end of the body fixed in the hole; when alarmed they dart back with great rapidity. Now it is quite clear that this movement depends upon the fact that the bristles at the posterior end hold that part of the body firm while the anterior part is being retracted. Probably the alternate arrangement of the bristles in *Urochæta* and in the Bermuda worm (which is a close ally of *Urochæta*, if not a species of the same genus) are useful to the worm in performing such rapid movements, inasmuch as they permit of a firmer hold of the ground. When these bristles become strong hooks, as in the Bermuda worm, the stability of the hinder end of the body must be enormously increased.

I have, however, no information as to the habits of these worms, so that I can only suggest a possible explanation of the presence of these remarkable hooks.

FRANK E. BEDDARD.

NOTES.

OUR readers will be glad to hear of the safety of Prof. A. C. Haddon, of the Royal College of Science, Dublin, who recently started for Torres Straits. He writes, under date September 9, reporting himself in good health, and well pleased with both the climate and his reception. If he is carrying out his original programme, he should be now busy among the islands in the middle of the Great Barrier Reef to the west side of the straits. His captures already embrace several new Actiniæ and some probably new Nudibranchs; and he is also at work upon the habits and placentation of the Dugong. He is much interested in the natives, and struck by the alarming rapidity of their decrease and modification. They are fast dying out, and their customs with them, and the information to be obtained from the younger men concerning the doings of their forefathers is so unsatisfactory that Prof. Haddon is losing no opportunity of studying the anthropology of the islanders and of collecting material in illustration thereof.

IT is understood that Prof. Giard is about to be appointed immediately to the new Professorship of Évolution des Êtres Organisés in the Sorbonne. His lectures will begin this month, and the first course will deal with embryological phenomena in relation to the Darwinian theory.

La Nature (October 20) prints an interesting article, by Dr. Camille Viguier, on the Zoological Station at the town of Algiers, the only institution of the kind which has yet been established on the southern coast of the Mediterranean. Eight years ago, when schools of law, science, and literature were formed at Algiers, Dr. Viguier resigned an educational office he held in France, in order to associate himself with the new Algerian schools, in the hope that he might be permitted to create a marine laboratory. This hope has been fulfilled, and, although the site has some disadvantages, he is, upon the whole, satisfied with the opportunities of research which have been provided for him. He calls especial attention to the fact that it is not necessary for naturalists to go to Algiers to profit by the institution. Those who write to him will receive, as soon as circumstances permit, and prepared in accordance with their directions, any animals that can be procured at Algiers.

THE Trustees of the British Museum have appointed Mr. Alfred Barton Rendle, late Assistant Demonstrator of Botany, Cambridge, an assistant in the Department of Botany at the Natural History Museum, in the vacancy occasioned by Mr. H. N. Ridley's taking the office of Director of the Botanical Gardens at Singapore.

MR. H. BURY, who has recently been elected to a Natural Science Fellowship at Trinity College, Cambridge, began the study of biology at Eton, and obtained a First Class in the Natural Science Tripos of 1885 (Part I.), having previously gained a Foundation Scholarship at Trinity College. He spent the winter of 1886–87 at the Naples Zoological Station, and the results of his work, which has added much to our knowledge of the development of Comatula, have been recently published in the Philosophical Transactions, with five illustrative plates. He returned to Naples at the beginning of the present year to study the larvæ of other Echinoderms, and his observations will be published in an early number of the *Quarterly Journal of Microscopical Science*.

THE Princess Louise will open the Durham College of Science, Newcastle, on Monday next, November 5, at 12 o'clock.

IT is reported from India that Mr. Griesbach, the geologist of the Indian Survey, sent for a time to Afghanistan at the request of the Ameer, has been compelled by the rebellion of Ishak Khan to postpone his geological exploration north of the Hindu Kush, and to remain at Cabul.

ACCORDING to *Allen's Indian Mail*, it is the intention of the Government of India to utilize the services of Mr. J. Duthie. Hitherto that gentleman has confined his botanical researches to Northern India, but it is now proposed that his sphere shall include not only the whole country but also regions beyond the Indian frontier. Accordingly he was sent with the Black Mountain Expedition.

THE British Consul at Costa Rica, in the course of his last annual report, states that a National Museum has been established at San José, and several valuable collections of Indian relics, birds, insects, plants, &c., have been presented or purchased. It is intended that in course of time it shall contain specimens of all the natural products of the country. A national publication and exchange office has also been opened, and all countries are invited to exchange periodicals and publications with Costa Rica.

ACCORDING to a Reuter's telegram from New York, dated October 29, two slight shocks of earthquake had occurred at New Bedford, Massachusetts.

IN the Report of the Committee of Council on Education in Scotland for the past year we find that while 1595 schools taught history and geography only fifty-nine took up elementary science. Strange to say, agriculture is not taught in any of the Scotch training colleges. In the secondary schools, what are called the University subjects—that is to say, Latin, Greek, and mathematics—are very well taught, particularly mathematics. The Technical Schools (Scotland) Act of 1887 has opened, says the Report, to School Boards a new field of operations in regard to a branch of education to which public attention has of late been very closely directed. Only very few schools appear to intend to take action under the Act, chiefly, no doubt, because the Boards are opposed to any increased expenditure. Technical instruction is already given in many of the higher schools. The Report recommends managers of schools in which it is proposed to give technical instruction to secure, if possible, the co-operation of local manufacturers, and to combine with other Boards as pointed out in the Act of 1887.

THE recent meeting of the Congress of American Physicians and Surgeons at Washington seems to have been a great success. It lasted three days, Dr. John Shaw Billings acting as President. *Science* considers that the meeting marked a new departure in national gatherings of American medical men. "It was a con-

vention of specialists," says our American contemporary, "of men who have pursued their investigations, each in his own department, far beyond the point reached by the ordinary practising physician, even though his professional equipment be of the best. The papers that were read, therefore, presented the results of the most advanced scientific researches in the several departments, and the organization of the Congress is such as to insure in the future the maintenance of this high scientific standard. All opportunity for scheming medical politicians to gain prominence or office is carefully guarded against, and the only chance that any physician has to gain distinction through membership of the Congress is by presenting papers of such high order of excellence as to command the attention and secure the approval of the learned members of the medical profession to whom, as to the most competent critics, he submits his work."

AT one of the meetings of the Anatomical Society, during the session of the Medical Congress in Washington, Dr. Lamb, of the United States Army Medical Museum, spoke briefly of a singular phenomenon he had observed in his examination of human breast-bones. It was the occurrence, in a number of specimens, of an eighth rib, the cartilage that is usually found below the seventh rib being fully developed into a rib. Dr. Lamb first saw a specimen of this kind about ten years ago. While teaching, he had occasion to observe the subject he had before the class with great care, and was surprised on one occasion, on counting the ribs, to find that there were eight. He made no further investigation at the time, but recently he has given the subject more attention, and now he has in his own collection four specimens, while in the Army Medical Museum there are eight more. In all these cases the phenomenon occurs in Negroes, but one additional specimen is that of an Indian. *Science* says that Dr. Lamb has made a thorough search of anatomical literature for references to this peculiarity. In the English books there is only a single incidental reference to it, and in that case the author does not say that he has ever seen a specimen. In German books there are two references, one of them being the one already mentioned by the English authority. The French anatomists do not mention it at all; and only one American, Allen, makes any reference to it. Among the anatomists attending the Medical Congress. only two or three had seen specimens. Dr. Billings, in a circular he has sent out to anatomists and others, has requested that information on the subject be sent to the Army Medical Museum.

PART 4 of the Synchronous Weather Charts of the North Atlantic and the Adjacent Continents has been published by the Meteorological Council. It deals with the weather of each day from May 25 to September 3, 1883, and is the completion of the discussion undertaken for the thirteen months from August 1882, in connection with the international system of circumpolar meteorological observations. This last volume of the Atlantic Weather Charts is in every way equal in value to the previous parts issued, notices of which have been given in NATURE. Part 4 represents the weather of the summer season when the conditions over the Atlantic are necessarily quieter than they are in the winter. There is, however, much that is of general interest, and the broken weather so commonly experienced during an English summer is well pictured. Considerable play is shown in the behaviour of the high-pressure area usually situated in Mid-Atlantic, and the direct influence of its movement upon the weather over a very large area of both sea and land is very evident. At the beginning of June the high barometer holds a very central position, and extends across the Atlantic from coast to coast, the barometer reading as high as 30·6 inches. Several low-pressure areas are skirting to the northward, but they are pushed to a higher latitude than usual, and too far to the north to cause any serious disturbance of the weather in the neighbourhood of the British Islands. The high-pressure area, however, soon breaks up, and by June 7 there is no isobar over the Atlantic with a higher value than 30·2 inches, whilst the depressions embrace the whole area to the north of 40° N., and are more serious in character. These unsettled conditions continue till about June 20, when the high-pressure area regains its normal position; but there is somewhat similar play in the high-pressure system shown in July and August, although to a much less extent. The charts show a good instance of storm development in Mid-Atlantic on August 11, which ultimately caused a heavy gale in England on the 14th. A low-pressure area was also formed in 28° N. and 65° W. on August 20, which developed to a hurricane by the 22nd, and apparently reached our islands in a modified form on August 28. Good instances of the movement of depressions are also shown, some disturbances being traced across the Atlantic. An additional sheet is given containing the charts for September 1 to 3, in order to show the passage across the British Islands of a steep cyclonic system which was accompanied by severe gales. The depression apparently originated in 20° N. and 55° W. on August 21, and, after crossing the British Islands at the commencement of September, it passed over the North Sea, and subsequently disappeared.

THE First Annual Report of the Meteorological Society of Australasia, which was established chiefly by the labours of Mr. C. L. Wragge, shows that the Society is making good progress, and now numbers upwards of eighty subscribing members. There are twelve observing-stations established under the auspices of the Society, and it is proposed to establish others in Fiji and Norfolk Islands. The Council also propose to collect observations from ships, with the view of carrying out investigations similar to those undertaken by Dr. Meldrum at the Mauritius. Several papers of interest have been read by members during the past year, and an abstract of the climatological observations is published in the Proceedings of the Society.

TWO remarkable new polymers of methyl and ethyl cyanides, forming well-developed crystals, have been obtained in the laboratory of Prof. E. von Meyer at Leipzig. They possess percentage compositions precisely the same as those of CH_3CN and C_2H_5CN, but twice the molecular weight, and are therefore represented by the formulæ $C_4H_6N_2$ and $C_6H_{10}N_2$. The latter compound was obtained as follows. Metallic sodium, in small pieces, was rapidly added to a solution of ethyl cyanide in absolute ether. A brisk action very soon commenced with formation of a white pulverulent precipitate, and escape of gaseous ethane C_2H_6. After the cessation of frothing, the mixture was warmed upon a water-bath, and the precipitate afterwards separated by decantation, washed with ether, and thrown into water. Decomposition at once occurred, accompanied by the separation of an oil, which on standing solidified in magnificent tabular crystals. These were readily obtained pure by washing with cold water, in which they are but sparingly soluble, pressing between filter-paper, and drying in a desiccator. On analysis they were found to give the same numbers as ethyl cyanide. The latter compound, however, is a liquid boiling at 98° C., while the new substance is a solid melting at 47°–48°. It may be distilled without change, boiling at 257°–258°, 160° higher than ethyl cyanide. Vapour-density determinations show that it possesses twice the molecular weight of the latter compound, a result which was confirmed by determinations according to the new method of Raoult, described in these columns a short time ago. The cycle of chemical changes resulting in the production of this curious polymer have been fully worked out, and are briefly as follows. One atom of sodium appears to replace an atom of hydrogen in one molecule of C_2H_5CN, forming sodium cyan-ethyl, C_2H_4NaCN; a second atom of sodium at the same

time seizes the CN of another molecule, leaving the C_2H_5 to combine with the displaced hydrogen to form ethane. Thus—

$$\begin{matrix} Na & C_2H_5CN \\ + & \\ Na & C_2H_5CN \end{matrix} = \begin{matrix} NaCN + C_2H_5H \\ \\ + C_2H_4NaCN. \end{matrix}$$

The sodium cyan-ethyl then combines with a third molecule of C_2H_5CN to form the sodium derivative $C_6H_9NaN_2$ of the new polymer; this unstable compound is finally decomposed in contact with water, with formation of caustic soda and the new polymer itself. The constitution of this singular compound was conclusively proved to be—

$$\begin{matrix} C_2H_5—C(NH) \\ \quad\quad | \\ \quad\quad CH(CH_3)CN. \end{matrix}$$

In a similar manner the methyl polymer was obtained by the action of sodium upon methyl cyanide, marsh-gas, CH_4, being evolved, and a white substance formed, which, on decomposition by water, yielded the polymer $C_4H_6N_2$ as an oil, eventually crystallizing in white needles melting at 52°–53° C.

THE British Consul at Barcelona, in a report to the Foreign Office on the agriculture of his district, says that a voracious caterpillar which made its appearance in myriads last year amongst the cork forests and stripped the trees completely of their foliage, is now attacked and devoured by another insect, a species of beetle, of a dark-green colour, and armed with a horn with which it cuts up the worms or caterpillars. Besides this deadly enemy, two others are at war with the caterpillar: a crab (*cangrejo*), and an insect, hitherto unknown, which destroys the bags containing the newly-laid eggs of the butterflies. There is very little doubt that the caterpillars will soon be completely exterminated.

A SCHOOL OF FORESTRY has been opened at Akhaltzik, in the Caucasian provinces. The scholars will be selected from the native forest-police actually in the service of the Russian Government. The increased demand made on the forest staff by the law which was passed last April, and which is in force in many districts since July, is the cause of the founding of this school. The officials hope that with an increased staff they will be able to check the devastation of Russian forests.

DR. A. JULIEN AND PROF. H. C. BOLTON have submitted to the New York Academy of Sciences a Report on the results of their researches on sonorous sands. They have collected samples from all parts of the world, and, on close examination, found that all sonorous sands are clean; that no dust or silt is found mixed with the sand; that the diameter of the angular or rounded grains ranges between 0·3 and 0·5 of a millimetre; and that the material may be siliceous, calcareous, or any other, provided its specific gravity is not very great. When these sands are moistened by rain or by the rising tide, and the moisture is evaporated, a film of condensed air is formed on the surface of each grain, which acts as an elastic cushion, and enables the sand to vibrate when disturbed. In sands mixed with silt or dust, these small particles prevent the formation of a continuous air-cushion, and therefore such sands are not sonorous, If this theory be correct, sonorous sand must become mute by the removal of the film of air. Experiments of the authors prove that by heating, rubbing, and shaking, the sand is "killed." All these operations tend to destroy the film of air condensed on the surfaces. On the other hand, samples of sonorous sand were exhibited which had been kept undisturbed for many years. They had retained their sonorousness, but, after having been rubbed for some time, became almost mute. The aim of the authors is now to make a sonorous sand.

MESSRS. A. C. MCCLURG AND CO., Chicago, have issued the fifth edition of "A Manual of the Vertebrate Animals of the Northern United States," by David Starr Jordan, President of the University of Indiana. The work has been wholly re-written, and the order of arrangement is reversed, the lowest forms being placed first.

WE have received the third edition of Mr. Milnes Marshall's well-known text-book "The Frog: an Introduction to Anatomy, Histology, and Embryology." The present edition, we are told in the preface, has been carefully revised, and an account of the development of the frog has been added.

MR. J. RUSSELL has put together a short account of the life and system of Pestalozzi. It is called "The Student's Pestalozzi," and is based on "L'Histoire de Pestalozzi," by Roger de Guimps. Messrs. Swan Sonnenschein and Co. are the publishers.

THE latest issue of the Proceedings and Transactions of the Nova Scotian Institute of Natural Science (vol. vii. Part 2) includes the following papers:—Glacial geology of Nova Scotia, by the Rev. D. Honeyman; list of Nova Scotian butterflies, by Arthur P. Silver; on the elementary treatment of the propagation of longitudinal waves, by Prof. J. G. Macgregor; Carboniferous flora, with attached spirorbes, by the Rev. D. Honeyman; fishes and fish development, by Harry Piers; Carboniferous of Cape Breton, by E. Gilpin, Jun.; Japanese magic mirror, by Harry Piers; museum meteorites, by the Rev. D. Honeyman; and Nova Scotian superficial geology, systematized and illustrated, by the Rev. D. Honeyman. There is also an appendix on birds of Nova Scotia, by Andrew Downs, edited by Harry Piers.

MESSRS. WILLIAM WESLEY AND SON have just issued No. 92 of their "Natural History and Scientific Book Circular," containing an important list of books on botany.

WE have received Mr. J. H. Steward's Catalogue (Part 5) of improved magic and dissolving-view lanterns and slides, with a complete catalogue of photographs for the magic lantern.

THE Calendar of the Huddersfield Technical School and Mechanics' Institute for the forty-eighth session, 1888–89, has been issued. From the report of the Governors for the session 1887–88 it appears that the institution has been making good progress "on every side." The buildings will soon have to be enlarged, and the Governors look forward to the hearty support and co-operation of the town and neighbourhood in this undertaking.

WE learn from *Science* that a manufacturing firm in New York has sent to the United States Department of Agriculture specimens of a new fibre they are making from the stalk of the cotton-plant. The samples received strongly resemble hemp, and seem to be adapted to all the uses hemp is put to. A few fibres of it twisted together in the hand show remarkable tensile strength, although no exact comparative tests with other fibres have yet been made. A collection of the fibres of hemp, flax, jute, ramie, &c., from all parts of the world is being made by the Department, and a new instrument has been invented by which it is expected that the tensile strength of each will be ascertained with great accuracy.

THE additions to the Zoological Society's Gardens during the past week include a Common Seal (*Phoca vitulina*) from British Seas, presented by Mr. Geo. Stevenson; a Tawny Owl (*Syrnium aluco*) from Ross-shire, presented by Mr. J. Weston; a Little Grebe (*Tachybaptes fluviatilis*), British, presented by Mr. Bibby; a Starred Tortoise (*Testudo stellata*) from Ceylon, presented by Mr. William Ford; an Alligator (*Alligator mississippiensis*) from Florida, presented by Mr. G. A. Ruck; a Puff Adder (*Vipera arietans*), an African Cobra (*Naia haje*) from North Africa, presented by Mr. Herbert E. White; a Macaque Monkey (*Macacus cynomolgus* ♂), a Larger Hill-Mynah (*Gracula inter-*

media) from India, three Red Deer (*Cervus elaphus* ♂ ♀ ♀), British, two White-tailed Gnus (*Connochætes gnu* ♂ ♀), bred in Holland; a Ruffed Lemur (*Lemur varius*) from Madagascar, deposited; a Red-crested Pochard (*Fuligula rufina* ♂) from India, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *NOVEMBER* 4-10.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 4

Sun rises, 7h. 2m.; souths, 11h. 43m. 40·9s.; sets, 16h. 26m.: right asc. on meridian, 14h. 40·3m.; decl. 15° 36′ S. Sidereal Time at Sunset, 19h. 23m.

Moon (New on November 4, 0h., and at First Quarter November 10, 16h.) rises, 7h. 13m.; souths, 12h. 18m.; sets, 17h. 12m.: right asc. on meridian, 15h. 14·5m.; decl. 13° 20′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	6 16	11 15	16 14	14 11·2	12 29 S.
Venus ...	9 54	13 47	17 40	16 44·3	23 28 S.
Mars ...	12 0	15 43	19 26	18 40·0	24 41 S.
Jupiter ...	9 30	13 36	17 42	16 32·6	21 27 S.
Saturn ...	23 7*	6 34	14 1	9 29·6	15 45 N.
Uranus ...	4 47	10 15	15 43	13 11·7	6 57 S.
Neptune..	17 19*	1 4	8 49	3 58·7	18 46 N.

* Indicates that the rising is that of the preceding evening.

Occultation of Star by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
6	B.A.C. 5954	6	18 13	18 57	75 348

Nov.	h.	
5	19	Jupiter in conjunction with and 3° 5′ south of the Moon.
6	3	Venus in conjunction with and 4° 28′ south of the Moon.
6	22	Mercury at least distance from the Sun.
8	0	Mars in conjunction with and 2° 35′ south of the Moon.
9	18	Mercury stationary.

Saturn, November 4.—Outer major axis of outer ring = 40″·7: outer minor axis of outer ring = 9″·7: southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Nov. 5,	1 49	*m*
			,, 10,	1 28	*m*
λ Tauri	3 54·5	12 10 N.	,, 7,	2 23	*m*
T Monocerotis	6 19·2	7 9 N.	,, 6,	5 0	*m*
R Canis Majoris	7 14·5	16 12 N.	,, 8,	23 42	*m*
			,, 10,	2 58	*m*
R Ursæ Majoris	10 36·7	69 22 N.	,, 7,		*M*
S Ophiuchi	16 27·8	16 55 S.	,, 9,		*M*
U Ophiuchi	17 10·9	1 20 N.	,, 6,	18 58	*m*
W Sagittarii	17 57·9	29 35 S.	,, 7,	19 0	*m*
β Lyræ	18 46·0	33 14 N.	,, 5,	2 0	*m*
S Vulpeculæ	19 43·8	27 1 N.	,, 8,		*M*
S Sagittæ	19 50·9	16 20 N.	,, 8,	19 0	*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 8,	6 0	*m*
Y Cygni	20 47·6	34 14 N.	,, 4,	3 0	*m*
			,, 7,	3 0	*m*
W Cygni	21 31·8	44 53 N.	,, 10,		*m*
δ Cephei	22 25·0	57 51 N.	,, 9,	1 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Camelopardalis	54	71 N.	Swift.
The *Taurids*	60	20 N.	Slow; bright.
	350	52 N.	Rather slow.

GEOGRAPHICAL NOTES.

IN some notes embodying the results of his own observations, contributed to the *Mittheilungen* of the Hamburg Geographical Society, Dr. H. Lindemann throws some light on the physical geography of the interior of Heligoland. He points out that the island is protected on the east from the action of the sea by a long and narrow sand-dune, about 1¼ mile distant. The gradual disappearance of this British possession, Dr. Lindemann points out, is but partly due to the action of the sea. This is especially the case with the western side, where the strength of the waves is much greater. The chief factors in wearing down the island are the heavy rainfall, the variations in the weather, and the dissolving power of the frost; all these causes effect the disintegration of the stones and the denudation of the land. The results can be seen better at work on the eastern side of the island, for the strata and the inclination of the Oberland are towards the north-east, and all the water consequently flows that way. The eastern side is largely planted with potatoes, and the gradual disappearance of these potato-fields gives us a tolerably good basis on which to calculate the sinking of this side of the island. There is now nothing remaining of a potato-field which only eighty years ago measured 80 metres, and another field, 25 metres broad, has been reduced within the same period to 3 metres. In old maps we find an ancient cemetery on the eastern side of the Oberland, which had to be removed to its present position. These causes, but, above all, the direction in which the strata lie, produce the different aspect of the eastern and western sides. The eastern cliff is mostly uniform and perpendicular; the western side offers a splendid and varied example of the invasive powers of the sea, with its many inlets, caverns, and chiselled pillars now separated from the main rock. From a comparison with the measurements taken in 1845, Lindemann finds that the western cliff had receded about 7 feet in the last forty years, or at the rate of about 2 inches a year. The Unterland was joined to the dune by a stone jetty, called the Waal, as recently as the seventeenth century. This Waal formed a kind of semicircular harbour, open on the north and south sides. If we take Geern's map, we find the place of the old northern harbour occupied then by green pastures and meadows. But this has all been swept away; the sea carried most of the jetty towards the Unterland and the dune. The destruction of the breakwater had the effect upon the mainland that the Unterland, against which the masses of stone were driven, was gradually so greatly increased that new rows of houses could be built upon the beach.

IT is stated that contracts have been entered into in America for the construction of two steamers intended for an expedition to the Antarctic regions, which is being organized by Mr. Henry Villard. The officers and scientific staff of the expedition will all be Americans and Germans, as the enterprise is stated to be in great part supported by Hamburg money. The expedition will start from New York, and its object will be mainly the exploration of the South Shetlands, South Orkneys, South Georgia, and the Poovel Islands. This expedition seems to be independent of that to be sent out by the German Government under the conduct of Dr. Neumayer.

MR. JOSEPH THOMSON has returned from Morocco in compliance with an urgent telegram from the British East African Company. Mr. Thomson will probably start immediately for Mombassa, and we have reason to believe will be intrusted with a very important mission to the interior.

PRELIMINARY NOTE ON THE ANATOMY AND PHYSIOLOGY OF PROTOPTERUS ANNECTENS.

Introduction.

OWING to the generosity of Prof. Wiedersheim, I have recently had the opportunity of making some observations on the structure and mode of life of *Protopterus*. Although I can at present only give a few brief notes on the subject, some points have already proved so interesting, that it has seemed worth while to give an abstract of my results up to the present time, leaving a detailed description until a later date.

I was fortunate enough to be present in Freiburg at the end of last June when a quantity of fresh material arrived. This was procured direct from the Gambia, owing, in the first instance, to

the energy of Dr. J. Beard, who, assisted by a grant from the Royal Society, hoped by procuring Protopterus in sufficient numbers, and keeping them alive under suitable conditions, to be able to study their development. My thanks are therefore due to Dr. Beard, as well as to Prof. Wiedersheim, for the specimens I have made use of.

The clods of earth in which the animals were inclosed in their torpid state having been opened up, they were found to contain about one hundred living specimens, varying in length from about 8 to 80 cm. These were kept in a tank in the Botanical Garden, in water which stood at a temperature of 18° Réaumur. They were fed with water-snails, earth-worms, Entomostraca, and small fishes, the last of which they seemed to prefer. But the abundant nourishment with which they were supplied did not prevent them from killing one another, so that at the date of writing only a small proportion still remain alive. In order to prevent this cannibalism, we should have isolated them by means of wire-netting, had it not been thought that this would greatly lessen any chance of obtaining embryos. Their vitality is very remarkable: after having been bitten severely, and having consequently lost much blood, they will usually live for some days.

The structure of the "cocoon," and the position of the animal within it, have already been described by Wiedersheim,[1] and in this connection I have only one point to add with regard to the respiration of Protopterus during its torpid state. Although in one or two of the specimens we noticed a slight redness of the tail, I doubt very much whether, as Wiedersheim supposed, the tail serves as a respiratory organ during this period. A close examination of that part of the cocoon-membrane which closes the bottom of the earth-tube, and which overlies the animal's nose, showed that no additional respiratory apparatus was necessary. Looking at this membrane from the outer side, the small aperture described by Bartlett and Krauss can be plainly seen. On the inner side, the rim of this aperture is produced into a funnel-shaped tube, the free end of which lies between the lips of the animal. Consequently, by means of this pipe, the Protopterus can inhale and exhale air during its long sleep. On being removed from the cocoon, moreover, the lungs were always found to be greatly distended and full of air, bubbles of which were immediately given off into the tank in which the specimens were placed. In all probability the above-mentioned tube is produced by suction, when the secretion which gives rise to the cocoon is still soft. The curious squeak which Protopterus makes when set free from the cocoon has been noticed by other observers.

In addition to dissections, and sections of various individual parts, I have made a complete series of transverse sections—in all about 2100—of a small female specimen: these are extremely instructive.

Integument.—Each outer cell of the epidermis is provided with a cuticular cap, and the whole of the epidermis is closely packed with goblet cells, which are less numerous on the paired fins than on the body, where they are less than the diameter of a single one apart. Multicellular glands, very simular to those of Amphibia, are also present here and there throughout the body, and are particularly numerous on the snout. Nests of lymphatic tissue are present beneath the epidermis in some regions.

Muscles.—The chief point of interest I wish to mention concerning the muscles is that they, more particularly the great lateral muscles of the tail, serve as stores of nutriment for the animal during its torpid state. A similar phenomenon has been described by Miescher-Rüsch in the salmon during the spawning season.[2]

The muscular tissue in places shows histologically all stages of retrogressive metamorphosis, and owing to this process, the leucocytes are able to absorb its broken-down remnants, which can be plainly recognized within many of the leucocytes which simply swarm into the muscles in these regions. In some parts the muscle is completely eaten away, so that nothing but the perimysium is left.

Nervous System.—An account of the structure of the nerves, with their numerous spindle-shaped nuclei, and of the remarkable nerve-cells, I cannot give here, and I also reserve at present a description of certain of the cranial and spinal nerves, and of the nerve-supply of the fins. I must, however, mention that the pulmonary nerve crosses its fellow at the base of the lungs, and then runs along the dorsal surface of the lung of the other side. A lateral nerve is situated on either side of the notochord, beneath the muscles, at the point where the dorso-lateral and ventro-lateral muscles meet. The spinal ganglia lie outside the canal. No trace of a sympathetic could be detected.

SENSORY ORGANS.—*Integumentary Sense-organs.*—These are very numerous in the head, and in the body they are not restricted to the main lateral line, but are present in regions above and below it also. They are situated within the epidermis, external to the scales. The moisture necessary for their persistence during the torpid period is produced by the gland-cells of the integument. A mass of lymphatic tissue is usually present directly beneath each: this may be concerned with its nutriment, for it is known that in Amphibians these organs are continually undergoing regeneration. I have been unable to discover any sensory organs in the integument of the paired fins, and the function of these curious filamentous appendages, with their large nerve-supply, is still problematical.

The pharynx is provided with sensory organs similar in structure to those of the integument.

Olfactory Organ.—The structure of the nose is very complicated. In the presence of accessory cavities, it resembles that of Amphibia, but in the folding of the epithelium it is more similar to that of fishes. The main cavity gives rise to dorsal and lateral extensions, the latter corresponding closely with the "pars maxillaris" of Amphibia. Posteriorly, the main cavity branches into a number of tubes, each with a small lumen; these in transverse section resemble gland-tubes cut across. The olfactory cells are in some parts diffuse, in others arranged in groups, as in many fishes and Amphibians.

No special glands are present in connection with the nose, as one would naturally expect. But the moisture necessary for the olfactory cells is probably produced by the numerous goblet cells which are present in the epithelium of the mouth and that lining the anterior and posterior narial passages. This may explain the peculiar position of the anterior nostrils, which open beneath the upper lip.

Eye.—No gland is present within the orbit. The lens is globular and relatively large, filling up the greater part of the posterior chamber, so that there is little space left for the vitreous body. The sclerotic is fibrous, but a few cartilage cells can be recognized in those regions in which the eye-muscles are inserted. The choroid is rudimentary, and contains no pigment, and there is no iris or pupil, the pupillary membrane being continuous over the front of the lens. The epidermis thins out slightly over the eye, and in this region the goblet cells are smaller and less numerous. The dermal fibres are directly continued on the one hand into the representative of the cornea, and on the other into the sclerotic. Processes of the pigment cells of the retina can be seen passing between the rods and cones. No trace of a processus falciformis could be seen.

ALIMENTARY CANAL.—*Lips.*—No muscles are present in connection with the lips, as is stated to be the case by Ayers.[1] Beneath the epidermis they consist of a curious embryonic connective tissue very rich in nuclei, similar to that found in the snout and tongue.

Tongue.—The tongue is covered with numerous filiform papillæ in older specimens, the histological structure of which I have not yet examined. None are present, however, in the sections of a younger specimen. Goblet cells are very numerous in the epithelium, which is folded so as to give rise to a number of simple gland-like sacs. No sense-organs can be seen in my sections. The extrinsic muscles are, on either side, (1) a large hyoglossus, and (2) a small band-like branchioglossus. There are no intrinsic muscles, the whole of the substance of the anterior part of the tongue beneath the epithelium consisting of the connective-tissue referred to above.

On the floor of the mouth, in front of the tongue, and between the two cusps of the mandibular teeth, is a curious tube-like epithelial organ which apparently opens by a small aperture near its posterior end into a median groove of the oral epithelium. This tube is lined by columnar epithelium with goblet cells.

Thyroid.—The thyroid is a small bilobed organ, situated between the connective-tissue and muscular portions of the tongue. Its epithelium is flat, and the tubules contain a colloid mass which stains deeply.

Thymus.—The well-developed thymus consists of adenoid connective-tissue with leucocytes, and lies on the dorsal side of

[1] *Anat. Anzeiger*, II. Jahrgang, 1887; and Proc. Brit. Assoc., 1887.

[2] "Ueber das Leben des Rheinlachses im Süsswasser," *Archiv. f. Anat. u. Physiol.*, 1881.

[1] "Beiträge z. Anat. u. Physiol. der Dipnoër," *Jen. Zeitschr. f. Naturwiss.*, Bd. 18, 1885.

the gill-arches. Black pigment is present in the anterior part of its inner portion.

Epithelium.—The epithelium lining the mouth consists of polygonal cells, apparently without cilia. In the pharynx, nests of simple glands, like those of the tongue, are present; and, as already mentioned, numerous sense-organs are to be found in the region of the gill-clefts. The epithelial cells of the stomach and intestine are columnar, but vary much in their form and proportions. Cilia could be detected here and there; in all probability they occur in isolated regions, as in the adult lamprey.

With the exception of the large liver, there is no trace of any gland in connection with the stomach and intestine, and digestion must be thus performed largely through the instrumentality of leucocytes.

Muscles of the Alimentary Canal.—The muscles of the walls of both stomach and intestine are only very slightly developed in torpid specimens, and are apparently broken up and separated by the lymphatic tissue to be described presently. They probably, therefore, undergo a similar degeneration to that observed in the caudal muscles.

Lymphatic Organs of the Stomach and Intestine.—The form of these organs has been described by Ayers (*loc. cit.*) I have not been able to verify his supposition that there are direct connections between them and the lumen of the intestine. A central part of the lymphatic organ running down the axis of the spiral valve can be distinguished from the rest by its more compact structure. Many of the leucocytes in these regions are full of fat-globules.

A large lymphatic body is present behind the cloaca and pelvis, and probably serves to protect the vent from the entrance of harmful substances.

Cloacal Cæcum.—The so-called "urinary bladder" opens into the cloaca between the rectum and the urinary and generative ducts. It has therefore much the position of the "rectal gland" of Selachians, and probably has nothing to do with the urinary bladder of other forms.

Lungs.—The cavity of the lungs is divided up by trabeculæ, which give the anterior unpaired portion a sponge-like appearance: a central lumen is present in the paired portion. A large lymphatic organ lies beneath the anterior unpaired part, the curious relations of which I hope to describe later, and will now only mention that the blood corpuscles migrate from it into the tissues of the lung.

Abdominal Pores.—As Ayers has shown, only one abdominal pore is usually present, and in my sections this ends blindly, and does not open into the cœlome. Probably its relations vary in different individuals.

Blood Corpuscles.—The chief peculiarity of the blood of Protopterus is the large size of the corpuscles, and the comparatively large proportion of the white in comparison with the red. The form of the latter resembles that of the red corpuscles of Amphibians. In length they measure from 0·040–0·046 mm., and in width 0·025–0·027 mm. The size of the white corpuscles varies greatly. The diameter of the largest, when not throwing out pseudopodia, may exceed the length of a red corpuscle. Two kinds may be distinguished, as follows:—(1) Large leucocytes of the ordinary form, the protoplasm of which is usually distinctly differentiated into a coarsely granular endoplasm and a hyaline ectoplasm. In specimens prepared for me by Dr. Goldmann, according to Dr. Ehrlich's method, the protoplasm and nucleus are coloured violet. (2) Leucocytes of various sizes, the largest being usually rather smaller than those described above. The granules in the protoplasm are finer, and in addition to the ordinary blunt pseudopodia, stiff filamentous processes are also formed. The protoplasm of these stains brownish-red by Ehrlich's method, and Dr. Goldmann informs me that a similar coloration occurs in human white corpuscles in cases of leukæmia. Prof. August Gruber was kind enough to make a careful examination of these corpuscles with me, and we were able to trace a gradual disintegration in those described under (2), until finally nothing but the greatly altered nucleus is left. It seems probable, therefore, that these leucocytes convey the nutriment from the alimentary canal (or muscles) into the blood, and there disintegrate.

Blood-vessels.—I have at present only one or two remarks to make on the arrangement of the blood-vessels. Hyrtl's description [1] of the vessels of Lepidosiren would answer equally well in most points to Protopterus. Peters [2] describes a single pulmonary artery, arising from the efferent branchial vessels on the left side. This soon branches into two, each branch running along the inner side of the corresponding lung. No mention, however, is made by Peters of the corresponding right vessel, which has precisely the arrangement described by Hyrtl in Lepidosiren. This right pulmonary artery also divides into two, one branch passing along the dorsal surface of each lung alongside the pulmonary branch of the vagus.

The caudal vein divides up into two renal-portals. These are said by Hyrtl to anastomose anteriorly with a paired azygos in Lepidosiren. I have been unable to find any such "azygos vein" in Protopterus. The two so-called "venæ cavæ posteriores" doubtless correspond to the posterior cardinals, though they are somewhat modified. No lymphatic vessels could be detected.

Urinary Organs.—The kidneys are closely invested everywhere except on their dorsal side, by lymphoid and fatty tissue, which posteriorly forms a large median mass, plugging the end of the cœlome. No nephrostomes are present, as was supposed by Ayers.

A quantity of pigmented tissue on the outer and lower borders of the kidneys may possibly represent the adrenals.

Generative Organs.—Concerning the structure of the female generative organs, I have, as yet, little to add to the descriptions of former observers. No accurate account of the male organs exists, and I am inclined to think that the descriptions which have been given up to the present time referred to immature females, the generative organs of which might easily be taken for those of a male.

I have been able to distinguish no essential differences in external form between males and females: the latter are by far the more abundant.

Each testis has much the form and relations of an immature ovary, and, like the ovary, is invested along its free edge and sides with lymphatic and fatty tissue. Along its ventral surface a slight groove can be distinguished, at the bottom of which the spermatic duct lies. Posteriorly, the two ducts come to the surface, unite, and open by a common aperture on a papilla into the cloaca, just as in the female. In transverse sections, the seminiferous tubules can be seen opening into the ducts; in ripe specimens, fully formed spermatozoa can be seen in their lumina. I have, up to the present time, found nothing which could correspond to the remnant of a Müllerian duct, and, as the ureter undoubtedly must represent the mesonephric duct, there remains no other explanation of the duct of the testis than to suppose it to be the homologue of the Müllerian duct.

The form of the spermatozoa is very curious: they are carrot-shaped, and each is provided with two long cilia. They are very small, the length of the carrot-shaped head being only about 1/25 mm.

Most of the above observations were made in the Anatomical Institute in Freiburg i/B, where I have profited much by the kind help and advice of Prof. Wiedersheim.

August 31, 1888. W. NEWTON PARKER.

A more detailed examination of a male specimen, in which the spermatozoa were not yet ripe, has shown that distinct rudiments of the anterior parts of the Müllerian ducts are present. Each has an abdominal aperture, similar in form and position to that of the oviduct, and extends backwards for a short distance, tapering off before the level of the kidneys is reached. In sexually mature individuals, all traces of the Müllerian ducts appear to have vanished.

The duct of the kidney must therefore, as in Elasmobranchs, represent a special collecting-tube developed in connection with the posterior mesonephric tubules. W. N. P.

University College, Cardiff, October 27.

[1] *Abhandlungen d. böhm. Gesell. d. Wiss.*, 1845.
[2] *Müll. Arch. f. Anat.*, 1845.

THE WHEAT CROP OF 1888.

SIR JOHN LAWES has communicated some interesting facts with regard to the wheat crop of the present year. It has been Sir John Lawes's endeavour for many years past to establish a statistical relation between the fluctuations of the yield of wheat upon his own well-known experimental field in Hertfordshire with the general average obtained over the United Kingdom. In order to do this he has selected certain plots and taken their average yields, and it is maintained that the result so obtained fairly represents the average yield over the United Kingdom.

This conclusion is based upon observations extending over upwards of forty years, and has been rather forced upon the attention of Sir John Lawes, than assumed in the first instance.

The central position of Hertfordshire (at least with regard to England), and the medium character of the soil and climate, afford some reason for expecting an average yield; and the various treatments to which the selected plots have been subjected also assist to secure an average and representative result. The selected plots are five in number, each of which has been similarly treated for the last forty-five years, and all of them have carried wheat every year during this long period. One of these plots has remained continuously unmanured and has yielded on an average 13 bushels per acre, which, strange to say, is one bushel above the official average crop of the United States of America. One has been continuously manured with fourteen tons of farmyard manure per acre, and has yielded an average of $33\frac{3}{4}$ bushels during the last thirty-six years. The remaining three selected plots have been treated with artificial manures upon a uniform and undeviating plan, and have yielded on an average respectively $32\frac{3}{4}$, $36\frac{1}{2}$, and $36\frac{3}{4}$ bushels per acre taken also over a period of thirty-six years.

The mean average of all these five plots taken over this long period is $27\frac{5}{8}$ bushels, and it is this *mean* which corresponds with, or at least closely approximates to, the average yield of the United Kingdom. The average yield of these five selected plots for the present year is $27\frac{5}{8}$ bushels per acre, equal to $26\frac{5}{8}$ bushels when calculated as of 61 pounds each. The average yield arrived at on the same principle last year was $28\frac{3}{8}$ bushels per acre, showing a deficit this year as compared with last of $1\frac{1}{2}$ bushel per acre. Again, comparing the result obtained from the Rothamsted standard plots with what is considered the usual standard average of 28 bushels per acre, the deficiency for the present year would appear to amount to $1\frac{1}{8}$ bushel per acre only.

Sir John Lawes's general deduction that the selected plots at Rothamsted fairly represent in yield the average of the United Kingdom is certainly an assumption which might be objected to on scientific grounds. It is, however, as already pointed out, rather to be regarded as an ascertained fact than as a simple assumption, and from the evidence of a large number of years, the fluctuations of yield at Rothamsted may be regarded as a barometer, if we may so express it, of the parallel fluctuations throughout the United Kingdom.

Judged by the standard of the Rothamsted yields, the wheat crop of 1888 is only slightly below the received average of 28 bushels per acre.

It is well known to agriculturists that the harvest of 1879 was the worst which the present generation has witnessed, and during the dismal summer which has now ended, many persons expressed an opinion that the harvest of 1888 was likely to equal in badness that of 1879. This discouraging view has, however, happily been dispelled, and the harvest of 1888, although inferior in both quantity and quality to an average one, is not to be reckoned as disastrous.

One important feature of the harvest of 1888 is, however, its irregularity, and this has not only given rise to many conflicting opinions, but made it exceedingly difficult to arrive at the truth.

The opinion of Sir John Lawes is that upon farms where the condition of the land was defective, as well as upon all lands where there was an excess of artificial nitrogenous manure, there was less than the average produce; but that when the manurial conditions were more favourable there was more than an average produce. Thus the continuously unmanured plot yields only 10 bushels per acre instead of 13, the average of the preceding 36 years. The farmyard manure plot, on the other hand, yields 38 bushels, against an average of only $33\frac{3}{4}$ bushels. Lastly, the plot which receives an excessive amount of nitrogenous manures in the form of ammonia salts, as well as mineral substances, yields only $35\frac{1}{4}$ bushels, against its average of $36\frac{1}{2}$ bushels. The result thus generally indicated is supported by experiments made beyond the list of the usually selected plots, and is in these experiments still more pronounced.

The economic conclusion arrived at by Sir John Lawes after carefully passing this evidence in review is that "Taking the average population of the United Kingdom for the harvest year 1888–89 at rather over $37\frac{3}{4}$ millions, the estimated requirements for consumption, at 5·56 bushels per head, would be about $26\frac{5}{8}$ million quarters. The area under wheat is reported to have been 2,663,436 acres, or nearly 300,000 acres more than last year. This area, at $26\frac{5}{8}$ bushels per acre, would yield nearly 9 million quarters (8,947,480), and deducting 2 bushels per acre for seed, there would remain rather over $8\frac{1}{4}$ million quarters (8,281,621) available for consumption, and there would therefore be required about $18\frac{1}{4}$ million quarters (18,394,271) to be provided from stocks and import. It is admitted that the wheat crop not only of America, but of some other countries whence we derive supplies, will be below the average. But during the last two months of the past harvest year our imports were at the rate of 21 million quarters per annum, and there seems no reason to fear that there will be any difficulty in obtaining sufficient supplies."

ON THE INFLUENCE OF LIGHT UPON THE EXPLOSION OF NITROGEN IODIDE.[1]

THE statement of L. Gattermann in his recent paper (*Berichte d. deutsch. chem. Gesellsch.* xxi. 751; following up V. Meyer's paper in the same volume, p. 26) on nitrogen chloride, that its explosive decomposition may be brought about, or its susceptibility to explosion much increased, by exposure to bright light, has recalled to my mind the fact, which did not specially impress me at the time, that I myself undoubtedly observed the same relation several years ago in the case of nitrogen iodide.

In a paper on the preparation and composition of the latter substance, published in the first number of this *Journal* (April 1879), it was noted that on two occasions the product obtained with the composition NI_3 or N_2I_6 "exploded in some quantity *under water* with much violence and complete shattering of the vessel."

I remember distinctly that in one of these cases I had just carried to a window, through which the sun was shining, the beaker full of water at the bottom of which was the black sediment of iodide, and was gently stirring the liquid with a glass rod, holding the beaker up so as to look at it from below, when the rod touched the lower part of the side or the bottom of the vessel, and the explosion occurred.

In the other case the iodide was being washed with ice-cold water of ammonia, the vessel standing on a table exposed at the time to the direct rays of the sun. I do not remember with certainty what seemed to precipitate the explosion on this occasion, but I believe it was the pouring some fresh liquid, from the height of a few inches, on the black sediment of iodide which had just been partially drained by decantation.

Under ordinary circumstances nitrogen iodide, while *wet*, exhibits no extraordinary sensitiveness, and may be safely worked with, only becoming highly dangerous on drying, so that I have little doubt that bright sunshine was influential in bringing about these two explosions.

J. W. MALLET.

University of Virginia, May 8, 1888.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Dr. Hill, now Master of Downing College, having resigned the Demonstratorship of Anatomy, a senior demonstrator and two junior demonstrators at £100 and £50 stipends, and a University lecturer on advanced human anatomy are to be appointed, providing four teachers for the aggregate stipend, £250, formerly received by the Demonstrator. Starvation pay this, considering the limited opportunities in Cambridge for supplementing the income of an anatomist.

A grant of £80 from the Worts Fund is recommended to be made to Mr. M. C. Potter, to enable him to make botanical researches and to collect specimens in Ceylon during the coming winter.

The old Chemical Laboratory being now vacant, it is to be altered into a Pathological Laboratory for Prof. Roy.

The new scheme of examinerships in natural science was passed last week; the chief features being the appointment of two examiners each in elementary biology and chemistry, to take the 1st M.B., and the "specials" for the ordinary B.A. The stipends are rearranged, and in addition to a fixed amount a proportionate sum per candidate is allotted to the examiners, 5*s.* for Tripos candidates, 4*s.* for 2nd M.B. physiology and anatomy, and 2*s.* for the rest. Thus the examiners in anatomy and physiology, if 100 candidates

[1] Reprinted from the *American Chemical Journal*, vol. x. No. 4.

present themselves for the Tripos and 100 for 2nd M.B., will receive £75 each. The papers of all candidates in a subject are to be looked over by both examiners, who must be present at all oral examinations and at the final meeting of examiners.

The Harkness Scholarship in Geology and Palæontology, for women in their first or second term of residence, has been awarded to E. Macdonald, of Girton College.

H. F. Newall, M.A. of Trinity College, has been recognized as a teacher of physics, D. Carnegie, B.A. of Caius College, as a teacher of chemistry, and J. R. Vaizey, M.A. of Peterhouse, as a teacher of botany, for the purpose of giving certificates for M.B. degree.

At Jesus College, on December 11, there will be an examination for scholarships in natural science, the maximum value being £80. Notice must be given to the tutors before December 1. Chemistry is essential, and one of the following: physics, elementary biology, animal physiology. Christ's College examination will commence on the same date, and a candidate may be elected at either College.

At St. John's College the open scholarship examination on December 11 may include all the subjects of the Natural Sciences Tripos, but every candidate must show a competent knowledge of two of the following subjects: elementary physics, chemistry, and biology.

SCIENTIFIC SERIALS.

American Journal of Science, October.—On a young tortoise with two heads, by E. H. Harbour. An account is given of a two-headed *Chrysemys picta* recently found near New Haven, Connecticut, and presenting some interesting physiological features. They appear to be two independent organisms inclosed in a common carapace, with separate and even antagonistic instincts and impulses, as shown in their struggles to move in opposite directions, in their independent breathing, sleeping and feeding at different times, and so on. They were still alive and vigorous on September 4, fourteen weeks after capture.—The structure of Florida, by Lawrence C. Johnson. In this paper, which was read before the American Association for the Advancement of Science at New York last year, the peninsula is divided longitudinally into four regions plainly marked by surface indications: (1) the Gulf Hammock in the west; (2) a central plain, or region of sinks; (3) the High Hammocks, or lake region; (4) the eastern slope, draining to the St. John's River.—Analysis of a soil from Washington Territory, with some remarks on the utility of soil analysis, by Edward A. Schneider. The specimens here analyzed are from the Rockland Ridge near "The Dalles" on the Columbia River. From this study the author infers that the action of hydrochloric acid on soils is far from uniform; that plant roots probably derive their nutrition from the finest sediments of the soil; that hydrochloric acid powerfully corrodes both the finest and coarsest sediments; that fertility largely depends not only on the quantity of phosphoric acid, but also on the mode of its occurrence, and that consequently the fertility of a soil cannot be determined by chemical analysis alone.—On the Rosetown extension of the Cortlandt series, by J. F. Kemp. The discovery of this extension of the well-known Cortlandt series is accredited to Dr. N. L. Britton, and the Rosetown area, due west of Stony Point, is here definitely circumscribed.—The contact-metamorphism produced in the adjoining mica-schists and limestones by the massive rocks of the Cortlandt series near Peekskill, New York, by George H. Williams. In previous papers were described the principal types and some intermediate varieties forming the complicated group of this series. Here the author deals with the unusual contact-metamorphism which they have occasioned in the adjoining schists and limestones, concluding with a summary of the evidence in favour of the eruptive origin of the massive members of the series.—The sedentary habits of Platyceras, by C. R. Keyes. The sedentary habits of this group of Palæozoic Gastropods is inferred from the analogous habits of their modern congeners, and from their attachment to various species of Crinoids during life.—On edisonite, a fourth form of titanic acid, by W. E. Hidden. The specimen here described is from the Whistnant gold mine, Polk County, California. Its analysis shows it to be a nearly pure TiO_2, like rutile, but differing in its crystallization from the three previously known forms of that mineral.—On two new masses of meteoric iron, by George F. Kunz. The first of these specimens, from Linnville Mountain, North Carolina, closely resembles the Tazewell, Claiborne, and Bear Creek (Colorado) meteorites in composition; the second, from Laramie County, Wyoming, approaches nearer to those of Rowton, Charlotte, and Jewel Hill.—Experiments on the effect of magnetic force on the equipotential lines of an electric current (continued), by E. H. Hall. An account is here given of the author's experiments with cobalt, nickel, and bismuth, together with a summary of results.—W. Spring gives a further account of his views regarding the compression of powdered solids, in reply to Mr. Hallock; and E. S. Dana contributes a short preliminary notice of beryllonite, a new mineral so named by him from the fact that it contains the rare element beryllium.

THE *American Meteorological Journal* for September contains:—(1) An article by Prof. J. E. Curtis on suction anemometers. Two different forms of such instruments have been proposed, corresponding to two distinct ways in which a moving fluid produces a diminution of pressure. In the first the suction is produced by the wind blowing through a horizontal tube, having a contracted section; in the second the suction is produced in a vertical tube, by the wind blowing across its mouth. The second form alone has come into limited use, under the name of the Hagemann anemometer. The author points out that these instruments are not more generally used partly because there is a feeling of uncertainty as to the definite relation of the suction to the wind's velocity. The paper deals almost exclusively with their history and theory. (2) An account by Mrs. J. N. Brodhead of her experience of the great cyclone at Calcutta, on October 5, 1864. (3) An article by Prof. H. A. Hazen on the advantages of Mount Washington as a meteorological station. No individual station has had its observations discussed more thoroughly, and one of the most important investigations has been the use of the observations in determining a proper reduction of barometric readings at great altitudes to sea-level, by Lieut. Dunwoody.

Bulletin de l'Académie des Sciences de St. Pétersbourg, vol. xxxii. No. 3.—On the determination of constants of the ellipsoid of the earth by means of geodetical measurements, by A. Bonsdorff. This paper contains new formulæ for the calculation of the eccentricity.—On the formation of meteoric currents from the disintegration of comets, by Dr. C. Charlier, being a mathematical inquiry into the orbits of meteorites.—On the aberration of fixed stars, by M. Nyrén. After having calculated it on the ground of observations of two stars, the Comes and the Polaris, M. Nyrén obtains very nearly the same numerical values as those formerly found for the same stars by W. Struve.—On a new method for determining the focal distance of a system of lenses for different rays of light, by Dr. Hasselberg.—Some remarks on the fables of Phædrus, by A. Nauck.—A note by Dr. W. Radloff on grave-inscriptions in Semiryetchensk.—On the phenyl-angelic acid, by A. Gernet.—The approximate elements and ephemerides of Encke's comet for 1888, from May 12 to August 28, by O. Backlund and B. Seraphinoff.—The tale of the Princess Bentres compared with the tale of the Emperor Zenon and his two daughters, by Dr. O. Lemin. (All in German.)

No. 4.—Diagnoses of new Asiatic plants, by Dr. C. J. Maximowicz, being the seventh instalment (in Latin, with four plates) of a capital work about new plants brought by Przewalski, Potanin, Taschiro, and several others, from Central Asia, Japan, &c.—On the "hyperelementary" terms in the theory of perturbations, a mathematical inquiry, by O. Backlund (in German).

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 22.—M. Daubrée in the chair.—On lameness caused by pain, by M. Marey. By means of his photo-chronograph the author studies the character of the peculiar limping action instinctively caused by the desire to diminish the pain of a sore foot in walking. From the standpoint of the mechanical laws regulating the pressure of the foot on the ground, the three cases are considered in which this pressure is either equal to, greater, or less than, the weight of the body.—A paper follows by the same author, in which the swimming action of the eel is studied and illustrated by the same photo-chronographic process. The eel was 0·30 m. long, reduced by its squirming action to 0·29 m., and its rate of progress was shown to be 0·019 m. in 0·1 second, or about

0·19 m. per second.—Elements and ephemeris of Barnard's comet, by M. E. Viennet. The comet here in question was discovered on September 2, 1888, by Mr. Barnard, at the Lick Observatory, California, and noticed two days afterwards by Mr. Brooks, of the Geneva (U.S.) Observatory. The observations on which these elements are calculated were taken on September 5 and 18, and October 1, the first at Besançon, the two others at Hamburg.—On some errors affecting the observations of transits, by M. Gonnessiat. The sources of error here discussed are the magnitude of the stars on the one hand, and their position on the other. In the latter case, the absolute error is shown to decrease rapidly to within about 2° of the Pole, after which it becomes pretty constant. Hence in determining the instrumental constants, the stars nearest the Pole should be preferred.—Reflected images on the spheroidal surface of the Lake of Geneva, by M. F. A. Forel. M. Riccò's recent note (*Comptes rendus*, cviii. p. 590) showed the deformation of the image of the sun reflected by the spheroidal marine surface. The observations now taken by M. Forel on the Lake of Geneva fully confirm the interpretation of the Sicilian astronomer. Attention is called to the fact that the theoretical demonstration of the probability of such deformations was first given by M. Ch. Dufour, of Morges. This new demontration of the rotundity of the globe is now no longer theoretical, but is borne out by the direct observation of the phenomenon.—On the intersection of two algebraic curves in a single point, by M. G. B. Guccia. Several geometricians have long been engaged on the inquiry into the number I. of the intersections of two curves, $\phi = 0$, $\psi = 0$, merged in a single point, P. Prof. Cayley and M. Halphen have given general solutions of this extremely delicate problem, and M. Guccia has now been incidentally led to a new general expression of the number I., which presents considerable interest thanks to its great simplicity as well as the numerous and easy applications of which it is capable.—On the combination of benzoic aldehyde with the polyatomic alcohols, by M. Maquenne. In a recent note on the valency of perseite the author described, under the name of *dibenzoic acetal of perseite*, a new compound analogous to that obtained by M. Meunier with mannite and benzoic aldehyde. He now shows that in the acetals derived from a polyvalent alcohol each molecule of aldehyde necessarily saturates two alcoholic functions. If the number of the latter is odd, the aldehyde will always leave at least one free, whence it results that the elementary composition of these acetals, in passing from any polyatomic alcohol to its next homologue, differs sufficiently for them to be at once distinguished by analysis. Here is therefore a new means of determining whether an alcohol is of odd or even atomicity.—Action of hypophosphorous acid on benzoic aldehyde; formation of a dioxyphosphinic acid, by M. J. Ville. M. W. Fossek has obtained acid crystallized products corresponding to the general formula—

$$R—CH,OH—PO(OH)_2,$$

which he calls oxyphosphinic acids. But no chemist had hitherto determined the existence of dioxyphosphinic acids. M. Ville, however, has now obtained a compound belonging to this new class of acids. The process, as here described, consists in making hypophosphorous acid act on benzoic aldehyde. It may be designated by the name of dioxybenzylene-phosphinic acid, its constitution being expressed by the formula—

$$\begin{array}{c} C_6H_5—CH\ .\ OH \\ | \\ PO\ .\ OH \\ | \\ C_6H_5—CH\ .\ OH. \end{array}$$

—M. G. Denigès describes the action of the hypobromite of soda on some aromatic nitrogenous derivatives, and the differential reaction between the hippuric and benzoic acids.

BERLIN.

Physical Society, October 19.—Prof. von Helmholtz, President, in the chair.—Dr. König gave an account of experiments which he had made with Ottomar Anschütz on the instantaneous photography of projectiles. After exhibiting and explaining the instantaneous photographs which Anschütz had made during the last few months, such as those of the funeral procession of the late Emperor Frederick, of episodes at the manœuvres, of wild beasts at the Zoological Gardens in Breslau, of the several positions of a soldier marching on parade, and of a lady dancing, he described the arrangements necessary for photographing a cannon-ball travelling at the rate of 400 metres per second. The cannon-ball was projected in front of a white screen illuminated by direct sunlight, occupying in its passage $\frac{1}{75}$ second: during this time four negatives were taken. The firing of the cannon, the momentary exposure of the plate, and the recording of time on the chronograph were provided for by electric currents. The experiments were made at Magdeburg at the Grüson rampart, and had to be completed in one day. Only one successful picture of the projectile was obtained, but the possibility of such experiments, and of the accurate determination of the several time intervals, were sufficiently indicated.—Dr. Budde spoke on the mechanics of forces acting on rigid bodies. As one outcome of this address may be mentioned a proposal of Dr. Budde's with respect to the nomenclature of conjugate forces. Ordinarily, of two conjugate forces only the second one is spoken of as conjugate, while no special name is given to the first. The speaker therefore proposed to call the first of two conjugate forces "male" and the second "female," and introduced this nomenclature into his address with very marked furtherance of its clearness.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Studies from the Morphological Laboratory in the University of Cambridge, vol. iv. Parts 1 and 2 (C. J. Clay).—Life of Sir William Siemens: Wm. Pole (Murray).—Die Gletscher der Ostalpen: Dr. E. Richter (Englehorn, Stuttgart).—Force and Energy: Grant Allen (Longmans).—Theoretical Mechanics: J. E. Taylor (Longmans).—Elementary Theory of the Tides: T. K. Abbott (Longmans).—Elemente der Paläontologie, 1 Hälfte: Dr. G. Steinmann and Dr. L. Döderlein (Engelmann, Leipzig).—*Challenger* Report, Zoology, vol. xxvii. (Eyre and Spottiswoode).—The Book of the Lantern: T. C. Hepworth (Wyman).—The Gold Fields of Victoria; Reports of the Commissioners for Quarter ended June 30, 1888 (Sydney).—The British Farmer and his Competitors: W. E. Bear (Cassell).

CONTENTS.

THURSDAY, NOVEMBER 8, 1888.

THE PREVENTION OF SMOKE.

THE people of London have lately experienced much inconvenience and discomfort from the dismal fogs which so often make their appearance at this season of the year. Considerable interest, therefore, attaches to the excellent lecture, by Mr. Rollo Russell, on "Smoke in Relation to Fogs in London," of which we to-day print an abstract. The importance of the subject no one will dispute, yet the questions connected with it have hitherto received very inadequate attention. That much might be done to purify the atmosphere, not only of London, but of all our great towns, is certain; what is needed is simply that the matter shall be taken in hand in earnest by the Legislature. The existing Acts of Parliament to abate the nuisance arising from the smoke of furnaces in the capital are efficient so far as they go. Inventors have produced mechanical stokers and other means of feeding furnaces, which have resulted not only in the prevention of smoke from such furnaces, but also in commercial advantages to their users, by reason of the increased efficiency and reduced consumption of the furnaces. Police-inspectors appointed to watch the chimneys of manufacturers' premises have done most useful work in preventing the emission of smoke, and London is to be congratulated upon the freedom from smoke from such sources. Legislation, however, has not gone far enough even in controlling the emission of smoke from furnaces, for in the provinces there is very great negligence. In some places by-laws exist, in other places there are none; but wherever they are enforced it is invariably the case that the fines imposed are too small, and that the real offenders, the manufacturers and users of the furnaces, do not adequately feel the effect of the penalties in which they are mulcted.

The careful observer in London will find that the nuisance from which we suffer does not arise from factory or other industrial chimneys, but from the millions of domestic chimneys. Why should legislation apply only to the comparatively small number of industrial furnaces in London, and leave the multitude of hotel and private fire-places to emit dense volumes of smoke? In a letter addressed some time ago to the *Times*, Mr. Alfred E. Fletcher clearly demonstrated the practicability of having open fire-places in well-warmed and thoroughly ventilated houses without smoke, but there is no law to enforce the application of such a system. To those who have not read Mr. Fletcher's letter, the apparatus may be briefly described. It consists of a coke stove in the basement, a flue to discharge products of combustion, an air-pipe drawing in air around the stove and discharging it when warmed through a grating on the ground-floor. The effect of this simple apparatus is such that warm, but not scorched, pure air ascends up the staircases, into the rooms, passes up the chimneys, and out at the windows, without creating draughts of any kind. The open fire-places in all the rooms may be maintained, but, to insure their smokelessness, they should either have one or other of the systems of under-feeding coal grates, or have incandescent blocks heated by gas. The duty of the fire-place is reduced to practically nothing, as the whole house is warmed by pure air from the heating chamber of the coke stove, and the bright open fire need only be maintained to satisfy English prejudice and custom.

This is one system which has come under our notice, and others might be mentioned. But one fact is enough—viz. that ordinary London houses can be thoroughly well warmed with pure air, well ventilated by known appliances, and with this knowledge the public ought to demand immediate legislative action. There is evidence that the necessity of legislation is beginning to be understood, for the House of Lords was some time ago approached by Lord Stratheden and Campbell with a Bill "to amend the Acts for abating the nuisance arising from the smoke of furnaces and *fire-places* within the metropolis;" and, although this Bill did not pass into law, its importance was such that a Select Committee was appointed to consider its terms and to report to the House of Lords. Mr. Ernest Hart gave many thoroughly practical suggestions for the prevention of smoke from domestic fire-places in his evidence before the Select Committee; and other evidence from scientific, legal, and police authorities was adduced. But at the time the public interest in the question was insufficient to secure the passing of the measure.

If members of the Houses of Lords and Commons would seriously consider the dangers to life, the inconvenience, the loss, the injury to household effects, and other disadvantages which the presence of smoke and other impurities in the air occasion, and would turn their attention to the very deficient legislative measures now existing, a speedy remedy would no doubt be effected. The necessity for the universal adoption of smoke-preventing appliances would probably bring out much latent inventive talent from the public. By reference to the Report of the Council of the National Smoke Abatement Institution, we find that inventors and manufacturers are continually introducing better means of consuming fuel. The greatest improvement is in apparatus for industrial furnaces, because it is with these appliances only that Acts of Parliament deal. As compared with some tests made by the Smoke Abatement Institution in 1881–82, tests made in 1886–87 show an apparent economy of 31 per cent. in consumption of fuel combined with complete smoke prevention. In the Transactions of the Sanitary Institute of Great Britain—in a paper on smoke abatement, read by Mr. Russell Duncan at the Bolton Congress—we find that inventors are working in the right direction, and that during the last ten years over 4200 patents have been taken out for various appliances having for their object the prevention of smoke and the more complete combustion of fuel.

An examination of the work done by the National Smoke Abatement Institution will show that if legislative measures could be carried it would not be necessary to restrict the use of one kind of fuel in favour of another, but that by means of suitable appliances houses might be heated by ordinary fuels—coal (bituminous and non-bituminous), coke, oil, and gas, or by improved systems of circulating warmed air and heated water.

SOME RECENT MATHEMATICAL BOOKS.

Euclid. Part 1, Books I. and II. By H. S. Hall and F. H. Stephens. (Macmillan, 1887.)

Algebraical Exercises. By H. S. Hall and S. R. Knight. (Macmillan, 1887.)

Key to Todhunter's Mensuration. By Rev. L. McCarthy. (Macmillan, 1886.)

Explanatory Arithmetic. By G. E. Spickernell. Third Edition. (Simpkin, 1887.)

Plane and Spherical Trigonometry. By H. B. Goodwin. (Longmans, 1886.)

Spherical Trigonometry. Part 2. By W. J. McClelland and T. Preston. (Macmillan, 1886.)

Solid Geometry: Solutions. By P. Frost. (Macmillan, 1887.)

Elementary Statics. By J. Greaves. (Macmillan, 1886.)

Differential Calculus. By B. Williamson. Sixth Edition. (Longmans, 1887.)

Differential Calculus. By J. Edwards. (Macmillan, 1886.)

Algebra. By Oliver, Wait, and Jones. (Ithaca: Finch, 1887.)

Practical Solid Geometry. By W. G. Ross. (Cassell, 1887.)

IF former periods of the world's history were characterized as the "Stone," "Bronze," and "Iron" Ages, the present epoch might well be entitled the "Book" Age. Amidst the flood of literature of all sorts which daily pours out of the jaws of our printing leviathans, didactic mathematics certainly claims its due proportion. The number of Algebras, Euclids, Arithmetics, and Trigonometries which appear, with the "rough ways made smooth and the crooked ways straight," make us regard the modern student with a mixture of envy and pity—envy at the possession of such broad highways to knowledge as we never dreamt of, and pity at the difficulty he must experience in choosing out of such a multitude. We cannot, moreover, help fancying that this plethora of books is not entirely without compensating disadvantages, for the very ease and tranquillity which which the student glides through the cleared forest, make him careless and inattentive of the land-marks and salient features that were so carefully noted by his more self-reliant, if less luxuriously-equipped, predecessor.

Of the twelve books on our table, we shall begin by noticing the most elementary. Among these is an instalment comprising Books I. and II. of "Euclid," by Messrs. Hall and Stevens, of Clifton College. For these revolutionary days it is remarkably orthodox; but certain changes have been introduced, very wisely as we think, where Euclid's enunciations were confusing, or the proofs were not sufficiently comprehensive, as in Propositions 8 and 26, where the identical equality of the two triangles is not usually emphasized. The authors, in their preface, enter very fully into the reasons which decided them to avoid the use of symbols at first, and also to preserve "the formal, if somewhat cumbrous, methods of Euclid," and with these reasons we in the main agree. If, however, for "a large majority of students 'Euclid' is intended to serve, not so much as a first lesson in mathematical reasoning, as the model of formal and rigid argument which most conduces to accurate and orderly thought in any field of study," we should welcome a book of geometry brought out for the use of those whose natural mathematical growth is stunted, and taste warped, by a too strict adherence to the cumbersome and often involved style of the ordinary text.

We admit the dilemma for those who wish to make "Euclid" serve the double purpose of an introduction to logic as well as geometry, but, at the same time, we are unable to see why, even for the logicians, *some* at least of the advantages of the German method cannot be introduced, such as the use of a, b, c for the sides, and α, β, γ for the angles of a triangle. A great deal of confusion arises from the use of *three* letters for an angle and *two* for a side, and the change to the simpler method would not only clear the student's mental vision, but leave the logic unimpaired.

One of the best examples of the defects arising from a rigid adhesion to the formal text is in Book I., Proposition 13. We have found many to whom this proposition in its existing form is one of the most repulsive in the book, and it has been almost touching to witness the joy evinced by a dullard on his first realizing that the obtuse angle was just as much in excess, as the acute angle was in defect of a right angle—a fact which the ordinary proof completely disguises. We do not think the alternative proof to Proposition 47 is likely to meet with much favour, and since the authors do not altogether discard alternative proofs, we should have preferred, instead, the neat alternative to Proposition 48 given in Casey.

For the purpose for which it is designed we do not hesitate to recommend the book. It is excellently printed, the construction lines being very properly faint, and the figures in all cases clearly drawn. The exercises, additional theorems, and hints to solution are also unusually well arranged, and will be truly welcome to the student who intends to go in for mathematics, as well as train his mind into logical habits.

"Algebraical Exercises," by Messrs. Hall and Knight, is, we presume, intended to be a companion and supplement to their excellent little "Elementary Algebra," which has met with such a generally good reception. While these exercises will, no doubt, be of considerable use, we think they might be improved, and rendered more widely serviceable, (1) if Part 1, devoted to the earliest rules, were extended beyond a meagre six pages—a range altogether out of proportion to what follows; and (2) if the exercises were more *gently* graduated in Part 2. For example, linear equations in three unknowns are introduced *per saltum* as early as p. 10, and all the papers after p. 6 strike us as being a good deal harder than those encountered by the Army Preliminary candidate, types of which are given on pp. 146–47. We also regret to find in the first ninety-seven pages an almost entire absence of book-work questions—a defect which is only partially made up for in the capital selection of typical public examination papers which follow. Example-grinding is no doubt an essential art, but in algebra, especially, the early parts tend to become purely mechanical, unless *real thinking* is encouraged and stimulated by rational questions on the processes employed. In a subsequent edition a few more recent University and Army papers would form a welcome addition.

Mensuration is represented by a key to Todhunter's small treatise, by the Rev. Lawrence McCarthy, of St. Peter's College, Agra. As a rule we have no great liking for keys, but if there is any country where such a key might be used with advantage, it is India. Possibly this is what prompted Prof. McCarthy to perform a labour which might fitly be termed an intellectual treadmill. We have always looked upon the "Mensuration" itself as Todhunter's least valuable work. It is full of long-winded rules which are never learnt, and the use of algebraic symbols by which all such rules could at once be rendered visible to the eye, is most curiously, and, as we think, unreasonably, avoided. The same spirit characterizes the key. Everything is worked out at full length with needless repetitions of figures, especially the oft-recurring 3·1416, which we have counted no less than twenty-nine and thirty-four times respectively on two pages chosen at random. Surely the symbol π might have been substituted with advantage here. Barring such defects, the work appears to have been well done, and will doubtless be of use, more especially to those who are unable to get tutorial assistance.

An "Explanatory Arithmetic," by Mr. G. E. Spickernell, which has reached a third edition, does not strike us as any improvement on existing treatises. Brevity, which is one of the points aimed at, is certainly secured, but at the expense of both elegance and lucidity. The rules read like excised telegrams, and are liable to be misconstrued in much the same way. Thus, to take an example of the telegram purporting to explain compound subtraction: "Take *like from like*; and whenever it is necessary, in order to make subtraction possible"; and a longer one for the subtraction of fractions runs thus: "Reduce minuend and subtrahend to equivalent fractions having their least common denominator; and then, having like parts of integers, take less number from greater, and write in figures, under remaining parts, their name." Similar highly cacophonous and ambiguous paragraphs are to be found scattered through the book, and give one the impression that they will frequently necessitate as much explanation as the principles they are intended to embody. Occasionally the author employs a definition which is palpably partial, thus: "When an integer or whole thing is divided into a number of *equal* parts, those parts are called fractions."

The entire book strikes us as being of the empirical cramming style, as opposed to the rational and scientific style so well exemplified in Brook Smith's treatise and the smaller one by Lock, in which rules are avoided as much as possible. On the other hand, it contains copious and very well assorted collections of examples and examination papers, with answers which can be readily removed from it if desired. These might be used with advantage, and the teacher, if a good one, could translate and expand the telegrams into a more rational and elegant form, or, still better, do without them.

Trigonometry, plane and spherical, is represented by two books—one, comprising the elements of both subjects, chiefly for the use of junior naval officers, by H. B. Goodwin, Naval Instructor; while the other is Part 2 of a "Treatise on Higher Spherical Trigonometry and Geometry," by Messrs. McClelland and Preston, of Dublin. Mr. Goodwin's work is intended to give, in one volume, the course usually required for an acting sub-lieutenant—which heretofore he has had to pick out of a variety of elementary works—and appears to admirably fulfil its author's intention. It is marked by simplicity of treatment, the avoidance of cumbrous rules, those bugbears of our ancient text-books, and a separation of the subject into distinct parts, each of which is complete in itself.

Messrs. McClelland and Preston's book is a new departure, in so far as, with the exception of the well-known treatise of Mulcahy, it is the first time that spherical geometry, as distinct from trigonometry, has been seriously put into text-book form. The authors are to be congratulated on their bold, clear, and systematic treatment of this too-much-neglected and really useful branch of mathematics. The work throughout is characterized by lucidity and originality of treatment, and is subdivided into complete chapters. Spherical and stereographic projection are also carefully explained, and their power as methods amply exhibited.

We cannot help thinking that both spherical trigonometry and geometry are far too much neglected in our educational curricula. In consequence, it is astonishing what errors are committed when even the simplest properties of a spherical surface are in question. The curvature of the earth is realized by few, and some who ought to know better have not yet grasped the fact that the latitude of Cairo approximately bisects the area of the northern hemisphere. Nothing but polar projections, or, better still, globes themselves, will ever correct the false impressions which we get from that terrible flat-ruled distortion entitled "Mercator's projection," from which all approach to curvature has been so carefully extracted.

The theory of the trade-winds, moreover, which has survived up to date in some of the text-books, takes no account of the shortening of the radius in considering the gain in eastward motion by the transference of the air at relative rest on the equator to higher latitudes. Thus, according to Loomis's "Meteorology," the gain is simply found by subtracting the linear velocity at the higher latitude from that at the equator; whereas when the shortened radius is considered, it amounts, in latitude 60°, to 1554 instead of 518 miles per hour, and at the Pole itself to ∞ instead of 1036 miles per hour. These are only a few of the most patent errors which arise from a neglect of spherical principles, and might be multiplied almost indefinitely.

"Hints for the Solution of Problems in Solid Geometry," by Dr. Percival Frost, is a book which cannot fail to be of great value to the student of this difficult but important branch of mathematics. Mathematical solutions have little analogy to, and, except in elementary works, none of the disadvantages of, classical cribs. In the present case, the execution of Dr. Frost's truly laborious work has been attended by an unexpected advantage, in leading to the discovery of certain errors and omissions in the statement of the problems themselves, which might otherwise have escaped notice. We heartily welcome Dr. Frost's hints, and trust they may receive the attention they so fully deserve.

While some branches of elementary mathematics are already in danger of being congested by a plethora of text-books, statics and dynamics seem to us to still pre-

sent an open field for the writers of really able didactic treatises. For those who have reached the Elysian fields of the calculus, no better book can be recommended than Prof. Minchin's admirable work; while for those who have not yet reached that stage, and perhaps never intend to, the "Elementary Statics" by Mr. John Greaves, which is written much on the same lines, can be unreservedly recommended. The science of statics, like everything else, has been obliged to *move* with the times, and Mr. Greaves, following the modern views, prefers to consider it as merely a particular case of the science of dynamics, and to base it upon the laws of motion. Thus, instead of the familiar proof of the parallelogram of forces on the principle of transmissibility of force, he deduces it solely by the aid of the parallelogram of velocities, from which, together with the third law of motion, the conditions of equilibrium are obtained more readily, and, in the author's opinion, more clearly, than usual. This expectation can only be tested by actual experience. Meanwhile we would recommend that, in a reprint of the book, the more salient propositions and results should be rendered more emphatic and conspicuous by being placed either in italics or large type. In their present form and position the plums are too disguised to be readily picked out. The work is characterized by thoroughness, and by a large number of worked-out examples illustrated by excellent figures, the material lines being very properly distinguished from the geometrical and force lines by thicker type. The free use made by the author of the purely geometrical method for solving some statical problems is elegant, but occasionally leads to a neglect of the statical or primary limitations under which they are stated. An example of this occurs on p. 86, where it is required to find the greatest inclination to the horizon at which a uniform rod can rest partly within and *partly without* a fixed smooth hemispherical bowl. The condition assumed for the maximum inclination leaves *no part* of the rod outside the bowl, which clearly violates the latter part of the question.

Machines are deferred to the last chapter in the book, presumably because some of the principles, such as virtual work, are dealt with in preceding chapters; but we think they might be advantageously introduced, at all events in a preliminary way, much earlier, since their consideration not only enlivens the otherwise dry discussion of abstract principles, but gives concrete expression to their reception. We are glad to see that the merits of this excellent little book are recognized by the authorities of the Mason Science College, who recommend it for one of their courses.

At the threshold of the higher mathematics we find two books on "Differential Calculus," which, though rivals, will, we trust, often be found in company, since each possesses certain merits and characteristics which distinguish it from the other. One is the well-known and excellent treatise by Prof. Benjamin Williamson, F.R.S., which has now reached a sixth edition. In this edition, besides careful revision, a short discussion is added on the elementary properties of solid and spherical harmonics, which are so frequently employed in the higher developments of electrical and optical theories. As a former edition of the book has been fully noticed in NATURE, we need only indorse the opinion then put forward, that it is one of the best treatises on the subject in our language. The other work, by Mr. J. Edwards, formerly Fellow of Sidney-Sussex College, Cambridge, is very different in style, and more elementary, in so far as it is, according to the author's design, "unencumbered by such parts of the subject as are not usually read in Colleges and schools." Compared with Prof. Williamson's treatise, it is distinctly more geometrical in method, and in this and some other points, such as large type, beautifully-drawn figures, an unusually full and systematic account of curve-tracing and the properties of curves, which, contrary to the usual custom, *precede* maxima and minima, it is more suited to the wants of the average student as a preliminary course of reading. Some of the geometrical illustrations, such as those of the compressed form of Taylor's theorem, $\phi(x + h) = \phi(x) + h\phi'(x + \theta h)$, and $Dz = \frac{dz}{dx}dx + \frac{dz}{dy}dy$, are very elegant, and help to keep alive the real meaning of differential symbols, which a too exclusive attention to algebraic analysis tends to annihilate. Symmetry and brevity have both been evidently studied, and a good example of this may be seen on p. 271, where the radius of curvature for an implicit function of x and y is deduced. If this be compared with the analogous method on p. 290 of Williamson's book, the difference in the style will be manifest. Regarding the two books together, we should advise a student to begin with Edwards, and then proceed with Williamson. Nothing in the former work need be omitted at a first reading, after which he may plunge fearlessly into the more complete and analytical treatise of the Dublin Professor.

Two books remain to be noticed, which lie somewhat outside the ordinary run of didactic works. One is an Algebra by Profs. Oliver, Wait, and Jones, of Cornell University, U.S. This, though originally intended as a text-book for their own students, seems, in the course of construction, to have developed into a work which, while it might be found really useful as a book of reference to teachers and the rare youth who *cultivates* mathematics, is quite unsuited to the ordinary student.

In some respects it appears to be an effort to regard algebra from the modern point of view as the science of finite operations, and to present it in the form of "a stepping-stone to the higher analysis," and there is much that is commendable from this point of view in the exposition of incommensurables, limits, imaginaries, derivatives, and graphic representation of equations, as also in the introduction of some fresh symbols, such as the Gaussian sign of congruence, $\equiv$, and $\gtrdot$ $\lessdot$ for smaller than and greater than in the sense of size only. The use of the signs $+ -$ as left-hand indices to indicate absolutely positive and negative quantities is also an improvement, and renders it easier to deal with negative and directional quantities. For English didactic purposes, however, this book will be chiefly useful as one of reference for the teacher.

In conclusion, we must not omit to draw attention to a very handy little manual of "Practical Solid Geometry," by Major Gordon Ross, of the Royal Military Academy, Woolwich, which is particularly adapted to military students. The method of orthographic projection by

"vertical indices" receives special attention, and a section on defilade—a subject not much studied in this country—cannot fail to be of use and interest to those who study the science of war. E. D. A.

OUR BOOK SHELF.

Examples in Physics. By D. E. Jones, B.Sc., Lecturer in Physics at the University College of Wales, Aberystwyth. (London and New York: Macmillan and Co., 1888.)

So many books have been written having titles similar to if not identical with that quoted above, the only object of which seems to have been to enable students to pass certain examinations with the minimum of knowledge, that it is a comfort to turn to one against which no such charge can be made. Mr. Jones's "Examples in Physics" has not been written "up to" any Syllabus, but the author has made use of portions of the manuscript in teaching classes of students taking the intermediate science and preliminary scientific courses of the London University, and he believes it will be found useful for students who are preparing for these examinations. There can be no doubt that the book will be of great assistance in this way, owing to the large number of examples and the excellent way in which they have been graduated. In addition to the examples, of which there are more than a thousand, with occasional hints for their solution, there are short explanatory chapters and paragraphs where experience has shown that they are needed. Thus, at the beginning, the C.G.S. units are thoroughly explained, as is the method of passing from one system of units to another by means of dimensional equations. Those approximate relations which are most often made use of are shown to be true, and examples illustrating the advantage of employing them are worked out. The method of using logarithms is explained, and both on pp. 19 and 21 the reader is told that there is a table of four-place logarithms at the end of the book. There is a page on which natural sines and tangents are given to three places, but not a vestige of a logarithm is to be found.

In the chapters on dynamics, hydro-statics, heat, light, sound, electricity, and magnetism, chapters which consist essentially of examples, there are clearly-written paragraphs explaining those points that do not generally seem to be grasped by students. The answers to the questions are given at the end.

The general arrangement of the book is particularly happy; it is clearly the work of a teacher whose object is to increase the real knowledge of his students, and not merely to drive them through the ordeal of an examination.

The Constants of Nature. By Frank W. Clarke. Part I. New Edition. (Washington: Published by the Smithsonian Institution, 1888.)

This volume consists of a series of tables of specific gravities of solids and liquids, and differs from the older edition in two respects. In the first place, the tables have been revised and greatly enlarged; and, secondly, melting and boiling points have been omitted, on the ground that they are already supplied by the two volumes by Prof. Carnelley, which are specially devoted to those data. How much the tables have been enlarged may be gathered from the fact that the older edition, with a later supplement, only gave 2963 substances, whereas there are now no less than 5227 distinct substances mentioned, and 14,465 separate determinations. As the author remarks, this increase is a noteworthy indication of chemical activity.

The tables are only intended to be complete as far as artificial substances of definite constitution are concerned, but, in addition to these, many minerals find places. For each substance, the formula, specific gravities, and authorities are stated. The elements take the first place, and these are arranged in order of densities. Then follow inorganic fluorides, chlorides, bromides, iodides, oxides, sulphides, &c., the various groups of organic bodies coming last. There is a very complete index to the names of substances, without which, of course, the book would be far from complete.

The author is to be congratulated on the successful completion of an undertaking entailing such a vast amount of patient labour.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Gresham College.

I trust you will allow me space for a short note upon Prof. Lankester's article in last week's issue of NATURE (p. 1), under the above heading. I have no intention of entering into a controversy with Prof. Lankester upon matters of opinion, but I am desirous of correcting statements, made by him in that article, which are inaccurate. In point of fact, the article is based entirely upon a misapprehension as to the purpose and function of the London Society for the Extension of University Teaching, and its position with regard to Gresham College. Prof. Lankester speaks of it as a Lecture Society, and refers to the "innumerable short courses" of lectures given in different halls, Vestry Halls, and others in London. He is evidently here confusing two distinct things. The short courses in large halls are given under the People's Lectures Scheme, which is an entirely different matter, and in connection with which we never use the word "University." The courses of lectures and classes carried on by the London Society and the Universities Joint Board involve a systematic course of work extending over a full term of ten or twelve weeks. It is a part of the same great movement which is carried on by the University of Cambridge (not by a Lecture Society), and more recently also by the University of Oxford, in different parts of the country.

Those who have had an insight into the methods and the result of the working of the University Extension movement have been struck with the excellence and thoroughness of the work done. Sir James Paget, in his annual address to students at the Mansion House, last February, spoke as follows with regard to some of the syllabuses:—

"As I looked through the syllabuses of such subjects as I can estimate, I could see that the amount of teaching in each of them is enough for a good beginning for some who may intend to make that subject a chief study for their lives, and enough to form an important part in the teaching of anyone who wishes to be in the fairest sense generally well educated."

In fact, the principles which underlie the University Extension movement cannot but meet with the approval of all interested in higher education. These principles are, "first, that the amplest facilities for the best kind of higher teaching, such as the Universities provide, should be brought within the reach of the great mass of the people by means of courses of instruction, given locally at convenient times and places; and, secondly, that a teaching system, as opposed to a mere examination system, is required to meet the educational needs of the time."

In his speech at Gresham College, Mr. Goschen made no claim that "Gresham's money should be assigned to the support of the lecturers of the Society." What he did point out was that the design of Sir Thomas Gresham was to establish, in the heart of London, University teaching for busy people engaged in the City, and that the aim of the University Extension movement, whether in London or the country, was in spirit the same.

Prof. Lankester charges this Society with making an objectionable use of the word "University" in order to gain financial support. He says, "the implication is that the teaching is such as is given at the Universities, and it is an entirely false implica-

tion." This implication, which Prof. Lankester calls false, we do assuredly make in the most explicit way. We assert that the teaching is deserving of the title University teaching, and I am prepared to submit to Prof. Lankester overwhelming evidence in support of this assertion.

The definition which he gives to University teaching is one which includes all the good schools, and is in no sense diagnostic of University teaching. His first condition our lecturers completely satisfy, for they have, as a rule, been men of the highest academical position, in every way the equal of those who fill ordinary College Professorships and Lectureships.

We contend that the essential characteristic of University teaching is the *method* employed in dealing with a subject; that the teaching of the Universities is directed to the elucidation of the principles of the subject taught, and to the end of bringing all the mental faculties of the student into play, so that he may be placed on the high road to pursue his studies in their higher developments; and that an important factor in producing these results is the personal intercourse between teacher and student. We assert that the University Extension method possesses these characteristics.

Our idea is that Gresham College would best fulfil its founder's intention if it were enlarged into a great central College, with permanent Professorships and all the facilities for laboratory work; the Professors, however, teaching in the evening instead of the day, so as to provide for the needs of the class which Sir Thomas Gresham intended to benefit, and for which the London Society is, in a tentative way, attempting to make imperfect provision. R. D. ROBERTS.

Charterhouse, E.C., November 5.

I CANNOT agree with Mr. Roberts that I have written under any important misapprehension of the nature and general objects of the organization of which he is secretary. I have expressed my conviction of the excellence of the courses of lectures given through its means. I object to its profession of bringing "University" teaching into London, and to its claim to represent either the University of the future or Gresham's University of the past. The fact that the Universities of Oxford and Cambridge and London have appointed members of a Committee to arrange lectures in London does not, in my opinion, constitute those lectures as parts of the teaching of those Universities, and the suggestion that this is the case—encouraged by the use of the term "University Extension"—is, in my opinion, greatly to be regretted. It is, perhaps, difficult to be sure as to the nature of the audiences contemplated by Gresham for his Professors. But supposing that his intentions could be realized in this special point by the delivery of lectures in the evening, I am at a loss to understand what public good can be served by the introduction of a new organization into London for the purpose of giving such lectures, when there are already two public institutions—viz. King's College and University College—which are not only ready to undertake such teaching if found desirable in the future, but have actually carried on such teaching in the past. The Professors of King's and University Colleges are University graduates, they are provided with laboratories and libraries and lecture-rooms, they have numbered among them some of the most distinguished scholars and *savants* of the day, and they have produced both trained investigators and large additions to existing knowledge. They only require additional endowment and public sympathy to fulfil in every respect the ideal of a true University in and for London. Yet a certain number of gentlemen connected with Oxford and Cambridge have persuaded those Universities to nominate a Committee called a Joint Board to kindly undertake the introduction of "University" teaching into London. I cannot believe myself that this new body, competing for the support of Londoners as representing the great educational want of London, viz. a real University, will fail to do harm by dividing the support which London can give to University institutions. I cannot suppose—after observation of their proceedings—that those who form the active body of the London Society for the Extension of University Teaching are as anxious to promote a true University in London as they are to find employment for their lecturers. This is quite natural, and, if admitted, is not otherwise than creditable; but the assertion of a claim to be representatives of University teaching in London on the part of these gentlemen is not so creditable.

November 6. E. RAY LANKESTER.

The Barbary Ape in Algeria.

IT may interest your readers to know that monkeys are still to be found wild at a place within three days' journey of London, and easily accessible to the most unenterprising traveller. Yesterday, in company with my son, I drove up the gorge of the Chifa, on the excellent main road between this place and Mediah. We halted at the spot where the appropriately named "*Ruisseau des Singes*" falls into the Chifa on its left bank, and ascended the narrow side-valley on foot. Its steep slopes are densely covered with brushwood, intermixed with a few oaks and stunted junipers. We had not proceeded more than ten minutes from the main road before we heard the chatter of a Barbary ape on the bank above us, and saw him scrambling along the rocks. Shortly afterwards, a fine large male of the same species was kind enough to mount a juniper-tree on the opposite side of the gorge to that on which we were seated, and exhibited himself to our gaze for at least fifteen minutes. His various attitudes were distinctly observable through a pair of opera-glasses, and we calculated his distance from us as not more than 400 yards in a straight line. A third ape was subsequently met with farther up the gorge, at a much nearer distance, but did not wait to be looked at.

I had previously seen Barbary apes on the Rock of Gibraltar, but they are there in a semi-protected condition, and perhaps introduced. In the gorge of the Chifa they are quite in a "state of nature," and in their native wilds. P. L. SCLATER.

Hôtel d'Orient, Blidah, Algeria, October 29.

Are there Negritos in Celebes?

PROF. FLOWER, in his interesting lecture on "The Pygmy Races of Men" (Journ. Anthr. Inst. vol. xviii. p. 82, 1888, and NATURE, vol. xxxviii. p. 67), after having spoken of the Negritos in the Philippines, says, apparently on the authority of Quatrefages: "As the islands of these eastern seas have become better known, further discoveries of the existence of a small Negroid population have been made in Formosa, in the interior of Borneo, Sandalwood Island (Sumba), Xulla, Bouron, Ceram, Flores, Solor, Lomblem, Pantar, Ombay, the eastern peninsula of Celebes, &c."

Without discussing here the foundation of this whole statement, I only beg to remark that in my opinion no Negritos or the like exist in the eastern peninsula of Celebes, or in the Island of Celebes at all.

Already in the year 1876, in a lecture, "Die Minahassa auf Celebes" (p. 29, note 11), I said:—"Prof. Gerland places Papuans, in the map of Waitz's 'Anthropologie der Naturvölker' (vol. v. part 1, Malays), in the eastern peninsula of Celebes, but I could not find in the letterpress of the work, on whose authority he makes this entry. It was this very note of Gerland, which induced me, when on the spot (in the year 1871), to search after them, but I did not succeed in discovering the slightest positive proof for such an assertion." And (*l.c.* p. 8) "In Celebes . . . no autochthonic Papuan element has been discovered." Neither has Dr. Riedel, the special and foremost investigator of the whole island, obtained any trace of Celebesian Negritos. I am therefore of opinion that Celebes at least (if not many more—perhaps all —of the quoted islands) ought to be omitted from the list.

As to the occurrence of Negritos in the Philippine Islands, I only spoke of them as existing in Luzon (as generally known), in Panay, Cebu, and Negros (see *Zeitschrift für Ethnologie*, 1873, p. 90, and "Ueber die Negritos oder Aëtas der Philippinen," Dresden, 1878, p. 25). A. B. MEYER.

Royal Museum, Dresden, October 24.

Altaic Granites.

HUMBOLDT and Rose, when descending the Irtysh between Boohtarminsk and Oostkamenogorsk, saw large masses of granite lying as if poured on the ends of metamorphosed slates (S. Rose, "Reise nach. d. Ural," i. 610); an observation mentioned by Zirkel ("Petrogr.," i. 506, 1866) as a famous one in relation to the age of the Altaic granite. No subsequent traveller appears to have succeeded in repeating that observation, because nobody could rediscover the actual place, which Humboldt and Rose did not define with much precision. Ritter, however, in referring to the subject, indicates the place as lying between two rivulets—Baryshnikof and Kozlovskaya.

After some unsuccessful attempts, I at last succeeded in finding this interesting locality. It is situated some five or six miles

from Boohtarminsk, on the right bank of the river, just at the entrance into the Kozlovskee gorge, and is known among the local inhabitants under the name of Slepoy Borok (Little Blind Forest). But the illustrious travellers have made rather a blunder in defining, after only a rapid examination from the deck of an Irtyshian karbaz, the mutual relations of the rocks. A section at right angles to the stream shows that the granite lies not *on* the slates, but *in* them, and that it occurs as a main vein, with some secondary ones, all having the same strike and dip as those of the slates. The main vein is some 30 or 40 metres thick, the secondary 0·5 metre, or still less. From the river one can see only the lower limit of granite, and as the joints of this rock are nearly horizontal, whilst the bedding of slate stands almost vertical, the appearance is presented which suggested the original inference that the granite has been poured out over the ends of the slates.

My measurements gave the following results:—On the level of the current the strike of the slates varies between 9h. 45m. and 10h., dip = 82°–85° north-north-east; lower limit of granite strike, 7h. 30m., dip = 73° north-north-east; upper limit of the same, strike 8h. 45m., dip 43°–45° north-north-east. The contact of the two rocks can be observed occasionally exceedingly well. In the vicinity of the granite the slate loses all traces of fissility, and becomes a very compact rock, with abundant scales of muscovite. Generally the slates are schistose, phyllitic, and chiastolitic. Both rocks are welded together. The secondary veins of granite somewhat differ in structure from the main vein, which is of a normal fine-grained variety, with little scales of biotite. The main vein is covered with young forest, consisting of pine, birch, and aspen trees, while on the slates nothing grows but rare bushes of gooseberry, honeysuckle, some species of horse-tail, and some grasses.

The conclusion of Humboldt and Rose, that some Altaic granites are younger than local schists and slates, remains indisputable. I wish to add that they are also younger than some local greenstones, as may be seen at Beeshbanovskee crags on the Irtysh, near Oostkamenogorsk, where a dioritic breccia is cemented with granitic matrix. A. BIALOVESKI.

Oostkamenogorsk, Western Siberia, September 20.

Rankine's Investigation of Wave Velocity.

THE investigation relating to the propagation of waves contained in chapter xv. of Maxwell's "Theory of Heat," and based on that of Rankine (pp. 530-31 of "Collected Papers") presents peculiar difficulty to most readers. "The kind of waves to which the investigation applies are those in which the motion of the parts of the substance is along straight lines parallel to the direction in which the wave is propagated, and the wave is defined to be one which is propagated with constant velocity, and the type of which is not altered during its propagation."

Two cross-sections of unit area at a fixed distance apart are conceived to travel through the substance with the velocity of the wave, inclosing between them a cylindrical space within which things are always in the same condition though the matter is continually changing. The momentum of the matter which enters through the front section in the unit of time is duly expressed, and also the momentum of the matter which escapes at the rear section. The difference of these two momenta is then equated to the difference of the pressures before and behind. The puzzle is to justify this *quasi* deduction from the second law of motion; and in connection with this puzzle, the question of sign occurs. For instance, if the momentum of the entering fluid exceeds that of the issuing fluid are we to attribute the gain of momentum to the fact that the contents of the cylinder are more strongly pushed forward behind than they are pushed backward in front? Such is the impression produced on the reader's mind by Maxwell's words: "The only way in which this momentum can be produced is by the action of the external pressures"; but it is not correct.

The momentum included between the two travelling sections is changed in two distinct ways: first, by convection—that is, by gain and loss of moving matter; secondly, by pressure before and behind. *The change from pressure must be equal and opposite to the change from convection; since, by hypothesis, the momentum included between the two sections remains always the same.*

Rankine merely says, "Then in each unit of time the difference of pressure $p_2 - p_1$ impresses on the mass the acceleration $u_2 - u_1$," and gives no explanation.

I remember being puzzled by this reasoning of Maxwell's some years ago, when I was writing Note A in Part 4 of "Deschanel," and getting over the difficulty by taking the two sections very near together; but my attention has been drawn to it afresh by the receipt of a paper by Prof. MacGregor, of Nova Scotia, in which the difficulty is pointed out, and evaded in the same manner in which I evaded it. Prof. MacGregor points out that Maxwell obtains a correct result only by help of a mistake in the algebraical work—the sign of a difference being changed in obtaining equation (7) from equation (6). This is certainly true as regards the fourth and fifth editions, which are now before me. In a later edition, Prof. MacGregor remarks, the sign of the difference is changed in equation (6), thus making the algebra right, but at the expense of making equation (6) inconsistent with what goes before it. The explanation contained in the sentence printed in italics above clears up the difficulty. J. D. EVERETT.

Belfast, November 2.

Alpine Haze.

PROF. TYNDALL'S letter in NATURE about Alpine haze induces me to say that as a non-scientific observer I have never, I think, during a residence of many years here, seen so much *local* fog or haze as this autumn.

On October 29—a perfectly clear and cloudless day here (Vevey and La Tour), with no appearance of fog, haze, or cloud, anywhere in the distance—I received a letter from Lausanne saying, "While I write (11.30) so dense a fog has suddenly come up that we fear for the boats on the lake." Other friends took a trip to Lavey les Bains. They were in perfectly clear air until a little beyond Villeneuve, when they found the whole Rhone Valley thick with fog, but on turning off at St. Maurice Station to Lavey les Bains (ten minutes' walk, and on perfectly flat ground) they again came into a quite clear atmosphere. As no fog whatever came here all day, I cannot say whether it was aqueous or not. We have both sorts here from time to time, but most commonly *dry*; this year has been rather an exception. I should say fogs had been more frequently damp than usual, and by observing the grass morning and evening I have found that there has been much more dew than is common in this locality.

Streaky hazes or "long horizontal striæ," as Prof. Tyndall calls them, have certainly been unusually prevalent this year.

La Tour de Peilz, November 4. M. C. C.

The Animals' Institute.

THE long-continued suffering of animals fatally injured in our streets, before the services of a slaughterer can be obtained, or the owner be found to give his permission, has often been referred to. Poor animals with incurable abdominal wounds, or, it may be, complete fracture of a limb, not unfrequently lie in the streets for hours before being put out of their misery. The police have no power to order their destruction until the person in charge assents, and he frequently cannot do this until his master has been communicated with. I remember one case where eight hours elapsed. I have recently found that complete absence of pain can be easily induced by subcutaneous injection of morphia, and perhaps you would allow me to publicly state that the apparatus and drug are always here at the service of the police gratuitously in cases of street accidents.

JOHN ATKINSON.

9 Kinnerton Street, Wilton Place, Knightsbridge, November 5.

N. M. PRJEVALSKY.

A TELEGRAM from Vyernyi—one of those small Russian towns which have grown of late in the outspurs of the Tian-Shan Mountains—announces the death of Prjevalsky, the bold and indefatigable explorer of the wildernesses of Central Asia In September last, immediately after having terminated the work which embodies the results of his fourth great journey to Central Asia, he started on a new journey, the fifth, thus prosecuting again what has been the aim of his life during the

last twelve years—that of reaching Lhassa in Tibet, and opening to science the lofty plateaus and highlands which separate East Turkestan from India. This time he proposed to start from Russian Turkestan, and his expedition had to be equipped at Vyernyi, on the north of Lake Issyk-kul. He arrived at Tashkend in October, and had left it on October 13 (old style?) on his way to Vyernyi, but he seems not to have reached that town, and must have died on the route, as far as we can judge from the telegram. The new expedition, which promised to be even richer in scientific results than all those which preceded it, was thus prevented. But Prjevalsky has left, in the travelling companions who remained so true to him in his adventurous journeys, a staff of young men who will certainly continue his work, and sooner or later open to science the dreary highlands which have baffled so many a bold explorer.

N. M. Prjevalsky was only in his fiftieth year, and usually enjoyed robust health. He belonged to a noble family, and was born in 1839, in the Government of Smolensk. At the age of seven he lost his father. During the early years of his life he was trained by his mother (whose maiden name was Karetnikoff), a teacher who stayed in their house, and a brother of his mother. He soon became an eager hunter, and spent all his holidays in hunting in the Smolensk forests with his uncle. This taste he retained during the rest of his life, and he frankly admitted that his first journeys in Central Asia were due as much to his passionate longing for rich hunting-grounds as to his desire to conquer for science the unknown wildernesses. Scientific interest developed more and more during and after his first Central Asian journey, when, accompanied only by three men, and possessing ridiculously small pecuniary means, he crossed the Gobi, reached Pekin, and, pushing westwards and south-westwards from the Chinese capital, explored the Ordos and the Ala-shan, and reached the Kuku-nor as well as the upper parts of the Yang-tse-kiang—the mysterious Dy-tchu of the Chinese geographers. And yet, when we saw him on his return from that wonderful journey, his eyes glittered and his face radiated chiefly when he was telling us of his achievements as a hunter and a discoverer of the ancestors of our domesticated animals—much more than when he was talking of his geographical discoveries, about which he always was, in fact, remarkably modest.

He received his first school education in the Smolensk Gymnasium, but he soon left this institution, and entered in 1855 an infantry regiment as a subaltern. Next year he became an officer, and five years later he entered the Academy of the General Staff. His love for geographical exploration had been to some extent developed by that time, and the dissertation he wrote on leaving the Academy was upon the Amur region, which was much spoken of in Russia. But he had not yet the means of satisfying his desire for travel, and he was compelled to return to his regiment and take part in the suppression of the Polish insurrection. He soon withdrew from active military service, and accepted the position of teacher of geography at a Warsaw Gymnasium, devoting his leisure hours to studies in natural history. It was only in 1867 that he was admitted into the General Staff and sent to Irkutsk, whence he immediately started for the exploration of the very little known highlands on the banks of the Usuri—the great southern tributary of the Amur. Here he found a wide field for exploration and hunting, and wrote a book on the Usuri region (published in 1869), partly of an ethnographical character. The Geographical Society awarded him for this book only a small silver medal; and, when Prjevalsky applied for means to enable him to explore Southern Mongolia, the Society was anything but generous in its response. Had it not been for his own small economies—he always lived a very simple life—and for the help he received from the then Russian Ambassador at Pekin (M. Vlangalli), himself an explorer of Mongolia, Prjevalsky could hardly have started on that remarkable journey. When he began the exploration of the land of the Tangutes, he possessed only 178 roubles (about £25); and when he reached, with his three companions, the sources of the Yang-tse-kiang, after having crossed the province of Han-su, the Tsaidam, and part of Northern Tibet, he had only 10 roubles left, and his camels were quite exhausted. The whole expedition, which lasted thirty-four months (November 1870 to September 1873), had cost only 6000 roubles; yet this undoubtedly was the most remarkable journey that had been made in Asia in the nineteenth century. Prjevalsky proved that, for resolute and enduring men, travelling on the Central Asian plateaus was much easier than had been supposed. He twice crossed the Gobi, reached the Kuku-nor, penetrated as far south-west as the spot where the Yang-tse-kiang rises from the confluence of the Mur-usu and the Nantchitai River, and returned with exceedingly rich zoological and botanical collections, after having travelled no less than 7320 miles across formerly quite unknown deserts and highlands. The work in which he embodied the results of that wonderful journey, "Mongolia and the Land of the Tangutes," was immediately translated into all civilized languages. The Russian Geographical Society hastened to present him with its great Constantine Medal, and most of the Geographical Societies all over Europe congratulated him on his discoveries, and awarded him medals, honorary diplomas, and the like.

Prjevalsky, in the meantime, was trying to find the means for continuing his explorations; but it was only in 1876 that he succeeded in obtaining from the Ministry of War the 25,000 roubles which were necessary to enable him to push as far as Lob-nor. His aim was not only to rediscover the basin of the Tarim and the great lake of East Turkestan, which had not been visited by any European from the time of Marco Polo; he desired to cross East Turkestan and the northern plateaus of Tibet, and to reach Lhassa. This time he started from Turkestan, and, following the upper part of the Ili River (the Kunges), he reached Kurla in East Turkestan, whence he crossed the desert and reached the Lob-nor. The great lake was thus rediscovered. But it was impossible to reach Lhassa by this route, and Prjevalsky returned to Kulja, and thence to the Russian post Zaisan. His aim was to penetrate into Tibet *viâ* Hami, the Tsaidam, and the sources of the Blue River. So he started again, from Zaisan to Gutchen. Unhappily, the skin disease of the steppes (*pruritis scroti*) overtook him, and he was compelled to return from Gutchen. Still, next March, he was again on his way to Lhassa, when the frontier authorities ordered him to postpone his expedition. He then returned to St. Petersburg.

The Lob-nor journey was made in 1877, and although only eleven years have elapsed since, it is almost impossible now to realize the imperfection of our knowledge of Central Asia at that time. When it became known that Prjevalsky had visited the Lob-nor, Baron Richthofen contested the fact, and maintained that the lake which receives the Tarim must be situated further north and due east from the mouth of the Ughen-daria; while now Lob-nor is perfectly well known. As to the natural history collections which were brought in from this second journey, they were even more valuable than those gathered during the first journey. They gave us a clear insight into the flora and fauna of those parts of East Turkestan; while the barometrical measurements enabled us to form, for the first time, a correct idea as to the characters of the Tarim depression of the great Central Asian plateau. It was also from this journey that Prjevalsky brought the wild camel—the ancestor of the domesticated species.

As soon as he was back at St. Petersburg, Prjevalsky hastened to prepare for a new journey; and after having written a short account of his Lob-nor journey,

"From Kulja, across the Tian-Shan, to Lob-nor," he left the Russian capital for Zaisan, and began his third journey, the most remarkable of all. He soon reached Barkul and Hami, the two Turkestan oases which were almost less known than some parts of the moon. He crossed the Western Gobi, and reached a spot, Dzun-zasak, in South Tsaidam, at the foot of the highlands which separate Mongolia from Tibet. Thence he went south, in order to reach the longed-for Tibetan city of Lhassa. The journey in the highlands which border the great plateau on the north-east was exceedingly difficult. Ridges, 16,000 feet high in their lowest parts (one of them was named after Marco Polo), separated from one another by deep valleys, the bottoms of which are 13,000 and 15,000 feet above the level of the sea, had to be crossed; and when the expedition reached the upper parts of the Blue River, it was brought by the guide to quite impracticable highlands, and had to find its way amidst the barren mountains, peopled by Tangutes, whose attacks had to be repulsed by force. Nevertheless, Prjevalsky crossed the highlands, and had already reached, under the 32nd degree of latitude, the great valley of the Tibetan river Khara-usu, whence the route to Lhassa was relatively easy; but here a new obstacle rose before him. The Dalai-lama had sent officials, who declared to Prjevalsky that the Tibetan nation would not allow Russians to enter the capital of the great chief of the Buddhist religion. The expedition was thus compelled to return; and so it did, re-crossing the same highlands in the midst of the winter. Having returned to the Ala-shan town Sinin, Prjevalsky did not like to go back to Russia without having visited the Hoang-ho, which makes a great bend to the north in the neighbourhood of Kuku-nor. He reached, in fact, the great river of China at Guidui, crossed it, and explored it for some 200 miles, and only then returned to Kiakhta, after having travelled about 14,700 miles, half of which stretch was surveyed, and bringing in more than 4500 specimens of mammals, birds, and fishes, 6000 insects, and many thousands of plants. The most remarkable "find" was, however, the wild horse—the ancestor of our present horse—which inhabited Russia and Poland some two hundred years ago, and has been described by the late I. Polyakoff under the name of *Equus przewalskii* (*Izvestia Russ. Geog. Soc.*, 1881). It is hardly necessary to say that this remarkable journey produced the greatest impression on the scientific world. The Russian Geographical Society elected Prjevalsky an honorary member; the city of St. Petersburg offered him its honorary citizenship, and many scientific bodies bestowed on him all kinds of distinctions. The general results of this journey were embodied in a work entitled "Third Journey to Central Asia," which also has been translated into many European languages.

As soon as the publication of this work was ready, Prjevalsky started again, in November 1883, on a new journey, again proposing to visit Tibet. This time he started from Kiakhta, crossed the Gobi in the winter, and soon reached the spot, Dzun-zasak, whence he intended to start for the exploration of the highlands of North-Eastern Tibet. But all kinds of misfortunes attended him. The expedition, freely provided with money, already numbered twenty-one men, and so it could not move with less than fifty camels and several horses. It was found very difficult to obtain such a number of animals from the poverty-stricken populations of South Tsaidam; and Prjevalsky, usually so mild in his relations with the natives, resorted to violence. The animals he thus secured proved to be quite unfit for journeys across the high ridges which fill up the space in the south of Dzun-zasak; and it seems most probable that by taking a route due south from that point, instead of proceeding south-westwards as he did during his third journey, Prjevalski committed an error. Not taking into account the north-eastern direction of the ridges, he had to cross the numerous ridges of the Upper Hoang-ho, instead of availing himself of the depressions having a south-western direction, which permitted him to reach the Khara-usu in 1880 without serious difficulty.

It is true that, by taking a southern direction, he reached the two great lakes Jirin and Orin, through which the Upper Hoang-ho flows, and that he thus solved one of the problems of the geography of Asia. But when he went further south, he had to cross such a succession of wild highlands of an Alpine character, that his camels were soon disabled; and when he reached the Dy-tchu, or Upper Yang-tse-kiang, some 120 miles to the east of the spot he visited in 1872, he found it impossible either to cross it or to follow the river downwards. He was obliged to return, and on his way back he even could not fully explore the lakes Jiriu and Orin, because the Tangutes, gathering in hundreds, violently attacked the caravan, and were repulsed only after having lost a great number of their warriors.

Having returned to Dzun-zasak, Prjevalsky went north-westwards along the foot of the ridges which separate Mongolia from Tibet, and, when at the lake Gas, he made a winter excursion into the highlands. This excursion enabled him to get a clear idea as to the series of parallel ridges which separate the Tsaidam from the higher terrace of plateaus of North-Eastern Tibet. Moreover, instead of returning from Lob-nor by his usual route, he pushed westwards into East Turkestan, as far as Khotan, and returned to Russian Turkestan *viâ* Aksu, thus covering nearly the same ground as that visited at the same time by Mr. Carey.

Years and years will pass before all the specimens of plants and animals brought in from his four journeys can be fully described. Maximowicz's description of Central Asian plants, now being printed by instalments in the Bulletin of the Moscow Society of Naturalists, already gives some idea of the richness of Prjevalsky's collections, which represent a total of 700 specimens of mammals, 5000 of birds, 1200 of reptiles and amphibia, 800 of fishes, 2000 mollusks, 10,000 insects, and from 15,000 to 16,000 plants. All the zoological specimens are in the St. Petersburg Academy of Sciences, the botanical specimens at the St. Petersburg Botanical Garden, the geological collections at the St. Petersburg University, and special funds have been granted by the Government for the publication of the scientific results of these journeys as soon as the necessary work has been done by the specialists.

The volume embodying the general results of Prjevalsky's fourth journey, and entitled, "From Kiakhta to the Sources of the Yellow River, Northern Tibet; and the Journey from Lob-nor through the basin of the Tarim," reached London only a few weeks ago, and the present writer was preparing an account of it when the sad news reached us from Vyernyi. Although less striking than his previous books, so far as geographical discovery is concerned, this work may be even more important for the light it throws on the nature of a wide unknown country. It presents also the clearest view of the traveller himself, and affords a clue to the causes of his success.

In a chapter devoted to the ways and means of travelling in Central Asia, Prjevalsky gives detailed instructions as to how an expedition ought to be organized, and when speaking of the traveller himself he writes:—"As to the person who will have before him the beautiful task of scientifically exploring new regions, his task will not be easy. The explorer will have to pay for the smallest discoveries by plenty of suffering, physical and moral. He must be strong physically and morally. Flourishing health, strong muscles, and still better an athletic complexion, on the one side, and strong character, energy, and resoluteness, on the other—such are the features which best guarantee success." And, after mentioning the necessity

of general scientific knowledge, and of special knowledge in, at least, some one branch, as well as the necessity of a real passion for travelling, Prjevalsky adds :—"Moreover, he must be an excellent shooter, and, still better, a passionate hunter. He must not despise any hard manual work, as, for instance, the saddling of horses and camels, the packing of luggage, and so on—in short, he must never be a 'white-handed' person ; he must not have habits of luxury ; and he must have a pleasant, lenient character, which will soon acquire for him the friendship of his travelling companions." In these sentences he characterized himself. To renounce, if necessary, every comfort ; to live the life of the other members of the expedition, without any distinction between the scientific staff and the simplest soldiers or Cossacks ; to sleep in the same tent, to eat the same food, and to do the same work as the rest—such were Prjevalsky's rules. We must add also that, especially during his first two journeys, his relations with the natives were of the most friendly character. He carefully avoided any conflict with them ; and when it happened once, during his first journey, that the natives were hostile to him, and this hostility might have ended in an armed conflict, he preferred to win their respect by the following stratagem. He and his three men—all four admirable sharp-shooters—opened a fire from their breech-loading rifles upon the carcass of a horse, from a great distance. In two minutes they had discharged thirty bullets each, and they advised the Mongols to see if any bullet had touched the carcass. The Mongols rushed, of course, to the carcass, and, to their great astonishment, after hard work with their knives, discovered most of the 120 bullets in it. They did not fail, after this, to treat their visitors properly.

It is impossible to mention Prjevalsky's name without being reminded of his travelling companions. He himself so often expressed his gratitude to them, and he always wrote with so much sympathy about their common experiences, that we shall only be carrying out his wish in stating that Lieut. Pyevtsoff during Prjevalsky's first two journeys, Lieut. Roborovsky in the last two journeys, and M. Kozloff during the fourth, have their full share in what Prjevalsky modestly described as his "scientific reconnoitrings" in Central Asia. Their portraits, as well as his own, are given in his last work. P. K.

SMOKE IN RELATION TO FOGS IN LONDON.[1]

LONDON fogs are produced by the mechanical combination of particles of water with particles of coal or soot, and require for their fullest development the following conditions : a still air, a temperature lowest at or near the ground in comparison with an elevation of some hundreds of feet, saturation or partial saturation of the air within a moderate distance of the ground, absence of clouds overhead, and free radiation into space. The artificial darkness and peculiar colouring occur with greatest effect at times when a very large quantity of coal is being burnt in domestic fireplaces, and cannot as a rule prevail during the night between 10 p.m. and 5 a.m., or to any great degree in warm summer weather. The early hours of summer mornings are the only ones in which clear views of the whole city are possible. Next in clearness come fine Sunday afternoons in summer, when fires are allowed to go down. The hours of greatest density are those following the time of greatest cold on the earth's surface and of the lighting of large numbers of kitchen and other fires. Thus about 8 to 10 a.m. is frequently the period of thickest and darkest fogs. It may be noted that on Sundays, when factory fires are inoperative, fogs in winter have been densest : on one Christmas Day there was absolute darkness during the whole day, thus showing the dependence of light-absorbing matter on kitchen and domestic fires. Many distinct conditions may alter the time of maximum density.

[1] Abstract of an Address delivered by the Hon. F. A. R. Russell, on March 1, 1888, under the auspices of the Smoke Abatement Institution.

The formation of a London fog appears to take place as follows. An ordinary thick white fog covers the city, say at 6 a.m. ; about a million fires are lighted soon after this hour, and the atmosphere becomes charged with enormous volumes of smoke—that is, the gases of combustion bearing carbonaceous particles. Now, these particles, as soon as they are cooled to the temperature of the air, or below it, begin to attach to themselves the water spherules already present and visible, and vapour may also be condensed on the particles. A thick layer of these united particles prevents light from penetrating it, and a very small quantity of finely divided carbon may stop the bright sunshine altogether, like the film of soot on a smoked glass. The invariable redness of the sun through smoke seems to show that the majority of particles are comparable in diameter to the length of a wave of blue light. Smoke prevents the warmth of the oblique sunshine from reaching and evaporating the white fog near the ground, and the white fog continues to radiate towards space and towards the ground, if colder than itself, without receiving compensation from the solar rays. A difference of 10° has occasionally been noted between thermometers at 4 feet and 100 feet above the ground, the upper one always being the warmest in fogs. Carbon is a good radiator, and tends from this cause to keep itself cold by radiation into space, and thus to accumulate vapour from the air, like the dewy surface of the earth. The importance of a clear sky and a dry upper air in promoting fogs in this respect is obvious.

It has been supposed, quite erroneously as I believe, that, as even without any visible smoke an enormous quantity of fine invisible dust exists in the London atmosphere, the abolition of coal smoke would fail to give us freedom from dark fogs. Proof is altogether wanting that ordinary invisible dust in cities or plains is of a kind to create an abnormal amount of fog, or to produce those dismal obfuscations of which tarry carbon is capable. Paris, as long as it burnt wood and charcoal, was free from idiosyncratic fogs ; so are the wood and anthracite-burning cities of the United States ; and so are the towns of South Wales, where anthracite is the common fuel. If London were to cease using fuel in the solid form, it would be as free from fog as the surrounding country.

The accumulation which produces the worst and most dangerous fogs in London cannot, as a rule, proceed for many hours without disturbances arising which tend to reduce their importance. In winter the warmth of the air exceeds by 2° or 3° that of the surrounding country, consequently an upward current is started, which rises to some altitude, and then flows away, bearing with it a stream of murky cloud ; a circulation of air consequently takes place in the lower strata. If, however, the cold of the lower layers greatly surpasses that of the strata some hundreds of feet higher, and if the sun dissipates the fog in the surrounding country, thus making the environs warmer than London itself, the black fog may very likely remain on the town all day. For this reason, on a fine cold still morning, with a bright sun, and temperature near the dew-point, persons arriving from the country are pretty sure to find a black fog in town between 10 and 12 a.m. The finest winter days are nearly always very bad ones in London, unless there be any wind between the surface and an altitude of 1000 feet, or the dryness be unusual for the time of year.

When the air is very dry near the surface, no dense fog is formed in London, and when very wet, streaming with fog, in the country, little fog occurs in London. The dry warm surfaces of the houses themselves, and the elevation of temperature above the dew-point, prevents wet fogs from reaching anything like the density they attain in the

country. A wet fog disappears under cover, showing its causation by the radiation of its particles towards colder surfaces or space, as well as by mixture of differing air-currents. A dry fog persists to some degree in a warm room, showing it to be largely composed of carbonaceous particles of visible magnitude. There is nothing in the geographical position of London to make it more foggy than many other parts of the country, and, owing to the conditions just mentioned, it would probably rejoice in a clearer air, on the whole, than that of the surrounding districts if ordinary coal were superseded by anthracite and gaseous and liquid fuel.

From the daily tables of weather of the Meteorological Office for 1872, 1873, 1875, 1876, 1877, and 1882, which happened to be in my possession, I have obtained the following results, showing the prevalence of fog at different stations at 8 a m.

The first column (*b*) shows the number of days per cent. of blue sky or free from cloud at 8 a.m., and the second column (*f*) the number of days per cent. when fog more or less prevailed at that hour.

	b.	*f.*
Aberdeen	22	2
Valencia	10·1	2·4
Hurst Castle	13·7	3·2
Leith	11	5·3
Dover	27·9	6
Holyhead	13·2	6·2
Liverpool	12·4	6·6
Pembroke	12	8·5
Scilly	2	10
Oxford	10·4	10·5
Yarmouth	14·5	12·2
London	8	13·5

Aberdeen and Dover have by far the largest number of fine mornings, Aberdeen, Valencia, and Hurst Castle the smallest number of fogs.

The following list, from data of the second order stations of the Royal Meteorological Society, shows the percentage of fogs—that is, the number of days with fog—for eleven stations, during the years October 1879 to September 1882.

	Days of fog.	Notes.
Carmarthen	4·5	No observations in 1880.
Southbourne	4·8	
Ramsgate	5	
Cheltenham	7	
Eastbourne	7·9	Only two years' observations.
Llandudno	8 8	
Babbacombe	14·6	
Hereford	19·1	Only one year's observations.
Croydon	25·9	
Strathfield Turgis	26	
Norwood	34·5	

The following table gives the results of the registration of bright sunshine by glass recorders at seven places in the South of England. The figures give the number of hours of bright sunshine recorded in the four years, 1883, 1884, 1885, and 1886. In the cases of St. Lawrence, Isle of Wight, and St. Leonard's, where data for only two years existed, the result is arrived at by multiplying the value in each case by two. Eastbourne only recorded in 1886, and the result for this year has been multiplied by four. The second column gives the number of hours of bright sunshine in November and December 1885, and January and February 1886.

City of London	3925	62
Kew	5713	222
Greenwich	4845	157
Eastbourne	6660	300
St. Lawrence, I.W.	6774	316
Southbourne	6115	
St. Leonard's	6880	

This absence of light must tell decidedly on the vital force of the community, taken as a whole, and even if we had no dense fogs it would be worth cleansing our atmosphere to get the proper amount of sunshine.

In the great fogs of 1880, the death-rate of London rose from 27·1 for the week ending January 24 to 48·1 for the week ending February 7, which was the period of thickest fog. The death-rate for nineteen provincial towns in this last week was 26·3. At Croydon the rate only rose from 35 to 36. In this period of three weeks from January 24 to February 14, the excess of deaths over the average in London was 2994. Probably ten times as many were ill from the combined effects of smoke and cold. In the week ending February 7, the deaths from whooping-cough were unprecedentedly numerous, 248, and from bronchitis, 1223. Clearly, persons liable to bronchial attacks should if possible keep out of London during winter anticyclones.

The moral reaction of this atmosphere is well worthy of consideration. If smoke were got rid of, there would be a great revival of plant vigour and human gaiety, the housewives who now give up in despair the attempt to keep their houses bright and clean would no longer lose heart, the dull brick walls would begin to deck themselves with colour and ornament, the grime which seems to pervade everything would disappear, and sky and earth would appear in their natural brilliancy.

As the result of a computation taking into account the damage to buildings, furniture, ceilings, wall-papers, works of art of all kinds, the extra washing, and consequent wear and tear, for 4,000,000 people, window-cleaning, waste of coal, extra lighting required, chimney-sweeping, loss of time by artists, &c., impairment of health, and many other items, it appears that the loss by our wasteful method of burning coal must be about £5,000,000 a year.

There are many ways by which householders may economize in the use of fuel. Among them may be mentioned, the use of hot-water pipes and coils at low pressure heated by a coke stove, improved kitcheners, anthracite, patent fuel, mineral oil, and gas. With a reduced price of gas, gas cooking-ranges ought to come into general use. Gas fires throughout a house, except perhaps in one room where an open fire might be kept up, save the following expenses: burning fuel at times when not required, labour and wear and tear in carrying coals, dirt and blackening of ceilings, &c., wood for lighting, sweeping of chimneys, coal-scuttles, fire-irons, emery, and the very considerable amount of domestic labour now taken up by cleaning, laying, and attention generally. Wherever an open coal fire is insisted on, the various improved grates may be employed with great advantage both for economy and smokelessness. Rows of houses could probably be heated economically by hot-water or steam pipes from a boiler in a central position; but the uncertainties of our climate make independent arrangements on the whole preferable. Wasteful ranges might be subject to a tax, which the wasteful householder might be presumed to be capable of paying without inconvenience, and smoky chimneys should not be allowed to pollute the atmosphere with impunity. This would have an effect opposite to that of the tax on light, the window-tax, which vexed the last generation, for it would tend to increase the brightness of London dwellings by the admission of sunlight.

The method of heating water by the arrangement known as the "Geyser" or "Therma" has the merit of utilizing nearly the whole of the heat given off by the gas in burning. This could be employed at the top of the house, the hot water running into pipes in the rooms or on landings, and finally into a cistern in the kitchen, and here, if necessary, an ordinary boiler for heating coils in the upper part of the house or in the hall would utilize the water not yet cooled to the temperature of the air.

The abolition of smoke would certainly effect a very large saving to the community, and would add greatly to the amenity of the climate, not only of London, but of all

the adjoining counties. Thousands of acres in the environs could be acquired and turned into gardens with the savings of a single year in the perfect combustion of fuel. In times of distress a sum equal to the wages of 100,000 labourers is now thrown away in the manufacture of an artificially coloured atmosphere. Remedies in accordance with science are at hand; it only remains for society to see that they are applied.

DESICCATED HUMAN REMAINS.

SOME time ago, Signor S. Marghieri, the Mexican archæologist, while exploring the eastern side of the Sierra Madre Mountains in Mexico, at an elevation of nearly seven thousand feet, discovered and explored a hermetically sealed cave. The floor was nearly smooth, the sides rough and rugged, and the vault covered with stalactites. At the far end of the cave, which was of considerable dimensions, four mummified human bodies were found. The bodies—a full-grown male and female, and a boy and girl—were in a sitting posture, hands crossed on the breast, and knees approaching the chin, with the head inclined forward. They were all carefully enshrouded in burial garments, and accurately placed facing the rising sun. We may suppose that the elder male and female were husband and wife. They sat side by side; the elder child, a boy, was placed to the right of the father; the younger, a little girl, to the left of the mother. There was no trace of any implements, utensils, or personal effects; nor were there on the walls hieroglyphics or pictographs. The cave had been sealed by means of sun-dried, adobe bricks, and adobe paste or plaster, together with natural rocks from the mountain. So well was the work done that none but an acute observer would have noticed the artificial closure.

The bodies were brought to San Francisco, and bought by Mr. J. Z. Davis, by whom they were presented to the State Mining Bureau, in the archæological department of which they are now preserved. The following description of them is taken from a careful report drawn up by Dr. Winslow Anderson, for the Board of Trustees of the California State Mining Bureau:—

These naturally mummified bodies differ from mummies proper, in the general acceptation of the term, inasmuch as no embalming process for their preservation was used. They were desiccated in their cave sepulchre by natural elements. The dry hot atmosphere extracted all the moisture from the tissues, and the bodies literally dried up as we would dry jerk-beef, or as the Indians of to-day dry the bison (buffalo) meat which keeps for years.

There is no evidence of these bodies having undergone any preparatory process. The brain, heart, lungs, abdominal and pelvic viscera are all intact and dried to a solid consistency.

The elder male body is about five feet eight inches tall, and well proportioned. The bones are large, and he must have had an excellent physique. He probably weighed between one hundred and eighty and two hundred pounds. All the body now weighs is fourteen pounds.

The integument is well preserved, and presents the appearance of dried hide, or thick parchment, of a dark gray colour, and all that remains between it and the bones are the dried muscles, tendons, nerves, and fascia. The body is well developed, the shoulders measuring from one acromion process to the other, three hundred and ninety millimetres (about fifteen and a half inches); the hands are small, and the fingers tapering; the feet are also small, measuring two hundred and forty millimetres (about nine and a half inches), and highly arched. The phalanges of the digits are perfect, each having the normal number of bones, and the ungual appendages are well preserved and not unusually long.

The body has dried in the sitting posture, hands crossed and knees drawn towards the chin. The cheek and lips on the left side protrude. This probably occurred during the time of mummification; the moisture leaking from the interior of the brain and surrounding tissues, through the cribriform plate of the ethmoid at the anterior portion of the calvaria, through the cribriform foramina into the inferior meatus nasi, and the head being inclined toward the left, produced this bulging from the force of gravitation. Being itself in turn dried up, the mouth maintained its present shape. Short stiff hairs can be seen on the head. The eyebrows and eyelashes are also distinctly visible. A little hair can also be noticed on the upper lip, but very little beard anywhere on the face. The ears are closely pressed against the sides of the head, and only the cartilages remain. The eyes are quite perfect, and present a slight outward obliquity. The nose, originally broad, has been more flattened by the shrinking of the cartilages and the alæ nasi. The lips are stiff and solid, and the tongue is shrivelled to the consistency of cork. There is a full set of masticators in his mouth, thirty-two in number, and all quite well preserved. A few of the dentures only have the enamel worn down to the dentine. The ribs are large and well formed, indicative of a well-shaped chest. The genitalia are well preserved. On the head there has been a large growth of hair, on the face very little, and on the body scarcely any at all.

Owing to the dried integument and fascia covering the cranium, accurate measurements of the skull are well nigh impossible. The following measurements, however, have been made with as much care and accuracy as the subject permitted. The cranial measurements are as follows: circumference, 530 millimetres; length, occipito-frontal, 178 mm.; breadth, bi-bregmatic, 140 mm.; breadth of frontal, 108 mm.; height, 135 mm.; facial angle, 71°.

The sutures and wormian bones cannot be inspected. The malar bones are quite prominent and the lower maxillary and face may be classified with the group orthognathous.

A careful study of this mesocephalic head would indicate that its possessor was of more than average intelligence. The perceptives are well developed. And, although the animal passions undoubtedly predominate, there is enough veneration or religion to class it among the scaphocephalic skulls.

The elder female body is in a better state of preservation than the preceding body. From a measurement of the individual bones, she would be about five feet five inches tall, and weighed, perhaps, about one hundred and fifty to one hundred and seventy pounds. The body weighs, in its present condition, only twelve pounds. The posture, integument, body, &c., resemble the one previously described. The large, oval pelvis, and the once well developed mammæ bear unmistakable evidence of gestation. The hands and feet are small and well shaped; the foot measuring only two hundred and fifty millimetres (about eight and one half inches). On the head is a luxuriant growth of hair, which centuries have not succeeded in destroying. It is very fine in texture, of a dark brown colour, and entirely unlike any Indian hair seen to-day. A curious feature is observed in connection with the small, well-proportioned ears, both of which are perfectly preserved, and that is, in each lobe is worn, even in the stillness of death, a piece of hollow bamboo or reed, about forty millimetres in length, and ten millimetres in diameter. This was probably considered an ornament in her day. The Indians of to-day pierce the helix and antihelix of the ear, through which holes they suspend ornaments of different kinds. The single perforation in the lobe of this mummified woman's ear would indicate a custom observed by her people, similar to the customs in vogue in the more civilized countries, and are not usually observed by Indians of our own period.

The eyes are singularly perfect, presenting a slightly outward and upward obliquity of the external canthi.

The nose is also quite perfect, and inclined to be rather broad and flat than thin and protruding. The malar bones are very prominent. The lips are thin and stiff, and the tongue is dried and solid. Two central incisores and one canine of the superior maxillary are gone, and several other teeth are badly caried.

Here, again, the hair and dried integument prevent absolutely accurate cranial measurements. The skull measures: circumference, 503 millimetres; length, occipito-frontal, 166 mm.; breadth, bi-bregmatic, 128 mm.; breadth of frontal, 103 mm.; height, 132 mm.; facial angle, 69°.

This skull presents a large forehead and well-developed reasoning powers. It is very rare to find so good a head among Indian women of to-day.

The little boy seems to have been about seven years old. The little fellow had been enveloped in his burial shrouds the same as the larger bodies—hands crossed on the chest, knees doubled on the breast, and the head inclined forward. All the bodies were probably tied in this position when placed in the cave. The body is about three feet tall, and weighs now only three pounds. The same general characteristics as to skin, tissues, bones, &c., that were observed in the preceding bodies, may also be seen here. The head is well developed for a boy of his age. The hair has been broken off near the scalp. Only the cartilaginous parts of the ears remain. There is the same contour of face—flat nose, high cheek-bones, outward obliquity of the eyes, &c. The upper and lower incisores and canine of the temporary or milk teeth are gone, and the permanent set coming at their roots in the alveolar processes.

The two anterior molars of the superior maxillary are just appearing through the alveolar processes, establishing the age with tolerable accuracy at about seven years.

In circumference the skull measures 440 millimetres; length, occipito-frontal, 146 mm.; breadth, bi-bregmatic, 120 mm.; breadth of frontal, 60 mm.; height, 114 mm.; facial angle, 71°.

A considerable part of the burial shroud remains about the body yet. The major portion of it is cotton fabric, firmly secured around the body by a stronger cord, made of braided hair.

The little girl may have been about fourteen to eighteen months of age. She weighs only a pound and a half. She has been enveloped in an animal's skin, the better to protect the tender frame. Both feet are gone, and the tibiæ and fibulæ protrude through the skin. The four upper and four lower incisores, with the corresponding canine teeth, have made their appearance, showing the child to be about fourteen to eighteen months old. Otherwise the same features are noticeable in this as in the proceding figures.

It would appear that the group of four belong to one family, and that they were buried by friends, and hermetically sealed in this cave for fear of some real or imaginary foe. It may have been at the time of the Spanish invasion, or it may have been during the warlike times anterior to this date, when the Aztec confederation was warring with the Toltec people.

From their physical and mental developments the race seems to have been a superior one.

The facial features observed in these bodies are not those found in that locality now. The cranial configurations and physical appearances would rather favour Aztec lineaments than those of the Indian of to-day. The fine dark brown hair is certainly not Indian, nor do the small hands and feet bear much resemblance to the huge hands and feet we see on the Indians now living.

The fabrics found on the bodies, forming the burial shrouds, are chiefly composed of cotton, hair, hide, grasses, and the bark of willows. The cotton is twisted and coarsely woven, each thread being from a half to one millimetre in diameter. The hair is treated in like manner occasionally, although usually it is braided with three or four divisions in each cord. Frequently we find strong strands made of strips of hide covered with willow bark.

Although the weaving of this interesting people is that known as the "plain" process—that is, where the weft passes alternately under and over the threads of the warp, producing more or less open mesh cloth—yet considerable skill and ingenuity were observed in the manufacturing of their blankets, mats, and ornamental cloths, which were frequently interwoven with beads and coloured threads, presenting various designs. Grasses and straws were also woven into mats and cloths, which were of great durability. The skins of animals were also used for clothing purposes.

THE PHILIPPINE ISLANDS.

MR. WALLACE, in his great work "The Geographical Distribution of Animals," divides the Oriental or Indian region of Mr. Sclater into four sub-regions, of which Java, Sumatra, Malacca, Borneo, and the Philippine Islands form one, which he calls the Indo-Malayan. In his discussion of the Indo-Malayan sub-region Mr. Wallace recognizes several subdivisions of it, and treats of the Philippine Islands as one of the most important of these. Though acknowledging the existence of divisions of his sub-regions, he failed to give them technical names, as being at that time uncalled for.

The purpose of this paper is to show that the Philippines themselves are separated into several distinct zoological divisions, and it seems therefore necessary for their study to give technical names to the primary and secondary divisions of the already recognized sub-regions. The terms province and sub-province seem least objectionable, and will be made use of, the Philippine Islands thus forming one of the provinces of the Indo-Malayan sub-region, and the divisions of the group itself sub-provinces.

The zoological province of the Philippines is co-extensive with the political division of the same name, with perhaps the exception of the islands of Sulu and Tawi Tawi, which lie between the Philippines and Borneo, but are claimed by the Spanish.

The sub-provinces proposed are—first, the Northern Philippines, consisting of Luzon and Marinduque, and a number of other small islands about Luzon; second, Mindoro; third, the Central Philippines, made up of the islands of Panay, Negros, Guimaras, Zebu, Bohol, and Masbate; fourth, the Eastern Philippines, comprising the islands of Samar and Leyte; fifth, the Southern Philippines, embracing the great island of Mindanao, with Basilau, and perhaps Sulu; and sixth, the Western Philippines, consisting of the islands of Paragua or Palawan, and Balabac.

The geographical positions of these sub-provinces are so far fortunate, that these names show their relations to each other very closely, as may be seen by consulting a map of the archipelago.

Of the sub-provinces, the Western Philippines, made up of Paragua and Balabac, and perhaps the Calamines, is of most importance, its animal life being much more closely allied to that of Borneo than that of any other sub-province of the group. This is especially noticeable in its mammals, of which it possesses, in common with Borneo, the genera *Tragulus*, *Tupaia*, and *Manis*, which are apparently absent from the rest of the archipelago. Of Bornean genera of birds, not found elsewhere in the group, *Iora*, *Criniger*, *Polyplectron*, *Tiga*, and *Batrachostomus*, are examples. This sub-province has evidently received a large part of its fauna from North Borneo, through Balabac, at a comparatively recent date, and

since its separation, on the north, from the rest of the Philippines, so that these genera have not flowed over into Mindoro and Luzon. In addition to these apparently late arrivals from Borneo, the sub-province possesses a large number of peculiarly Philippine birds and mammals, which show it to be an integral part of the province.

The rest of the Philippines would seem to have received their Malayan fauna at another time, and by the other way of Sulu and Mindanao. They possess the mammalian genera, *Galeopithecus*, *Tarsius*, and *Cervus*, which are apparently wanting in the western sub-province, and the genera *Macacus*, *Sus*, *Viverra*, *Paradoxurus*, and *Sciurus* in common with it. Of birds, the genera *Loriculus*, *Cyclopsitta*, *Buceros*, and *Penelopides* are examples of forms which are more or less generally distributed over the archipelago outside of the western sub-province.

The grounds for dividing the Philippines east of Paragua into sub-provinces are, to a great extent, based upon species, and especially upon the existence in each of representative forms of the genera *Loriculus*, *Buceros*, *Penelopides*, *Pitta*, *Chrysocolaptes*, *Dicæum*, *Cinnyris*, &c. The hornbills form perhaps the most striking example of this distribution of representative species. Of the eleven species of hornbills obtained in the islands, the western sub-province has one, the southern three, the central two, the eastern two, Mindoro one, and the northern sub-province two, and we have found no case of a single species occupying more than one sub-province, or of more than one species of a genus in a single sub-province. The genus *Chrysocolaptes* of woodpeckers is also noticeable, each sub-province possessing its own species, with the exception of Mindoro, which lacks the genus altogether. The genus *Loriculus* of the parrots is of the same character. Of other animals than birds, the genus *Sciurus* of mammals, and *Draco*, the flying lizards, seem to have representative species in each sub-province, and the land-mollusks are probably distributed in the same way.

The above examples are a few that come to mind before a careful study of our collections has been made, and they do not by any means show all the reasons for the conclusions arrived at. These are the results rather of the general observations of five careful men who have been collecting and studying in the Philippines during the last year. During this time we have visited and collected in fifteen islands of the group, and these the largest and most important. I am satisfied that the study of our collections with the aid of the libraries and collections at home, will only strengthen the conclusions of this paper. It may be necessary to make the so-called western sub-province of more importance in the arrangement, but the non-existence in nature of exactly equivalent divisions of any kind is well recognized.

It is hoped that our work may aid in untangling some of those puzzles in which students of Philippine zoology have found themselves involved, and that it will also add considerably to the sum of knowledge concerning this, as yet, imperfectly known corner of the earth.

Manilla, July 2, 1888. J. B. Steere.

BAROMETRIC OSCILLATIONS.

THE following account of what appears to have been the passing of H.M. surveying-ship *Egeria* through the embryo of a cyclonic disturbance, has been received from Captain Aldrich, of that ship.

Admiralty, August 1. W. J. L. Wharton.

H.M.S. "Egeria," at sea, June 6, 1888.

I send the following extract from my journal, which may possibly be of interest to meteorologists:—

"*May* 31.— . . . There has been a swell from the south-west during the day. The lower clouds come from the eastward, while the upper ones are from the westward. This appears to be a common occurrence in this locality.

"♀ *June* 1.—Weather cloudy all night, and wind-force 2·3, gradually veering, till at 3.30 a.m. it was to the northward of east. Barometer rising slowly. At 6 a.m. the wind shifted to the north-east in a rain-squall; nimbus, and a generally dark appearance in the sky. At 7 a.m. the officer of the watch sent down to tell me the barometer had fallen 0·12 of an inch in the previous hour. I was about to commence dressing at the time, but, hurrying on my things, I looked at the aneroid in my fore-cabin, and found the report correct. I immediately reset the aneroid and went on deck, and although there were no signs of any forthcoming disturbance, the light sails and mainsail were taken in. At 7.20 I had the barometer again read, when it was reported to have risen nearly $\frac{1}{10}$ inch in the twenty minutes. I went down immediately, and found by the aneroid this jump had taken place. During this time there was no change in the weather, though the wind drew to the north-north-east."

Now there is no doubt whatever that the barometer went up 9/100 in twenty minutes. Of course, it cannot be known to an hour when it previously dropped ·12, as the instrument was not read between 6 a.m. and 7 a.m., and the drop may possibly have been greater even, and also *may* have taken place as suddenly as it rose afterwards. There is no doubt that some extraordinary disturbance of the atmosphere took place, though beyond the foregoing observations we neither saw nor experienced anything of it. The following are the actual readings of the mercurial barometer:—

a.m.		Inches.	Position.
6	...	30·170	Lat. 24·04 S.
7	...	30·050	Long. 179·04 W.
7.20	...	30·144	
7.30	...	30·154	
7.45	...	30·186	
8	...	30·186	
9	...	30·200	

Pelham Aldrich.

NOTES.

THE following is the list of names recommended by the President and Council of the Royal Society for election into the Council for the year 1889, at the forthcoming anniversary meeting on the 30th inst.:—President: Prof. George Gabriel Stokes. Treasurer: Dr. John Evans. Secretaries: Prof. Michael Foster and Lord Rayleigh. Foreign Secretary: Prof. Alexander William Williamson. Other Members of the Council: Prof. Henry Edward Armstrong, Henry Bowman Brady, Charles Baron Clarke, Dr. William Huggins, John Whitaker Hulke, Prof. John W. Judd, Dr. Edward Emanuel Klein, Prof. E. Ray Lankester, Prof. Herbert McLeod, Sir James Paget, Bart., William Pole, William Henry Preece, Sir Henry E. Roscoe, Dr. Edward John Routh, Prof. Arthur William Rücker, and Captain William James Lloyd Wharton, R.N.

THE Pasteur Institute, Paris, is to be opened on the 13th inst., in presence of numerous delegates of the French Academies of Science and of Medicine, and of the Medical and Scientific Faculties. President Carnot will perhaps be present.

WE regret to announce the death of the well-known geologist, Dr. Theodor Kjerulf, Professor at the University of Christiania, and Director of the Geological Survey of Norway. He died at Christiania on October 25, at the age of sixty-three. He received his appointment as Professor in 1858, and since that time has made many important contributions to geological science.

THE death is announced of Herr Johann Kriesch, Professor of Zoology and Prorector of the Royal Joseph Polytechnic at Budapest.

THE first wing of the Durham College of Science, Newcastle, was opened by the Princess Louise on Monday. The plans for the structure as a whole are very elaborate, and it is expected that the building, when completed, will be a great ornament to Newcastle. The wing just opened is about a third of the College, and has cost £23,000. The remainder will be built when the necessary funds are raised. Many of those who took part in the opening ceremony afterwards met at luncheon. Mr. John Morley, responding for the House of Commons, referred to the number of eminent men of science now in Parliament. Touching on the question how far Parliament may be expected in future to sanction expenditure for the promotion of such objects as the Durham College of Science has been founded to maintain, Mr. Morley said that the House of Commons would be willing to sanction grants from the public purse for objects of this kind in proportion to one thing, and that was to the evidence that could be brought before them that in localities an effort had been made to raise as abundant funds as these localities could provide.

H.M. SURVEYING-SHIP *Egeria*, under the command of Captain P. Aldrich, R.N., has, during a recent sounding cruise and search for reported banks to the south of the Friendly Islands, obtained two very deep soundings, of 4295 fathoms and 4430 fathoms (equal to 5 English miles) respectively; the latter in latitude 24° 37′ S., longitude 175° 8′ W.; the other about 12 miles to the southward. These depths are more than 1000 fathoms greater than any before obtained in the southern hemisphere, and are only surpassed, as far as is yet known, in three spots in the world—one off the north-east coast of Japan, of 4655 fathoms, found by the United States s.s. *Tuscarora*; one of 4475 fathoms, south of the Ladrone Islands, by the *Challenger*; and one of 4561 fathoms, north of Porto Rico, by the United States ship *Blake*. Captain Aldrich's soundings were obtained with a Lucas sounding-machine and galvanized wire. The deeper one occupied three hours, and was obtained in a considerably confused sea, a specimen of the bottom being successfully recovered. Temperature of the bottom, 33°·7 F.

IT appears from the Annual Report of the Société des Naturalistes de Moscou, which was read at its annual meeting on October 15, that the Society now has 535 members. During the past year the Society sent out MM. Zarudnyi, Litvinoff, Lorentz, Milutin, Kosmovsky, Golenkin, and Rostovtseff for the exploration of the Transcaspian region and the Caucasus, as well as for zoological and botanical explorations in several provinces of Central Russia. Besides its *Bulletin*, the Society has brought out a new instalment of its *Mémoires*.

THE courses of lectures at the Tomsk University were opened on September 13. There are already sixty-nine students, all Siberians.

NOTWITHSTANDING the considerable difficulties which have been met with in the digging of a canal to connect the Obi with the Yenisei, and the want of money for the completion of the undertaking, the work of connecting the two great arteries of navigation in Siberia is still advancing. In the summer of the present year a boat 56 feet long and 14 feet wide, taking 3½ feet of water, was drawn from the Obi into the Yenisei with a load of 40 tons of flour. The two rivers are 630 miles apart.

ON October 17, 1887, Mr. William Colenso, F.R.S., read before the Hawke's Bay Philosophical Institute a "Jubilee paper," entitled "Fifty Years Ago in New Zealand." This paper has now been published. It contains, among other interesting records, an excellent account of the introduction of the printing-press into New Zealand, and of the printing of the New Testament in the Maori language in 1837. Recalling the events of his life during his long residence in New Zealand, Mr. Colenso refers to December 25, 1835, when he met Darwin in the Bay of Islands, and spent with him "a happy long day."

THE atomic weight of tin has been redetermined by Prof. Classen and Dr. Bongartz, of Aix-la-Chapelle. Four distinct series of determinations have been made, including in all no less than forty-seven separate estimations. The accuracy of the work may be judged from the fact that the difference between the highest and lowest values obtained is no more than 0·4. The first series consisted in oxidizing pure tin to stannic oxide, and thus determining the ratio $Sn : O_2$. The purest commercial tin was taken as the starting-point, and the 0·5 per cent. of impurities removed by the following process. It was first converted to stannic chloride, $SnCl_4$, by the action of dry chlorine gas; the chloride was next fractionally distilled, and a portion eventually obtained boiling constantly at 120° C. This was diluted with water, and treated with solution of sodium sulphide until the precipitated sulphide of tin redissolved; a quantity of caustic soda solution was then added, and the liquid allowed to stand for a few days. It was subsequently submitted to electrolysis in weighed platinum dishes, upon the interior surface of which the tin was deposited as a beautiful silver-white metal. The tin obtained in this manner was exceedingly pure, and eminently suitable for use in atomic weight determinations. Weighed quantities of it were, in the first series of experiments, oxidized with redistilled nitric acid; the excess of acid was expelled upon a water-bath, and the residual stannic oxide first gently ignited over a small flame, and finally more strongly heated in a muffle furnace. The mean atomic weight derived from eleven such experiments is 118·76, a value considerably higher than the usually accepted one, 117·8, based upon Dumas's redetermination in 1858. In the second series the ratio of $Sn : Cl_4 + 2NH_4Cl$ was estimated, as given by electrolysis of the double chloride of tin and ammonium, $SnCl_4 . 2NH_4Cl$. Pure stannous chloride prepared as above was readily converted into this double salt which was obtained in fine crystals. Weighed quantities were dissolved in solution of ammonium oxalate and submitted to electrolysis, the tin being again deposited, washed, dried, and weighed. Sixteen such estimations give the mean value 118·81. The third series were precisely analogous, the double chloride of tin and potassium being employed; the mean of ten determinations affords the number 118·80. In the fourth series pure tetrabromide of tin was electrolyzed in presence of ammonium oxalate and oxalic acid, and the ratio $Sn : Br_4$ thus arrived at. The mean result of ten experiments in this series is 118·73. Finally, the mean value deduced from the whole forty-seven experiments is 118·77, or in round numbers 118·8, oxygen being taken at Stas's value, 15·96. If oxygen be 16, tin becomes slightly less than 119·1. This important metal may therefore be added to the interesting list of those whose atomic weights are probably whole numbers.

MR. G. V. HUDSON notes in the current number of the *Entomologist* that on March 7 he observed the largest assemblage of moths he has ever seen in New Zealand. They were flying round an electric light suspended from the yard-arm of the steamship *Aorangi*, at the wharf in Wellington Harbour. He thinks that at a moderate computation there were over three hundred specimens. He could not capture any, owing to the great height of the light; but they appeared to be chiefly *Mamestra composita* and *Porina signata*. Mr. Hudson points to this as a good instance of the efficiency of the electric light in attracting insects. He has found that an ordinary lamp will not attract more than a dozen or twenty specimens, even under the most favourable circumstances.

In the current number of the *Zoologist* Mr. John Cordeaux has some valuable notes on the occurrence of Pallas's sand grouse in Lincolnshire. Exclusive of the flocks seen near the coast late in August and in September and October, which may probably be referred to birds coming from the interior, and approaching the sea before taking their departure, the number actually recorded as visiting Lincolnshire is, as far as Mr. Cordeaux is able to ascertain, about 184. Making a liberal allowance for flocks seen more than once, he thinks the number may certainly be estimated as considerably exceeding a hundred. Taking the dates as they occur, the number in each flock, and the localities in which the birds have been seen, he is inclined to place the actual number at about 140 to 150 between May 18 and July 28. The number killed was twenty-five, of which sixteen were shot, eight died of poisoned grain, and one was killed on the railway. Mr. Cordeaux has no information which indicates that any sand grouse nested, or attempted to nest, anywhere in Lincolnshire.

The current number (vol. xvi. Part 2) of the Transactions of the Asiatic Society of Japan contains a number of specimens of Aino folk-lore translated literally by Mr. Batchelor, whose name is already known to our readers in connection with Aino studies. Mr. Meik, a civil engineer, employed by the Japanese Government to travel round the Island of Yezo to advise as to the most suitable sites for the construction of harbours, describes his journey. He draws attention to the diurnal inequality of the tides on the Yezo coasts. This amounts to 3 feet at spring-tides along the south-east coast, the maximum rise of a spring-tide being 6 feet, while the range of an ordinary spring-tide is about 4½ feet. The lowest tide at new and full moon occurs about 10 a.m., and the second daily tide reaches a minimum about three days and a half before new and full moon, or at the change of tides. On the south-east coast this minimum afternoon tide occurs about 6 p.m., and only registers a few inches, while on the west coast there is practically only one tide in the twenty-four hours for four days before and one day after new and full moons, and during this period the tide takes sixteen hours to rise and eight to fall. Mr. Parker discourses in his usual very learned way on the Chinese and Annamese languages.

At a recent meeting of the Society of Science of Christiania, Prof. G. Storm demonstrated the identity between Ginnunga Gap, referred to in the *Sagas*, and meaning the "World's End," and the present Davis Straits.

The first discovery of remains of cave-dwellers in Scandinavia has been made in a cave in the Great Carl's Island, in the Baltic, a couple of miles west of the Island of Gothland. Last year a farmer, while digging for mould for a plantation, discovered in a cave or grotto layers of ashes and charcoal mixed with bones. The latter, having been forwarded to the Royal Museum at Stockholm, were found to be the bones of horses, bullocks, pigs, birds, and fishes. In consequence of this discovery, Prof. G. Lindström commissioned Dr. L. Kolmodin to carry out excavations in this cave in a scientific manner; and the result is that indubitable remains of cave-dwellers have been found. The cave is situated about 20 metres above the sea-level, and consists of two parts, an outer one, about 12 metres long and 7 metres wide at the mouth, and an inner one, about 9 metres long and 1½ metre wide; the latter leading into a transverse gallery running south-west and north-east. Dr. Kolmodin began by excavating the layers at the mouth of the cave, and here he encountered, almost in the exact spot where the fire-place had been, a grave 5 metres in length, 2½ metres in width, and 3·2 metres in depth. There are alternate layers of ashes and charcoal, interspersed with remains of the animals named above. The bones of "domestic" animals decrease in quantity downwards, whilst those of seals increase. The explorer found, at a depth of 24 decimetres, fragments of coarse pottery of a primitive kind and some chips of flint; at a depth of 28 decimetres an implement of flint; and in the lowest layer, at 32 decimetres depth, two small drills of bone. Several of the fragments of pottery found below a depth of 24 decimetres bore traces of simple ornamentation. Everywhere in the layers were found bits of granite and chalk, clearly showing that they had been split by fire. Most of the bones had been split or crushed, and the marrow extracted. Among the remains was part of a human cranium. It may be added that the island on which the discovery was made is only a couple of hundred acres in extent, and uninhabited.

The Pekin correspondent of a Shanghai newspaper writes that a special edition of a work on natural philosophy, compiled by Dr. Martin, the head of the Foreign College of Pekin, has been prepared for the use of the Emperor of China, and that sixteen volumes of Macmillan's Science Primers have been translated into Chinese by Dr. Edkins, the well-known scholar, at the instance of Sir Robert Hart, the Inspector-General of Chinese Customs. "These elementary books will supply a want felt in the preparatory science schools which are now being inaugurated, especially in Tientsin. Excellent prefaces to the series, inculcating the advantages of a scientific training, praising the advance made in science in the West, and the valuable contribution or legacy which Sir Robert Hart, amid all his other work for the regeneration of China, is leaving, have been written by the two foremost statesmen in China—the Viceroy, Li Hung Chang, and the Marquis Tseng."

The ninth monthly part of "The Cyclopædia of Education" (Sonnenschein) has been issued. The work will be completed in about twelve parts.

Dr. A. B. Griffiths has in the press "A Treatise on Manures," which will be published by Messrs. Whittaker and Co., of Paternoster Square.

Great pains are taken to secure that the penny science lectures at the Royal Victoria Hall shall be attractive and successful. On November 6, Mr. W. Lant Carpenter delivered a lecture on "Speech-Recording Machines." The following are the announcements for the remainder of the present month: November 13, Mr. Harold Cox, "India"; November 20, Dr. W. D. Halliburton, "The Throat and the Voice"; November 27, Prof. H. G. Seeley, "Underground Heat."

Since September last a system of storm-warnings has been in use on the coasts of the Black Sea; they are issued from the Odessa Meteorological Station in connection with the Central Physical Observatory at St. Petersburg. Signals announcing the approach of strong north-eastern and south-western winds, as well as of storms, are shown at Odessa, Sebastopol, Kertch, Taganrog, Rostoff, Poti, and Batum. The signals are the usual cones and cylinders.

In the Jamaica Weather Report for the month of August, Mr. Maxwell Hall gives an interesting account of the more prominent features of some of the West Indian cyclones observed there during the last ten or twelve years. These hurricanes usually originate in the regions of heavy rains which advance as far north as latitude 15° in August and somewhat farther north in September and October. From November to July the rains withdraw nearer the equator, where the divergence of the air-currents is insufficient for their generation. All the cyclones which have passed Jamaica confirm the theory of the influx of the wind towards the centre. Mr. Hall states that there is a feature often observed in Jamaica which is not noticed elsewhere, viz. after a cyclone has passed, and is moving away, it draws the winds and clouds after it for one or two days, and that this fact enables him to draw conclusions as to the direction in which the cyclone is moving.

THE thirteenth yearly Report of the Forest Meteorological Stations of the German Empire, for the year 1887, published by Dr. A. Müttrich, contains monthly and yearly results for sixteen stations. The temperature and humidity are observed in the open country, in the forests, and in the crowns of the trees. The Report contains, besides, other data of interest, such as evaporation and rainfall, and the dates of first and last frosts.

IN a lecture delivered at Trevandrum by Lieut. Harold Ferguson, and reproduced in the *Madras Mail*, on "Some Popular Errors about Snakes," poisonous snakes are divided into three classes: (1) sea-snakes; (2) viperine snakes; and (3) poisonous colubrine snakes. All members of the first class are poisonous. The second class have flat triangular heads, and may be subdivided into pit vipers and true vipers. The true vipers are recognizable by having scales and not shields on the head, and the pit vipers by a deep depression between the eye and nostril. It is not easy to distinguish the third class at first sight from the harmless snakes, but it is an unfailing sign that the latter are covered on the head with large flat scales, which are absent from the head of poisonous colubrine snakes. With regard to Southern India, the poisonous snakes are not numerous. The largest is the dreaded hamadryad (*Ophiophagus elaps*), which is very rare. Other poisonous snakes found in that region are the cobra, the bungarus or krait, the callophis (four species), two species of *Trimeresurus*, *Daboia elegans*, and *Echis carinata*. In all there are about twelve species of poisonous snakes, five of which inhabit the low country—namely, the cobra, the two species of bungarus, the daboia, and the echis; the others are met with only in the hills.

THE British Vice-Consul at Candia, in Valencia, in a recent report on the agriculture of his district, refers to the insect pests of that province. He says that almost all men and boys there are fond of shooting, and they ruthlessly slaughter the small insectivorous birds. The result is, of course, disastrous to the farmers. The apple-trees in the district have been almost all destroyed by a worm called locally "*bañarriquer*," which eats its way into the trunks of the trees, and then spins a thick cobweb over the branches. The eggs, which number from thirty to fifty in each nest, are easily found; but this is rarely, if ever, done. Strange to say, the worm confines its attention to apple-trees; other fruit-trees flourish there. At Lucerne, in the same district, two species of insect pests are found, both caterpillars—*oruga verde* and *gusaro negro*. The latter is difficult to deal with, as it burrows under the roots of plants, but the former is easily swept off the leaves by a net. The pea crop is attacked by the *cadell*, a worm, and the *blanquita*, an aphis. To complete a list of the ills from which agriculture suffers in that region, mildew has this year attacked the vine.

THE *Journal de la Chambre de Commerce de Constantinople* states that a method of solidifying petroleum has just been discovered. A small quantity of soap is added, and the mixture is heated. When the mixture is allowed to cool, the product can be cut into small cubes like those of compressed charcoal. Thus petroleum can be used as a combustible, it being now easy to transport and manipulate it.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Miss D. Hughes; a Vervet Monkey (*Cercopithecus lalandii* ♂) from West Africa, presented by Miss Helena Blow; a Common Genet (*Genetta vulgaris*) from Andalusia, presented by Lord Lilford, F.Z.S.; a —— Fox (*Canis* ——) from India, presented by Colonel Sir Oliver B. C. St. John, K.C.S.I., F.Z.S.; an Axis Deer (*Cervus axis* ♀) from India, presented by Captain W. Miller; two Hobbies (*Falco subbuteo*) from France, presented by M. P. A. Pichot; a Peregrine Falcon (*Falco peregrinus*), captured at sea, off the coast of Florida, presented by Captain J. Smith; a Knot (*Tringa canutus*), British, presented by Mr. C. Whymper, F.Z.S.; a Pallas's Sand-Grouse (*Syrrhaptes paradoxus*) from Fifeshire, N.B., presented by Mr. John Duncan; an Egyptian Vulture (*Neophron percnopterus*) from North Africa, presented by Captain A. Kent; a Polar Bear (*Ursus maritimus*) from the Arctic Regions, a Ruffed Lemur (*Lemur varius*) from Madagascar, a Yak (*Poëphagus grunniens* ♀) from Tibet, deposited; a Blue Jay (*Cyanocitta cristata*) from North America, purchased; a Crested Pigeon (*Ocyphaps lophotes*), a Bolle's Pigeon (*Columba bollii*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBSERVATION OF FAINT MINIMA OF VARIABLES.—Mr. S. C. Chandler has followed his Catalogue of Variable Stars by another much-needed piece of work. Pointing out how deplorably deficient is our knowledge of the light-changes of such variables as become too faint for ordinary telescopes to follow them, he strongly urges (*Astr. Journal*, No. 183) upon the possessors of the great modern refractors that they could not better employ their high optical powers than in this neglected field. And in order to afford the greatest possible inducement for them to undertake such a research, or at all events to remove as many hindrances out of their way as possible, he has supplied the data necessary for identification for some sixty-nine or seventy stars, the minimum for which is fainter than the 12th magnitude, together with a hypothetical ephemeris for the time of minimum for those stars which are likely to pass through that phase during the next fourteen months. The time of minimum has been assumed to precede that of maximum by 0·45 of the period. Mr. Chandler believes that he has discovered a curious relation between the form of the light-curve and the period. He does not, however, give this relation, but expects that the minima will fall earlier than predicted for stars with periods of from five to ten months or longer than thirteen months, but that in stars with periods from ten to thirteen months the minima will probably fall a little late. Argelander's method of observation is recommended.

OXYGEN LINES IN THE SOLAR SPECTRUM.—M. Janssen has made a very toilsome, not to say somewhat dangerous, expedition in order to determine whether the groups of lines seen in the solar spectrum, and which he had shown to belong to oxygen, were wholly due to the influence of our atmosphere, or partly due to the absorption of oxygen in that of the sun. He therefore resolved to ascend Mont Blanc at a late period in the season, when the cold would be sufficiently intense to secure the absence of any appreciable amount of water vapour from the atmosphere. The station chosen was that of Les Grands Mulets, on account of the cabin there, which would afford shelter to the observers during the period that it might be necessary to continue the observations. The ascent was made on October 13, and the following day was devoted to the adjustment of the instruments. October 15 and 16 were most fortunately very fine days, and the observations were made under the most favourable conditions. The result was to show that both the bands and lines of oxygen, as identified by M. Janssen in the solar spectrum by his previous experiments, are due entirely to the earth's atmosphere. The system of bands—those in the red, in the yellow, and the blue, the intensity of which varied with the square of the density of the absorbing oxygen—was altogether wanting, and the groups of dark lines, viz. A, B, and α, which M. Janssen had found, in the experiments above referred to, to vary as the simple density, were so much enfeebled as to leave little doubt but that they, too, would disappear could we wholly eliminate the influence of our atmosphere. Of course this result does not prove the absence of oxygen from the sun, but merely that it does not show its presence by the same characteristic bands and groups of lines as it does in the case of our own atmosphere.

NEW MINOR PLANETS.—Herr Palisa has discovered three new minor planets, No. 279 on October 25, No. 280 on October 29, and No. 281 on October 31. Of these No. 279 may possibly prove to be Medusa, No. 149; and No. 280 was at first thought to be Oppavia, No. 255, but is more probably a new planet.

COMETS FAYE AND BARNARD.—The following ephemerides for these objects for Berlin midnight are in continuation of those given in NATURE, vol. xxxviii. p. 626:—

1883.	Comet 1888 *d* (Faye). R.A. h. m. s.	Decl. ° ′	Comet 1888 *e* (Barnard). R.A. h. m. s.	Decl. ° ′
Nov. 11	8 6 9	5 49 9 N.	4 30 32	0 24·4 S.
13	8 7 39	5 27·6	4 17 59	1 5·4
15	8 8 59	5 5·6	4 4 55	1 46·7
17	8 10 11	4 44·1	3 51 26	2 27·5
19	8 11 15	4 22·9	3 37 39	3 7·4
21	8 12 9	4 2·4	3 23 43	3 45·7
23	8 12 56	3 42·4	3 9 45	4 21·9
25	8 13 34	3 23·1 N.	2 55 56	4 55·3 S.

DISCOVERY OF A NEW COMET.—Mr. E. E. Barnard, Lick Observatory, Mount Hamilton, discovered a new comet on October 30 (local time). Place at October 31·0399 G.M.T., R.A. 9h. 43m. 22·2s.; Decl. 15° 18′ 52″ S. Daily motion, R.A. + 1m. 32s.; Decl. 9′ *n*. Physical appearance: slightly elongated; 1′ in diameter; 11th magnitude, or fainter; strong central condensation.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *NOVEMBER* 11–17.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 11

Sun rises, 7h. 14m.; souths, 11h. 44m. 13·2s.; sets, 16h. 14m.: right asc. on meridian, 15h. 8·4m.; decl. 17° 38′ S. Sidereal Time at Sunset, 19h. 39m.

Moon (at First Quarter November 10, 16h.) rises, 14h. 7m.; souths 19h. 0m.; sets, 0h. 2m.*: right asc. on meridian, 22h. 25·5m.; decl. 13° 20′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 27	10 39	15 51	14 2·6	10 10 S.
Venus.....	10 13	13 57	17 41	17 21·7	24 35 S.
Mars......	11 50	15 38	19 26	19 3·0	24 11 S.
Jupiter....	9 10	13 14	17 18	16 39·0	21 41 S.
Saturn....	22 41*	6 7	13 33	9 30·8	15 41 N.
Uranus....	4 22	9 49	15 16	13 13·2	7 6 S.
Neptune..	16 51*	0 36	8 21	3 57·9	18 44 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
12	ψ^3 Aquarii	5	17 9	18 24	103 267
16	ξ^2 Ceti	4	21 11	near approach	10 —

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Nov. 15,	1 8	*m*
Algol	3 0·9	40 31 N.	,, 14,	4 33	*m*
			,, 17,	1 22	*m*
λ Tauri	3 54·5	12 10 N.	,, 11,	1 15	*m*
			,, 15,	0 7	*m*
T Monocerotis	6 19·2	7 9 N.	,, 14,	3 0	*M*
R Canis Majoris	7 14·5	16 12 N.	,, 15,	22 34	*m*
			,, 17,	1 49	*m*
R Leonis	9 41·5	11 57 N.	,, 16,		*m*
R Virginis	12 32·8	7 36 N.	,, 15,		*m*
U Virginis	12 45·4	6 7 N.	,, 16,		*m*
β Lyræ	18 46·0	33 14 N.	,, 14,	18 0	*M*
η Aquilæ	19 46·8	0 43 N.	,, 17,	18 0	*M*
S Sagittæ	19 50·9	16 20 N.	,, 11,	19 0	*M*
R Sagittæ	20 9·0	16 23 N.	,, 15,		*m*
U Capricorni	20 41·9	15 12 S.	,, 15,		*M*
Y Cygni	20 47·6	34 14 N.	,, 13,	3 0	*m*
			,, 16,	3 0	*m*
δ Cephei	22 25·0	57 51 N.	,, 12,	19 0	*m*

M signifies maximum; *m* minimum.

Nov.	h.	
13	13	Venus at greatest distance from the Sun.
17	4	Mercury at greatest elongation from the Sun 19° east.

Meteor-Showers.

The principal periodic shower of the week is that of the *Leonids*, max. November 14, radiant R.A. 149°, Decl. 22° N.; but no great display is to be expected this year or for several years to come. Other showers are as follows:—

	R.A. °	Decl. °		
From Lynx	125	40 N.		Swift; streaks.
Near ξ Ursæ Majoris	165	30 N.		,, ,,

GEOGRAPHICAL NOTES.

THE great Constantine Medal was awarded this year by the Russian Geographical Society to Prof. Romanovsky for his geological work in Russian Turkestan. For more than five years the learned Professor explored various parts of Turkestan, and thus laid the first foundations for the geological knowledge of this region. His first work, "Materials for the Geology of Turkestan," was published in 1876, and it contained the description of eighty-eight species of fossil animals (of which thirty-four were new species) and fourteen species of plants belonging to the Carboniferous, Triassic, Jurassic, and Chalk deposits of Turkestan; the Silurian and Devonian deposits of the region being so greatly metamorphosed as to have most of their fossils destroyed. This first work was soon followed by papers contributed to the *Verhandlungen* of the St. Petersburg Mineralogical Society, in which papers Prof. Romanovsky described the fossils of the Ferghana deposits (Upper Chalk, characterized by their richness in *Ostreæ*, some of which belong to new genera), and the Sarvadan brown-coals, which contain the new lizard *Brontozoum tianschanicum*, and are of the same age as the Connecticut Trias Sandstone. The second part of the "Materials for the Geology of Turkestan," published by Prof. Romanovsky, contains the description of all the palæontological collections gathered in Turkestan by MM. Mushketoff, Syevertsoff, Barbot-de-Marny, and Okladnykh; and no less than 144 species of fossils (of which forty-nine are new) have been described in this second instalment of the "Materials." It was precisely the palæontological work of Prof. Romanovsky which enabled M. Mushketoff to arrive at the remarkable general conclusions as to the great features of the geology of Turkestan, which are embodied in his capital work, "Turkestan," and which rendered it possible for both geologists to draw up the geological map which illustrates it.

AT the same sitting the great medal of Count Lütke was awarded to Th. P. Köppen for his work in botanical and zoological geography. His work on the distribution of Conifers in Russia, published in 1885, is an exhaustive inquiry into the subject, and his numerous monographs on the distribution of insects in Russia, as well as of the squirrel and the stag, as also his monographs about the Siberian cedar, the Scotch fir, the larch, the *Juniperus*, and so on, are most valuable contributions to the botanical and zoological geography of Russia; while his last work on the birth-places of the Indo-Europeans and the Finns and Ugrians (published in the Russian Journal of the Ministry of Public Education for 1886), although made in a new direction, is an important contribution to this much debated subject. Large gold medals were awarded to Prof. M. M. Kovalevsky for his "Modern Customs and Old Law: the Customary Law of the Ossetes"; to Prof. Vs. Th. Miller for his "Ossetian Studies"; and to M. Pirogoff for a statistical work about Kostroma. A small gold medal was awarded to L. P. Zagursky, to whom the ethnography of the Caucasus is indebted for so many valuable works, and all ethnographers will be grateful for his endeavours to save from oblivion and to continue the works of Baron Uslar, which undoubtedly are the most serious researches ever made into the study of Caucasian languages. Gold medals were also awarded to A. S. Vilkitzky for his determinations of the length of pendulum on Novaya Zemlya and at Archangelsk; to N. Y. Dinnik for explorations in Northern Caucasia; and to D. Bulgakovsky for a manuscript work on the inhabitants of the Pinsk marshy tracts. Nineteen silver medals were awarded for various geographical works of less importance.

ON THE ORIGIN AND THE CAUSATION OF VITAL MOVEMENT.[1]

II.

TO this end permit me to go a little into detail concerning nerves. Nerves are processes of nerve-cells composed of fibrils of immeasurable fineness, which, in the so-called axis cylinder of the medullated nerves, are united by a stroma inside a very fine membrane called the axolemma. In proportion to the microscopic dimensions of the ganglion cells, of which the separate nerve-fibres form a part, these latter are for the most part enormously long, many as long as our arms and legs, and that is one of the reasons why the perception of the unicellular nature of the nerves made way but slowly. In fact, it was not easy to accustom oneself amid the microscopic swarm of cells, to find single ones so grown in length that they could be wound about us like a cocoon thread. As it is the task and function of the motor nerves to lead towards the periphery the impulses sent out by their ganglion cells in the spinal cord, their activity always admits of ready perception through the muscular twitching. Even when the nerve is divided and artificially excited at the peripheral end, the muscles betray it. On the other hand, no visible physiological reaction is found at the central origin of the motor fibre when stimulated at the periphery, so that at first we were quite in darkness as to whether in general it conducted centripetally. Nature, however, has presented us with a contrivance by which we are enabled to demonstrate the possibility of such an inverted or centripetal nerve-conduction. The contrivance consists in the branching division of nerve-fibres, so frequently found in muscles, as will at once be seen in a preparation from a frog (Fig. 3). In many muscles these branchings are so arranged that we can use them for an experiment as simple as it is conclusive of nerve-conduction in both directions.

FIG. 3.

FIG. 4.

In the *gracilis* muscle of the frog the nervation is fashioned in the manner displayed schematically upon this diagram (Fig. 4), and in more detail on the following (Fig. 5). In reality the arrangement is like this. Now, if I cut up the muscle according to this diagram (Fig. 6), we get at the tip, *z*, nerve-fibres, which are connected with the muscle-fibres at *o* and *u* only by the branchings at the points *x x*, but which in life served only for the parts of the muscle removed at *f* and *f'*.

An experiment[2] will now convince you that nerves severed from their own muscle-fibres act quite well backwards upon those placed centripetally to them, which they can only do if nerves can also conduct centripetally, and so long as a path is preserved for this through the branchings. If we cut out the neighbourhood of the branchings, it is all over with the reaction of the muscle.

We can make another experiment on the same muscle.[1] We see that when we excite the lower tip of the muscle, only the lower portion twitches and not the upper. The two portions are in fact connected only by means of a very short tendon, the so-called *inscription*, which passes completely through the muscle (*i i* in Fig. 5), so that it really consists of two muscles. If the nerve common to both is stimulated at any point, then both parts of the muscle contract, but if the muscle substance itself is stimulated, then the contraction travels no further from the place where the stimulus was applied than to the limits of continuity of the muscle-fibres.

The power of motor nerves to conduct in both directions is certainly of general significance in regard to the inner mechanism of nerves, but we have only approached it here, because it was necessary for the decisive proof of muscular irritability, as obtained in our last experiment with the *m. gracilis*. Whenever a muscle is provided with a nervation and branchings of the separate nerve-fibres like that of the *gracilis*, some group of muscle-fibres can serve to indicate whether a stimulus has affected this alone or the nerves lying in it as well. If nerves are present at the point of stimulation, and if the agent was at the same time a nerve stimulus, this is shown by the simultaneous contraction of distant parts which are accessible by means of the nerve's power of conducting in both directions. In cases where we can see the coarser nervation, the indirectly produced contractions can be predicted, and these form so certain a criterion of neuro-muscular excitations that by them the presence of the finest nerves may be proved, whose existence might otherwise be quite incapable of proof by any other means, as, for instance, by the use of the microscope. If these contractions are wanting, as was the case in our experiments with the lower end of the muscle, we know that either the spot stimulated is free from nerves, or that the stimulus employed was ineffectual as to the nerves, and affected the muscle substance exclusively. In both cases, then, independent irritability is proved for those muscle-fibres which were directly excited and contracted.

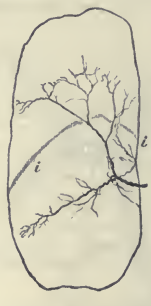


FIG. 5.

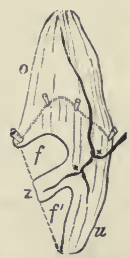


FIG. 6.

Now, since we have just employed an electric stimulus which is equally effectual on muscle and nerve, it follows that we had to do with the first case; that is to say, the muscle showed itself free from nerve at its end. We have reason for specially bringing forward this experimental proof of the absence of any kind of nerves in large tracts of muscle, because it compels those who in spite of all assume the presence of nervous matter in certain microscopic disks and striæ of the muscle-fibre as a whole, to deny that this supposed nervous element possesses any power of conducting in both directions or any irritability at all; for in fact it is not possible to excite the motor nerve of a muscle-fibre by any stimulus whatever applied to the actual terminations of the nerve within the fibre. The facts besides combine to prove, as need hardly be said, yet another proposition—they prove at the same time that pure muscular excitation does not travel back to the nerves.

This may be shown still better with the small pectoral muscles of the frog's skin than with the *m. gracilis*. We need only dissect it in the manner shown in the drawing (Fig. 7), and stimu-

[1] "On the Origin and the Causation of Vital Movement (*Ueber die Entstehung der vitalen Bewegung*)," being the Croonian Lecture delivered in the Theatre of the Royal Institution on May 28, 1888, by Dr. W. Kühne, Professor of Physiology in the University of Heidelberg. Continued from vol. xxxviii. p. 629.

[2] Kühne, "Ueber das doppelsinnige Leitungsvermögen der Nerven." *Zeitschr. f. Biol.*, vol. xxii. p. 305. To demonstrate the experiment on the *gracilis*, the muscle was fixed on a white piece of cork by needles, and held by elastic holders, and its image thrown on the wall highly magnified by the so-called Krüss lantern.

[1] Kühne, *ibid.*, pp. 312, 324.

late the spots *n* and M: if we stimulate *n*, everything contracts; if M, the excited half only.

The preparation which you now see (corresponding to Fig. 2), and which shows the nervation of the very thin muscle with all the nerve-endings stained dark with gold, makes that relation clear, for here again in truth the result of morphological research is in gratifying accordance with results obtained experimentally. The muscle is seen to be for the most part free from nerves; indeed the entire nervation with all the nerve-endings might be

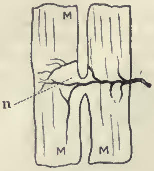


FIG. 7.

said to be formed of one nerve-*line* only, if we disregard the few digressing fibres, which, again, in part are not motor.

Under rather higher powers we see the nerve-endings proper (Fig. 8), the distinct demonstration of which by means of the gold method has now been achieved, in much the same way as here, in all the classes of vertebrates with the exception of the osseous fishes. In all cases these decisive preparations have proved that the vastly preponderant number of the muscle-fibres is entirely free of nerves, and that the nerve-endings are

FIG. 8.

confined to very small spots which we term fields of innervation. Most muscle-fibres have only one field of innervation, very long ones occasionally several, at the most eight. Thus the assumption, opposed to the idea of independent irritability, that muscle-substance is well nigh completely riddled with nerves, is refuted and rejected from the morphological side also.

From the absence of nerves in long tracts of muscle-fibre we immediately conclude that the latter shares with nerves the faculty of independently propagating its own excitation. This is what the beautiful microscopic observations of Sir William Bowman[1] on insects' muscles long since led us to suspect. As in the nerve, so in the muscle, conduction takes place in every direction, and as the field of innervation almost without exception occupies a median position during a normal contraction, the conduction takes place in both directions, towards the tendinous ends. By way of distinction the velocity of conduction is, according to species, temperature, &c., three to ten times less than von Helmholtz fixed it for nerve. As conduction in irritable tissues means nothing else than that one excited spot becomes the stimulus for the adjoining portion at rest, the independent irritability of the muscle-fibre comes into employment in every movement and during the entire duration of life; from the moment that the field of innervation becomes active all the muscle substance remains left to itself, and until the contraction is ended must be regarded as independent and acting in response to its own direct excitation.

Once clear on the fundamental question, and sure as to the method we have to employ in order to stimulate according to choice either muscle- or nerve-substance alone, or both together, we may seek to determine in what respect the irritability of the two components of the motor machine differs. The differences as regards chemical stimulation appear very great; in respect of electric, thermic, and mechanical, on the other hand, only quantitative. However, under chemical stimulation, according to Hering's classical researches,[2] a point formerly overlooked comes into consideration—namely, the complication introduced by the electromotive behaviour of the tissue, an automatic electrical stimulation one might say. When stimulation takes place by moistening the transverse section with conducting liquids, it is indeed difficult, if not impossible, to trace the chemical factor in presence of the electrical. Gaseous stimuli alone, like ammonia, have thus far remained free from the suspicion of acting electrically. To these a few others of similar action, such as bisulphide of carbon,[3] have been added, and such as are conveyed to the muscle by blood-vessels, and bathe the fibres from all sides. With these in particular we may class distilled water, which is excessively destructive to irritable substances, von Wittich[4] being the first who showed how strongly it stimulates muscles, while killing nerves without excitation. But, again, with this kind of stimuli, we cannot at present tell whether they do not set up in the tissues, over narrow but numerous areas, excitatory electric currents, thus working only indirectly by way of auto-electric stimulation. And since, finally, the same might apply to the thermic and mechanical actions which likewise arouse demarcation currents in the muscle—that is, to all stimuli—we find ourselves in the presence of the possibility of reducing all irritability to a reaction to electrical processes, and of seeing vital electricity elevated into immeasurable importance.

The means by which muscle may be stimulated interests us, in the first place, on this account—to ascertain, once for all, how it procures its excitation *in life*, or what may be the action of nerve upon it. Did we know that, we should have grasped at the same time the nature of nervous activity.

Nerves end blindly in the muscles; as a rule they are not even finely pointed, and still less do they spread out diffusely in such a way as might make the true ending difficult to find. They end quite distinctly. But the ends always lie beneath the sarcolemma, in such a way that no foreign tissue intrudes between them and the muscle, so that what is fluid in the muscle can directly moisten the nerve. The sublemmar nerve is clothed with nothing else than the axilemma. The nerve never penetrates into the depths of the muscle-substance; on the contrary, it remains confined to the sublemmar surface of the contractile cylinder or prism. Each nerve-end consists of several branches, like antlers, arising by division, which together

[1] "On the Minute Structure and Movements of Voluntary Muscle," Phil. Trans., 1840, p. 457; and "Muscle—Muscular Motion," in the "Cyclopædia of Anatomy and Physiology," edited by B. B. Todd, vol. iii. 1847, pp. 506–530.

[2] "Ueber direkte Muskelreizung durch den Muskelstrom," Vienna, *Sitzber. k. Akad.*, vol. lxxix., Abth. 3, 1879.

[3] "Ueber chemische Reizungen; nach Versuchen von. stud med. C. Iani." *Untersuch. aus der Physiol. Instit. der Univ. Heidelberg*, vol. iv. 1882, p. 266.

[4] "Experimenta quaedam ad Halleri doctrinam de musculorum irritabilitate probandam instituta," Königsberg, 1857; and *Virchow Archiv*, vol. xiii. 1858, p. 421. In these papers, with the discovery of the excitation of muscle by distilled water, appears without doubt the first fact which overthrew the old theory of the equal irritability of muscle and nerve.

form the terminal nerve-branch. Apart from the form of the antlers, this short description is exhaustive for many animals, since neither in the sublemmar nerve need any special additional structures occur, such as nuclei, nor any kind of modification of the muscle-substance in the field of innervation. There is much to indicate that the nerve-fibre proper, or axis-cylinder, does not change its constitution in passing through the sarcolemma, still it is to be remarked that the twigs of the terminal branches, although as long as they live often apparently longitudinally striated, have not yet, even in the most favourable stainings, been found to present the general fibrillar structure of nerves.

According to these results of morphological research, it appears that contact of the muscle-substance with the non-medullated nerve suffices to allow the transfer of the excitation from the latter to the former. The only strange thing is that in reversed order excitation of the muscle never extends to its own nerve. This is still stranger because, according to Matteucci's well-known discovery, a *foreign* medullated nerve simply laid upon the muscle is powerfully excited by the contraction—so powerfully that the smallest contracting muscle barely touching it in more than a mere point excites the strongest nerve, while, on the other hand, we never see muscles excited by nerves which are merely pressed against them.

In the investments, then, of the nerve and the muscle-substance appears to exist one of the elements which admits the neuro-muscular excitation *exclusively* to the field of innervation, and among those investments it need not be the medullary sheath. The delicate membranes of the sarcolemma and neurilemma suffice, for muscle cannot be excited by superimposed *non*-medullated nerves. At any rate, I have tried in vain to excite muscles by the most intimate contact of the fine terminal ramification of the optic nerve in the retina or the *n. olfactorius* from the pike, or even the delicate nerves of Anodonta, by stimulating these non-medullated nerves.

If we imagine the activity of the nerve to start with a chemical process, and that a chemical stimulant, as du Bois-Reymond[1] once suggested, is, at the same time, secreted in contact with the muscle, we understand very well the necessity of direct contact, and in this case it would suffice if the sublemmar nerve were to run in *any* form for a short distance under the sarcolemma. The branching then would mean the enlarging of the contact. But however rich and intricate the ramifications may be, we can by no means say they display throughout the principle of increase of superficies; on the contrary, they are often astonishingly poor and small. As concerns their form, they are *not* irregular, but so strikingly uniform that this point deserves particular attention as being apparently indispensable for innervation.

Instead of describing the forms, allow me to show you the object itself in a selection taken from the most diverse vertebrates. First from the Amphibia (Fig. 9): rod-like branchings with long outstretched twigs, a form which crops up again in a remarkable way in many birds. The rule here is asymmetry of the divisions: all the twigs have the form of a bayonet.

The following preparation shows the termination in the dog (Fig. 10). Here the branches are crooked, and hence quite divergent, so that the points of agreement with the form of the Amphibia are at first overlooked. But if we examine the divisions, you will remark that these are again unsymmetrical and give off branches whose ends lie very diversely removed from the common place of origin. The ends are, as a rule, turned towards each other, and often so approximated that it is at times troublesome to find the gaps between them, and if they do not lie in the same plane they appear to be united into a ring. In other cases one end overlaps the other, but we then find that all the points of the branches which are turned towards each other lie at unequal distances from the nearest bifurcation. This law holds good in all the thousand cases of motor endings thus far observed, and shows a strict order in the apparent chaos of these structures. And yet among the organic forms there is scarcely one which varies so much in other respects, and often is so inextricably complicated as this.

The drawings (Fig. 11, from the muscles of the guinea-pig, and Fig. 12, of the rat) and a preparation from a lizard (Fig. 13) may serve as a voucher for the truth of the above statement. We see there everywhere the hooks making their appearance with a short and a long claw, like the swivel we hang our watch on in the pocket.

[1] "Gesammelte Abhandlungen zur allgemeinen Muskel und Nervenphysiologie," vol. ii., p. 700.

The voluntary muscles of all vertebrates and of many invertebrates consist of fibres, the contents of which are pefectly regularly disposed in layers and transversely striped. For shortness, this striped mass may be called "rhabdia." This it is which has been universally identified with the contractile substance. But it has been ascertained that in many cases the nerve-ending does not come at all into direct contact with the rhabdia, but with another mass, which is highly nucleated and of pap-like softness. This latter is unstriped, and has all the appearance of protoplasm. It occurs in very varying quantity under the nerve-antler; in Amphibia, where the sublemmar nerves run out in a long course, it is not apparent as a separate layer, but it occurs more abundantly in the same measure that the branchings retract, and the field of innervation becomes smaller. At first it is found chiefly between the twigs, in the intervals of the branching, and then in the form of a sole, which, among the much-contorted branchings of reptiles and mammals,

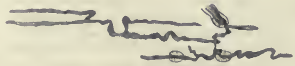


FIG. 9.

FIG. 10.

grows thicker, till it sometimes in some nerve-eminences forms quite a thick cushion. Since we have succeeded in making the nerve-endings visible in uninterrupted series of very fine sections of mammalian muscle stained with gold, there can no longer be any doubt that the complete separation of the sublemmar nerves from the rhabdia by measurable layers of sole-protoplasm, though not the rule, is yet by no means rare, and that many muscles possess no other sort of nerve-endings than such as these with apparently indirect contact.[1]

It would be difficult to understand why the innervation should have in some muscles, as in the Amphibia, no intermediate layer, while having in the majority of cases an interrupted layer, and in others a continuous layer of varying thickness to traverse. But when we consider what the substance of the sole is, of what it consists, how it is distributed, and when we know its origin, it appears that it is identical and stands in continuous connection with the long-known second constituent of muscle-

FIG. 11.

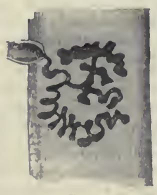


FIG. 12.

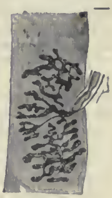


FIG. 13.

fibres, of which, as well as of the rhabdia, the fibres are composed. It is that substance, considered by Max Schultze to be the protoplasmic remnant of the cells composing muscle, which occurs in greatest amount around the nuclei of muscle, and extends in long threads throughout the entire muscle-fibre. So many transverse connections occur on the very numerous stronger and finer nucleated threads that the whole mass, called sarcoglia, becomes a trellis-work almost of the same fineness as the better-known transverse striation of the rhabdia, and everywhere surrounds and interpenetrates the latter. This minute internal structure of muscle has only become at all well known since the introduction of gold staining, thanks especially to Messrs. Retzius and Rollett.[2] Had it been suspected earlier, and had we appre-

[1] Kühne, *Verhandlungen des Naturhist.-medicinischen Vereins zu Heidelberg*, Neue Folge, vol. iv. pp. 4, 5.

[2] G. Retzius, "Biologische Untersuchungen," 1881. A. Rollett, "Untersuchungen über das Bau der quergestreiften Muskelfaser," *Wien. Akad. Denkschr.*, vol. xlix., 1885.

ciated the volume of the sarcoglia, whose existence is thereby shown, and which rivals that of the rhabdia, we might have studied this component of muscle in its physiological relations to contractility, as well as in its morphological and genetic relations, which are the only ones yet known.

If now, in many cases, it appears that the nerve comes in contact only with the surface of a thick layer of sarcoglia, while the rhabdia everywhere is covered by very fine layers of the latter, whose absolute absence in the field of innervation can nowhere be demonstrated, we have to conclude that in general the nerve does not act directly upon the rhabdia, but only on the sarcoglia. This at once gives the latter a physiological interest. We have to ask whether the glia is the medium that conducts the stimulus between nerve and rhabdia, or whether it is itself the contractile element, while the rhabdia has a signification other than that formerly attributed to it when we were completely ignorant of the glia.

All contractile substance requires the co-operation of an elastic element. Where is this to be found in the muscle-fibre? The envelope of the sarcolemma, which is certainly elastic, but delicate, and whose mass is almost infinitesimal compared with that of the muscle-fibre, cannot satisfy the requirement; but more solid structures freely distributed in the paste-like sarcoglia could perhaps do so, and such we find in the rhabdia, in the form of prismatic particles, ranged with such constancy and with such regularity longitudinally and transversely, that we may hold them to be the elastic element. Then the sarcoglia would become the contractile element, and the nerve would have an easier task.

I could wish that this view might be accepted as an hypothesis. As far as I can see, it does not contradict experience, for it only puts back the muscle nearer to the protoplasm and to all that is contractile, and so far coincides with experience that we find muscles in the same measure less elastic and more sluggish in protoplasmic movement the richer they are in sarcoglia, as in the case of the red muscles, nucleated and rich in glia, which contract more slowly but with greater power than the white muscles, poorer in glia, which are quick and spring-like, and also the sluggish embryo muscles, in which glia predominates because as yet but little protoplasm has been converted into rhabdia; and further the cells of unstriped muscle-fibre, which are wanting in the regular transverse striation, and contain, as it appears, besides more abundant glia, an elastic material of special form and arrangement.

The hypothesis would be overthrown if contractile fibrils were found in which no sarcoglia was to be detected. But even in the finest fibrils of *Stentor*, the structure of which Bütschli [1] has recently elucidated, we must hold the significance of punctated transversely penetrating indentations to be protoplasmic, and we can therefore scarcely expect ever to find a contractile thread in which nothing whatever should be found of the primitive contractile material such as it everywhere exists.

Of late, this view [2] has been defended from the purely morphological side,[3] on the strength, namely, of the very fine reticular structure of protoplasm to which more attention is being paid, and which is demonstrable on objects of all grades of organization. Protoplasm, in fact, is not so formless as at first appeared, but shows a structure comparable with nothing better than with the appearance presented by a transverse section of muscle with its glia framework stained with gold. We may expect that these reticular structures, whose consistency appears to vary extraordinarily, will some day lead to the establishment of a fruitful hypothesis of the inner mechanism of protoplasmic movement, in place of that held hitherto, which affords no glimpse into the essence of vital mechanical work.

Compared with this larger problem, that of the causation of vital movement appears the more accessible of the two, the latter being considered as a physiological inquiry after the constitution of the normal stimulus by which work is done. Perhaps, indeed, the answer is to be looked for from the most perfected organization of muscle, where the iniatory process is localized by a distinct nerve-ending, rather than from the primitive organization, where the excitation may set in at any place, and lies in the protoplasm itself. We know distinctly that the muscle-wave begins in the field of innervation, for we

[1] "Dr. H. G. Bronn's Classen und Ordnungen des Thierreiches," neu bearbeitet von O. Bütschli, Leipzig und Heidelberg, 1888, vol. i. p. 1298.

[2] Kühne, "Neue Untersuchungen über motorische Nervenendigung," *Zeitschr. Biol.*, vol. xxiii. pp. 88-95.

[3] A. van Gehuchten, "Étude sur la structure intime de la cellule musculaire striée," *La Cellule*, vol. ii. p. 289.

have long seen the natural contraction in the interior of transparent insect larvæ starting from the nerve-eminences. We know this also from the experiments of Aeby, who followed the muscle-wave myographically from the nerve-line onward, and now we are able to display the beginnings of the contraction as local thickenings at the point of attachment of the nerves caught and fixed by sudden hardening. Since the nerve grasps the muscle in a restricted region, it expends its action upon this exclusively; that which follows on as muscular activity is the nerve's work no longer.

Galvani and his successors for more than a century suspected that nervous forces were electrical, and, in reality, the celebrated champion of electro-physiology in our day has been able with the galvanometer to render the excitation of nerves, unattached to muscles or ganglion-cells, evident as the negative variation of the natural nerve-current, to cause movement of a magnetic needle instead of a muscle, or to put the needle in the place of sensation. After this no consideration of the nature of nervous activity is conceivable which does not take into consideration this discovery of du Bois-Reymond's—least of all where the nerve has to excite something with which it is not fused, like muscle, but which it only touches, and that not directly, while still invested by the axolemma. Only during excitation, as Ludimar Hermann has taught us, are electric currents issuing from the nerve through its conducting surroundings, in which the course of these currents of action is to be estimated from the duration of the negativity of the nerve-tract excited, and from the speed of propagation of the nerve-wave, if we know the conductor and the disposition of the nerve. The motor ending fixes the latter, and so peculiarly that we can only presuppose from it a furthering of the excitor effects of the currents of action.

The currents of action of muscle, whose electromotive behaviour agrees so wonderfully with that of nerve, have long been proved to produce excitor effects, although only powerful enough to act upon nerves; but there are also, under certain conditions discovered by Hering, such effects from nerve to nerve.[1] Is the possibility, we may hence ask, to be excluded, of one muscle exciting another, and is it quite impossible that a nerve only throws a muscle into contraction by means of its currents of action?

The first question we can answer. I will do so by a simple experiment. Two muscles, the nerves of which are disposed of by poisoning with curare, need only to be pressed together transversely over a narrow area to make a single muscle of them of double length, in which the stimulation and contraction are propagated from one end to the other. Since the transference from one muscle to the other is done away with as soon as we bring the finest gutta-percha between the muscles as an insulator, or gold-leaf as a secondary circuit, the first muscle must have excited the second electrically.[2]

[1] *Sitzber. der k. Akad. zu Wien*, vol. lxxxv. Abth. 3, 1882, p. 237.

[2] Kühne, "Secundäre Erregung vom Muskel zum Muskel," *Zeitschr. Biol.*, vol. xxiv. p. 383.

THE ASTRONOMICAL OBSERVATORY OF PEKIN.

IN the course of a lecture delivered before the Pekin Literary Society, on the Astronomical Observatory of the Chinese capital, Prof. Russell said that it is the oldest in the world. The oldest in Europe is that of Denmark, founded in 1576 by Frederick III., at which Tycho Brahé made his famous observations. The Royal Observatory at Paris was not opened till 1671, and that of Greenwich three years later. The Pekin Observatory was established in 1279, in the reign of Kublai Khan, the first emperor of the Mongol dynasty, and three of the original instruments yet remain. In 1378, these instruments were probably used in observing Halley's comet, and they will be used twenty-two years hence to witness its next return. If the visitor enters by a door in the south wall of the Observatory, he comes into a court running east and west. In this court are kept the three original instruments. There were four at one time, but the fourth, a celestial globe, has disappeared. Kuo Shouching, a Chinese astronomer, who flourished in the reign of Kublai Khan, was the maker of these. Before their construction, bronze astronomical instruments, which were made about the year 1050, were used, first at K'ai Fêng Fu, the capital of Honan, whence they were removed to Pekin. Kuo Shouching found these

worn out by age, and otherwise unsuitable, as the height of the Pole differed by 4°; and so he constructed four instruments, of which three now remain. In the east end of the court is the equatorial armillary, which is made of bronze, and consists of (1) a massive horizontal circle, held up at four corners by four dragons, each of which with one upraised palm supports the bronze circle, while round the other palm a chain is passed and fastened behind to a small bronze pillar,—the dragons are themselves works of art; (2) a double vertical circle firmly connected with the horizontal circle at its north and south points, and supported at its lowest point by a bronze pillar. On the vertical circle, which, like the other, is fixed, at a distance equal to the latitude of Pekin, that is 40°, are two pivots corresponding to the North and South Poles. Revolving round these pivots are two circles, one double, corresponding to the solstitial colure—that is, the great circle passing through the Poles and the solstices; the other single, corresponding to the equinoctial solure—that is, the great circle through the Poles and the equinoxes. Half-way between the Poles is another circle, which corresponds to the equator, the rim of which is let into the two colure circles. There is also another circle, making with the latter an angle of $23\frac{1}{2}°$, and corresponding to the ecliptic. Finally, inside these circles, all of which revolve together round the polar axis, there is another double circle, representing the polar circle or declination, and between the rims of this double circle revolves the hollow tube through which observations were made. It is probable that there were originally threads across the tube to define the line of sight. There are in the circles $365\frac{1}{4}°$—that is, a degree for each day in the year—and each degree is subdivided into divisions of 10′ each. When using this instrument the observer turned round the inner circle till the heavenly body was sighted in the centre of the tube, and then the distance of the star was read from the Pole on the polar circle, and its position on the equator by the equatorial circle. The complex construction was in some particulars of no use whatever: the ecliptic and one of the colures were useless. At the west end of the court are the other two instruments, the equatorial, or astrolabe, and the altitude and azimuth instrument. The former is remarkably simple in its construction. There is a fixed bronze circle placed parallel to the equator, and there is another double circle perpendicular to it, which moves round an axis passing through the centre of and perpendicular to the equatorial circle. Of course there is also the hollow tube for observation. This instrument is free from the clumsiness and complexity of the first-named instrument, and in the form of its mounting much more closely resembles those in use at present in all Observatories than the other instruments. The altitude and azimuth instrument consists of two circles, one horizontal and fixed, the other vertical and movable round an axis passing through the centre of the horizontal circle, and was used to observe the altitudes of the heavenly bodies and their distances from the north and south points. It is curious to observe that all these instruments are exactly similar to those constructed by Tycho Brahé, the great Danish astronomer, who was the first European to make astronomical instruments of metal. And thus we see that the Chinese anticipated European astronomers by at least three centuries, and that the former had at that very early date attained great proficiency both in the science of astronomy and the art of metal-carving. Verbiest, the Jesuit father, says that these instruments had, at the beginning of the present dynasty, fallen into disrepair. The truth was that they were far too clumsy, and were so heavy that it took several men to move them; and in some positions, from the profuseness of ornament, the stars could not be observed at all. Besides they had got out of position, and there were no appliances for righting them. It is more than probable that during the latter part of the Ming dynasty astronomy had been neglected, and so the old instruments fell into disuse. In the year 1670, so bad were the old instruments, that Verbiest was ordered to make six new instruments. It appears that when the high Ministers of State were ordered to go to the Observatory, and make certain observations, the calculations of Verbiest were verified as correct, while those of Wu Ming Hsuen, the Chinese astronomer, were proved to be wrong. And so Verbiest was intrusted with the calculation of the calendar and the construction of these instruments, which were of the same general character as the old instruments, but much more accurate, and more easily adjustable. The circles are divided into 360°, and each degree into six parts of 10′ each. By means of the diagonal scale and a movable divided scale, the observer could, on the new instruments of Verbiest, read to 15″, instead of 10′ as in the old instruments. Since the time of Verbiest two more instruments have been added—namely, an altitude and azimuth instrument, in the fifty-fourth year of Kang Hsi (1715), the other an equatorial armillary in the ninth year of Khien-Lung (1745). The former is said to have been a present from Louis XIV. to the emperor, but by some it is attributed to a German Jesuit, named in Chinese Kilian, and is remarkable for the total absence of ornamentation, and for the degrees being marked in foreign numerals. One of the most curious objects in the Observatory is the Qw'ei Ying T'ang, a three-roomed building lying a few yards to the south of the steps. It is evidently very old. In it is a stone slab 16 feet 2 inches long and 2 feet 7 inches broad, with a groove on both sides, and raised about 3 feet above the ground. At the south end of the slab is a brass pillar, which was formerly 8 feet high, but to which the present dynasty have added 2 feet more, extending to the roof, and at its summit is a small circular hole $\frac{3}{10}$ inch in diameter. Another brass pillar 3 feet 5 inches high stands at the north end of the slab. At noon the sun shines through the little hole in the roof, and throws an elliptical shadow of the sun on the slab, or on the brass pillar at the north end about the winter solstice. By observing the distance of the sun's image from the foot of the south brass pillar the solstices and equinoxes were determined. For instance, at the summer solstice the distance should be 2 feet 9 inches. The instruments of Verbiest are almost perfect of their kind, and will remain a lasting memorial of the industry and genius of the devoted missionary. At the time that he made them they were growing out of date in Europe. The telescope had already begun to be used largely in astronomical observations, and Verbiest must have known of it. The question arises, How does it happen that the Chinese, who in the thirteenth century were far ahead of Europe in the construction of these instruments, seem to have made no headway since? Many reasons can be given, but the chief one is that with them the main object of making astronomical observations was to regulate the calendar, and to give the time to the people; and for this accurate instruments were not needed, and their want was never felt. The greater problems of the heavens never seriously attracted the attention of the Chinese astronomers. The Astronomical Board consists of eighteen officials, with the fifth prince, an uncle of the emperor, at their head. There are, including students, altogether 196 persons attached to the Board. The privilege of becoming a member of this Board has become hereditary, though it is not of necessity so. The policy, however, pursued by the Board, of keeping secret the book tables of the sun and moon, and everything used in regulating the almanac, tends to encourage the hereditary principle. No one can see them but the relatives of the Board; and so vacancy after vacancy is filled up by members of the same family as the predecessor, and as the office is an honorary, and not a lucrative, one, the people do not grumble at their exclusion. The principal duty of this Board is to prepare the calendar, the most important book published in China. Besides astronomical facts, it gives the lucky and unlucky days, on the latter of which no Chinese will transact the least business. Another duty is to observe eclipses, and this appears to be the only occasion on which the instruments are still used. On every New Year's Eve, at midnight, astronomers from the Board seat themselves in the Observatory, and watch the way in which the wind blows a number of banners which are hung around. As the wind blows, so will the new year be. This year the wind blew from the north-east, the fortunate direction, and therefore it will be a year of long life and plenty.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following were elected Fellows of St. John's College at the annual election on November 5: W. N. Roseveare, B.A. (mathematics), Master at Westminster; E. H. Acton, B.A. (botany and chemistry); F. W. Hill, B.A. (mathematics); T. Darlington, B.A. (philology), University Scholar, London, Head Master of Queen's College, Taunton, author of "The Folk-Speech of South Cheshire"; H. F. Baker, B.A. (mathematics), bracketed Senior Wrangler in 1887.

SOCIETIES AND ACADEMIES.

LONDON.

Mineralogical Society, October 30.—Anniversary Meeting. —Mr. L. Fletcher, President, in the chair.—The Hon. Secretary, Mr. Scott, read the Annual Report, which showed that the

state of finances was not unsatisfactory. The excess of assets over liabilities was £247 6*s.* 11*d.*, and the expenditure on the Journal during the year had been £79 13*s.* 9*d.*, being about the average of late years. The elections to the Society during the year had been six, of whom one was an Associate. The Council had to regret the loss by death of one of their Corresponding Members, Prof. vom Rath, of Bonn, of whom an obituary notice by Prof. Lewis appeared in No. 37 of the Journal. Mr. Solomon Birkett, one of the Associates, had also died, having been killed by a railway train near Whitehaven. Three meetings had been held since the last anniversary, two in London, and one in Edinburgh.—The following is the list of officers and Council elected for the coming session:—President: R. H. Scott, F.R.S. Vice-Presidents: Rev. S. Haughton, F.R.S., and Dr. Hugo Müller, F.R.S. Council: Prof. J. W. Judd, F.R.S., Prof. E. Kinch, Prof. W. Ivison Macadam, J. J. H. Teall, Prof. A. H. Church, T. M. Hall, J. Stuart Thomson, Major-General C. A. MacMahon, Dr. C. A. Burghardt, H. A. Miers, R. H. Solly, and Dr. J. M. Thomson. Treasurer: Prof. T. G. Bonney, F.R.S. General Secretary: L. Fletcher. Foreign Secretary: T. Davies. Auditors: B. Kitto and F. W. Rudler.—The President then delivered an address which will be printed in the next number of the Journal.—The following papers were read:—On large porphyritic crystals of feldspar in certain basalts of the Isle of Mull, by T. H. Holland, communicated by Prof. Judd, F.R.S.—Note on the crystalline forms of silicon and carbon, by Prof. Judd, F.R.S.—On the supposed fall of a meteorite stone at Chartres, Eure-et-Loire, in September 1810, by the President.—On percylite from a new locality, by the President.—On various twins of calcite, by H. A. Miers.—A description of a new polarizing microscope, by Allan Dick, communicated by J. J. H. Teall.—Note on Colorado hydrophane, by Prof. A. H. Church, F.R.S.

Paris.

Academy of Sciences, October 29.—M. Janssen in the chair.—On the telluric spectrum at elevated stations, and particularly on the spectrum of oxygen, by M. Janssen. We print elsewhere (see "Our Astronomical Column") a brief account of M. Janssen's conclusions.—Decomposition of the phases of a continuous movement by means of successive photographic images taken on a tape or band of sensitized paper while being unrolled, by M. Marey. In order to complete the researches lately communicated to the Academy, the author here submits a strip of sensitized paper on which a series of images has been fixed at the rate of twenty per second. The process, as now perfected, will allow of successive images being taken of a man or an animal in motion, without the necessity of operating before a dark ground.—On the alleged subsidence of the ground in the centre of France, between Lille and Marseilles, by General Alexis de Tillo. The author traverses the conclusions arrived at by M. Goulier in his communication on this subject inserted in the *Comptes rendus* of August 20, 1888.—Survey of the Upper Javary, by Admiral de Teffé. A short account is given of the expedition undertaken in 1874 by Baron de Teffé and Don Guillermo Black, to determine the frontier between Brazil and Peru, where those States are conterminous in the valley of the Javary, a headstream of the Amazons.—On vapour-tensions, by M. Ch. Antoine. Some new relations between tensions and temperatures are worked out theoretically.—Photography applied to the study of electric discharges, by M. E. L. Trouvelot. During a series of experiments carried out for the purpose of studying the electric spark, the author has been led to repeat the interesting researches made in 1884 by M. E. Ducretet, and published in the *Comptes rendus* for December 1 of that year.—On the separation of cobalt and nickel, by M. Baubigny. Here the separation is effected by the method of the nitrites—On the chlorureted derivatives of acetylacetic ether, by M. Genvresse. The monochlorureted derivative of this ether was prepared by M. Allihn, and the bichlorureted by M. Conrad. But doubts having been thrown on the formulas determined by them, the author here resumes the study of these compounds.—On the employment of the bichloride of mercury as a therapeutic remedy and a prophylactic against Asiatic cholera, by M. A. Yvert. During his recent residence in Tonquin, the author successfully employed this preparation for the cure of cholera in doses varying from 0·02 to 0·04 gr. in twenty-four hours. Of forty-five patients so treated nine only succumbed, or about 20 per 100, the normal rate in that region as in Europe being 66 per 100. It was also administered to convalescents in districts where the epidemic had again broken out and had already made one victim. None of those who took this preventive medicine was attacked.—M. Raphael Dubois contributes an account of some new researches on the action of the chloride of ethylene on the cornea; and M. C. J. A. Leroy describes the normal form of the cornea of the human eye.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Colour: C. T. Whitmell (Lewis, Cardiff).—Die Korallenriffe der Sinaihalbinsel: J. Walther (Hirzel, Leipzig).—Table of Quarter-Squares: J. Blater (Trübner).—On the Use of Certain Organic Acids: J. F. Knott (Bale).—Fifty Years Ago in New Zealand: W. Colenso (Napier).—Nouveaux Mémoires de la Société Impériale des Naturalistes de Moscou, tome xv. liv. 3, 4, 5 (Moscou).—Alpine Winter in its Medical Aspects, 4th edition: T. Wise (Churchill).—Animal Physiology: W. S. Furneaux (Longmans).—Graphic Pictures of Native Life in Distant Lands: H. Leutemann, translated by G. Philip, jun. (Philip).—The Unknown Horn of Africa: F. L. James (Philip).—A Course of Elementary Mathematics, 2 parts: S. Ray (Lahiri, Calcutta).—The Zoo: Rev. J. G. Wood (S.P.C.K.).—Practical Geometry for Science and Art Students, 10th edition: J. Carroll (Burns and Oates).—Recherches Expérimentales et Théoriques sur les Équilibres Chimiques: H. le Chatelier (Dunod, Paris).—On the Pollination of Phlomis Tuberosa, L., and the Perforation of Flowers: L. H. Pammel (St. Louis)—Specimens of Eozoon canadense and their Geological and other Relations: Sir J. W. Dawson (Montreal).—Observations upon the Morphology of Gallus bankioa of India: R. W. Shufeldt.—Vaccination proved Useless and Dangerous: A. R. Wallace (E. W. Allen)—Himmel und Erde, 1 Jahrg. Heft. 1 (Berlin).—The Auk, October (New York).—Quarterly Journal of Microscopical Science, October (Churchill).—Contributions to our Knowledge of the Meteorology of the Arctic Regions, Part v. (Eyre and Spottiswoode).—Hourly Readings, 1885, Part iv. (Eyre and Spottiswoode).—The Geological Magazine, November (Trübner).—Journal of the Chemical Society, November (Gurney and Jackson).

CONTENTS.

THURSDAY, NOVEMBER 15, 1888.

FOSSILS OF THE BRITISH ISLANDS.

Fossils of the British Islands, stratigraphically and zoologically arranged. Vol. I. Palæozoic. By Robert Etheridge, F.R.SS.L. and E. (Oxford: Clarendon Press, 1888.)

GEOLOGISTS and palæontologists will hail with satisfaction the publication of the first volume of Mr. Etheridge's "Fossils of the British Islands," which has just issued from the Clarendon Press. Most of us who have been occupied in this department of science have long been expecting the appearance of this work, for those who have laboured much among fossils know full well the value of such a help to study.

When the late Prof. John Morris, in 1854, brought out the second edition of his "Catalogue of British Fossils," only about 4000 species were known, and yet so great was the need of some such aid, that the publication of that catalogue has been referred to as marking an epoch in British palæontological studies. During the thirty-four years which have since elapsed, palæontology has made most rapid strides, and Mr. Etheridge now estimates the number of British fossils at nearly 18,000 species. So vast an increase is of itself sufficient to show the necessity for some enlarged work of reference, which should bring the accumulated material within the grasp of the working palæontologist; and such is the scope of this catalogue of the "Fossils of the British Islands." Indeed, Mr. Etheridge tells us, in his preface, that it was to facilitate his work as Palæontologist to the Geological Survey of Great Britain that he, in the year 1865, commenced the manuscript of this tabular arrangement of fossils.

Those of us who have had the advantage of working for a number of years beside the author, and have been able to use these manuscripts, which were ever placed freely at our disposal, have learned to know their value, and to look forward with no little desire to the time of their publication.

The works of reference for fossil species, which have hitherto been available, are D'Orbigny's "Prodrome de Paléontologie" (1850), Bronn's "Index Palæontologicus" (1848), and Morris's "Catalogue of British Fossils" (1854). The first of these is divided into stratigraphical sections, with an index of species at the end. Bronn's "Index" is arranged alphabetically throughout, and the horizon of each species is indicated by letters referring to a table. The alphabetical arrangement is convenient for obtaining references to authors and descriptions, but the second reference for the horizon is troublesome. Morris's "Catalogue" is divided into zoological sections, similar to those adopted by Mr. Etheridge, and in each section the genera and species are placed alphabetically, with references to authors and descriptions, the chief horizon of each species being given on the right of each page.

In neither of these catalogues have either references or horizons been given in such detail as in the work now before us, which is not only a catalogue of all known British species, but also a table giving their full distribution in time, with voluminous references to the most important descriptions and figures; and the synonymy has been, as far as possible, brought up to date. The tabular form adopted by Mr. Etheridge has necessitated the division of the book into several stratigraphical groups; and consequently this first volume, which includes all the species recorded from Palæozoic formations, is divided into four sections: (1) Cambrian and Silurian; (2) Devonian or Old Red Sandstone; (3) Carboniferous; (4) Permian or Dyas.

In an appendix, three of these sections are repeated, so as to bring the work down to the year 1886.

At the head of each section the divisions of the formations which have been adopted are explained, and the important localities are noted at which the beds occur. Each page is arranged with vertical columns, one for every stratigraphical division, and on the left are the names of the genera and species, stars being placed opposite the latter, in the appropriate column, to mark their distribution. One column indicates those forms which pass up into the next higher section, while on the right the references are given.

The strata included in Section 1 are thus divided:—Cambrian, including (1) Harlech and St. David's beds, with Longmynd, Llanberris, and Bray Head; (2) Menævian; (3) Lingula Flags; (4) Tremadoc. Lower Silurian, including (1) Arenig; (2) Llandeilo; (3) Caradoc or Bala; (4) Lower Llandovery. Upper Silurian, including (1) Upper Llandovery; (2) Woolhope Limestone; (3) Wenlock Shale: (4) Wenlock Limestone; (5) Lower Ludlow; (6) Aymestry; (7) Upper Ludlow; (8) Tilestones or Passage Beds and Downton Sandstones.

The second section, Devonian or Old Red Sandstone, is divided into lower, middle, and upper beds, and the third or Carboniferous section is divided into (1) Calciferous Series; (2) Lower Limestone Shales; (3) Carboniferous Limestone; (4) Upper Limestone Shales (Yoredale); (5) Millstone Grit; (6) Lower Coal Measures; (7) Middle Coal Measures; (8) Upper Coal Measures.

The fourth section, Permian, is divided into (1) Passage Beds; (2) Rothliegende; (3) Marl Slate; (4) Lower Limestone; (5) Middle Limestone; (6) Upper Limestone.

Under each of the four sections the plants are first dealt with, the genera being in one alphabetical series; and then follow the animals, which are divided into the following groups, the genera in each being arranged alphabetically—namely, Rhizopoda (divided into Spongida and Foraminifera), Hydrozoa, Actinozoa, Echinodermata, Annelida, Crustacea, Arachnida, Myriapoda, Insecta, Polyzoa, Brachiopoda, Conchifera, Gasteropoda, Pteropoda, Cephalopoda, Pisces, Amphibia. In the appendix the Placophora and Heteropoda are separated from the Gasteropoda.

There can be no question but that in works of reference the alphabetical order is the simplest, and therefore the best, where it can be adopted; but in the present instance it was obviously necessary to make geological divisions; and the zoological groups which Mr. Etheridge has used are nearly the same as those of Prof. Morris's "Catalogue," which in practice has been found very easy for reference. This arrangement has the advantage also of bringing together the members of each of the groups, and the index of species supplies what further help is needed.

The zoological divisions being merely for convenience, little need be said about them; but, at the same time,

there are one or two points which may be noticed in passing. The Sponges are retained as a sub-group of the Rhizopoda in the sub-kingdom Protozoa. Possibly the first pages of this work were in type before the separation of the Spongida from the Protozoa, which is now generally accepted, was so strongly insisted on by biologists. It is well, however, for students to be reminded that the Sponges are regarded by most naturalists as presenting a higher type of organization than is found in the Protozoa.

In the first part of this volume the name Conchifera is used for the bivalve Mollusca. This is to be regretted, inasmuch as the term is incorrect, unless it can be used to include all the shell-bearing Mollusks. In the latter part of the appendix, Pelecypoda is used instead of Conchifera. It may be that the latter name has priority, but surely it is not so appropriate nor so correct as that of Lamellibranchiata, which has for so long been in general use; and it seems questionable whether this reversion to old names, for groups of animals, is justifiable.

Perhaps the most difficult part of the work which Mr. Etheridge has undertaken is the correction of specific synonymy, and specialists in certain groups may possibly be inclined to differ from him; but those who have done most in the endeavour to rectify the synonyms of fossil species will best appreciate the difficulty of the task, and be most ready to make allowance for any difference of opinion in these matters.

There are few palæontologists who have such a grasp of the entire range of British fossils as Mr. Etheridge, and probably none better qualified for the work, the first part of which is here so successfully accomplished. The author is to be congratulated on the completion of so much of his task, and on the admirable manner in which it has been printed and published.

That the book has been printed by the Clarendon Press is sufficient guarantee of its excellence. The careful typing and arrangement, as well as the clear printing and good paper, are all that could be wished for, and add greatly to its value.

It is much to be desired that the Secondary and Tertiary portions of this catalogue should speedily be in the hands of geologists; but one reads with regret, in a note at the end of the preface, that the pressure of official duties will prevent the author preparing these parts for publication, although the manuscript is practically complete. It is sincerely to be hoped that the delay may be of short duration, and that Mr. Etheridge will shortly see his way to the completion of this valuable work, which every geologist and palæontologist ought to possess, as it must of necessity be for many years the standard work of reference for British fossils.

E. T. N.

YORKSHIRE LEGENDS AND TRADITIONS.

Yorkshire Legends and Traditions, as told by her Ancient Chroniclers, her Poets, and Journalists. By the Rev. Thos. Parkinson, F.R.Hist.S., &c. (London: Elliot Stock, 1888.)

LEGENDARY LORE has its interest if not its value to the anthropologist as well as to the philologist. It sometimes happens that a word has given rise to a legend, and that the existence of a legend or tradition indicates identity of race, or a common origin of two widely separated peoples. The science of philology in particular welcomes the data in legends and traditions faithfully given in the vernacular, and undoubtedly affords in its turn a scientific explanation of the origin or meaning of some of them.

The limitation in the title of the present work does no small injustice to the subject, and to the qualifications of the author for treating it, as it excludes many of the most interesting, most local, and most characteristic of Yorkshire legends and traditions, which must be well known to so true a Yorkshireman as the Rev. Thos. Parkinson. This, however, is only a first instalment or "wainload of the marvellous from this county of broad acres," and as such is acceptable. It would be well if the author were to give references to all the principal sources of these legends, as some of them are found in more than one shape, and different versions of the same story sometimes present details which identify the legend with a third legend found perhaps in a distant locality, or with more than one such, thereby proving a still more remote common origin. We will presently exemplify this in the Handale legend. Our author has grouped his materials under nine heads: legends and traditions connected with the early history of Yorkshire; those of abbeys and monastic life; of Satanic agency; Barguest and ghost legends; Mother Shipton; dragons and serpents; battle-fields; legends of wells, lakes, &c.; and miscellaneous legends. The name of *Eboracum*, or York, has proved a fertile source of legends, several of which are collected in the opening chapter; its true origin, not being scientifically demonstrable, is, as our author justly observes, "buried in obscurity." Of the legends connected with the coming of the Danes, that of "Buern the Busecarle" (pp. 10-14), taken from the translation in the "Church Historians of England" (by the Rev. Jos. Stephenson) of the Anglo-Norman "Metrical Chronicle" of Geoffrey Gaimar, written in Stephen's reign, is of historic interest, and receives further elucidation and support from the self-evident fact that "Buern Busecarle" is a Scandinavian or Old Norse and not an English name and title. Björn Bús-Karl = "Björn the farmer of the King's estates," or the "Karl" (A.S. ceorl) of the Konungs-bú or royal demesnes, as he is, in fact, described in the legend. It was because he was a Northman that he called in the aid of the Danes, including Northmen, when his wife was dishonoured by King Osbert, as related by Gaimar. Passing by numerous legends, among which that of the death of King Ella, and the probable site of Ellsworth and Ellecroft or Ellecross is interesting, we note (p. 96) that the horn of Ulphus, or Ulfr, a Norseman who, about the time of King Canute, governed in the western part of Deira, is "a portion of the tusk of an elephant, about 3 feet long." The author excites the reader's curiosity by adding that "round the thick end are engraved a number of emblematic figures, in some respects not unlike those found on Egyptian and Assyrian monuments." Surely there must be extant some expert opinion as to the species of elephant, as to the date and nationality of the workmanship, and whether the tusk was brought from the East, already engraved, by the far-roving Northmen—but here our author fails us. The "Filey

Haddock Legend" (p. 121) appeared in Hone's "Table Book," 1838, ii. p. 638, signed "T. C."

We are compelled to pass over much interesting matter, such as the legend of the submerged town in Semerwater (for which see also Barker, "Wensleydale," 1856, p. 239), which is also told of Gormire Lake, of Lough Neagh, and is one of the legends of the Rhine. Gormire Lake is formed on the back of a landslip. The original version of the pretty ballad on the legend of the Troller's Gill (p. 127) and of Billy B.'s adventure (p. 130) is given in Hone's "Table Book," 1838. The article is signed "T. Q. M.," the *nom de plume* of the late Dr. J. H. Dixon in the "Table Book." Also our author's "Wise Woman of Littondale" (p. 134), from the "Table Book," ii. p. 775, signed "T. Q. M.," is one of Dr. Dixon's productions. With regard to the probable site of "Stoknmore" (p. 162) a local tradition identifies the erect column of stone, 11 feet high, known as the "Long Stoop," on Yeadon Moor, as the subject of Mother Shipton's prophecy "then will ravens sit on the cross, and drink as much blood of the nobles as of the commons."

With reference to the Handale legend (p. 168), the version there given is taken from the *Leisure Hour* for May 1878, but that, in its turn, is almost word for word taken from Ord's "History of Cleveland," 1846, p. 283, as related by Mr. Marr, then tenant of Handale farm. Another original version obtained by the present writer from Mr. Robert D. Watson, of Loftus-in-Cleveland, describes the hero as "a *shoemaker* of the name of Scaw" [O.N. Skór, a *shoe*] who "had a suit of clothes made into which he had by some means stitched all over it razor-blades edge outwards," an item which is identical with that given by our author (p. 171) of the slayer of "the dragon of Loschy Wood." Though our author does not include Beowulf among his legends of Yorkshire, we are of opinion that the able demonstration of Mr. Haigh, in his "Anglo-Saxon Sagas," 1861 (which can now be supported by additional facts, overlooked by him, such as the two coins of "Hæreth," found in Northumbria and engraved in Hickes's "Thesaurus," 1705, iii. p. 168), that Cleveland was the scene of the principal event recorded in Beowulf, remains unshaken by the numerous foreign and English commentaries that have since appeared on the subject. We cannot doubt that the "Grendel" destroyed by Beowulf was a religious house at "Grendale, afterwards Handale," as appears by a charter, anno 1133 (Charlton, p. 90), but space forbids doing more than noting the omission. There are some grievous misprints, *e.g.* "bretwalder" for "Bretwalda" (p. 11), "Worsaal" for "Worsaae" (p. 221), "Upsalier" for "Upsalir" (p. 222).

Among the miscellaneous legends is one called "Swine Harrie" (p. 219) apparently belonging to deer-stealing days, and found in various forms. It appeared in Hone's "Table Book," 1838, ii. p. 722. The thief in carrying his burden home slips in crossing a rock or wall, and is hung by the cord. It should, however, be observed that "Hanging" is a common epithet in the Pennine Hills, *e.g.* "Hanging Stones." *Apropos* of this legend "there is a crag on Embsay Moor called 'Deer Gallows,' and it is said a deer was once hanged there. There is a deep crevasse in the rock becoming narrower toward the bottom, and—the story goes—a deer once fell down and caught with its horns across, and so was hanged" (J. J. Wilkinson). "Gallas" or "gallowses" in the dialect means "braces." The name "hanging" as applied to rocks alludes to some physical feature, but may have localized the legend.

Among the excluded legends we may mention the pretty legend of the "Walling in the Cuckoo," by "t' Hoastik Carles" (the Austwick Carles) and "the funny one of Wengby," "Meal Ark Spring," "Hobthrush Hall," "Simon Amangus," and many others. The legend of "Wallin' in t' Cuckoo" has a wide range, and is told of various villages in the Northern counties. We have heard it in Borrowdale. It is told of Austwick, near Settle, that, seeing a cuckoo in a tree, the carles began to build a wall round it to keep it, and its consequent, the summer, always there, and they were very much astonished when it flew away.

There is another legend of Austwick:—

> "T' Austwick Carles cried 'Whittle ta t' tree,'
> Lifted t' bull ower t' yet, an t' pig inta t' stee."

"There was only one 'whittle' or butcher's knife in Austwick, and when anyone wanted it he went to the tree in the middle of the village in which it was kept stuck. If it were not there, he cried 'Whittle ta t' tree!' three times, and whoever had it was bound to bring it." In the glossary to "Studies in Nidderdale," 1882, p. 291, occurs the article "Wittaled. 'Ah've gittan fas'en'd ta t'sod, if ah aint gittan *wittaled* ta t'tree.'—*Nid. Al.*, 1880. *Wel.* Gwydaw, *to grow woody;* gwyden, a *tree;* gwyddawl, rudimental; so 'wittaled' means rooted to the tree so as to form part of its wood, grafted." Here, the legend evidently takes its origin in the similarity of sound of the two words "whittle," a *butcher's knife*, a familiar word, and "wittaled," *grafted*, a forgotten word preserved in the phrase "wittaled ta t'tree." As to lifting the bull over the gate, they were too simple to know that the gate was meant to open (J. J. Wilkinson). These are samples of the excluded but most interesting legends, which will, we hope, find a place in our author's promised second series.

JOSEPH LUCAS.

FOREIGN BIOLOGICAL MEMOIRS.

Translations of Foreign Biological Memoirs. Vol. I. *Memoirs on the Physiology of Nerve, of Muscle, and of the Electrical Organ.* Edited by J. Burdon-Sanderson, M.D., F.R.SS.L. and E. (Oxford: Clarendon Press, 1887.)

THIS volume is the firstfruits of a scheme which was started some years ago. The original intention was to translate and put before English readers as soon as possible after their publication the most important foreign papers on various biological subjects. Owing to various difficulties, the original scheme had to be altered, with the result that the first volume of the proposed series has taken the form of the present book.

Prof. Burdon-Sanderson has in this volume confined himself to that subject in which he himself is especially interested—viz. the physiology of nerve, muscle, and the electrical organ—and has collected and edited the translations of the most important papers which have appeared in the German language during the last five years on this

very difficult and abstruse subject. The book is divided into three parts : viz. Part I. Researches relating to the Law of Contraction, which contains the well-known papers of Tigerstedt, Grützner, and Hering on this subject; Part II. Researches relating to Secondary Electromotive Phenomena, containing papers by Du Bois-Reymond, Hering, Hermann, and Biedermann; Part III. Researches on the Electrical Phenomena of Certain Electrical Fishes, by Du Bois-Reymond. In addition, a short summary of two or three of the most recently published papers on the subjects treated of in this volume is given in the form of an appendix.

The memoirs selected for translation in Parts I. and II. form a group of papers which are most interesting to all those who are anxious to obtain more than a text-book acquaintance with the difficult questions treated of in them. The translations have been made with great care and accuracy, and with a careful attention to style, though naturally the original German construction is more palpable in some than in others; it would, however, be invidious to select any individual memoirs in this respect. The editor is also to be congratulated upon the manner in which the especially difficult German phraseology inherent to the nature of the subject has been rendered into English. Uniformity of translation among the different translators has been fairly well attained. Perhaps the most noticeable discrepancies are in the translation of the words "*Lücke*" and "*Schwelle*." The phenomenon of the "*Lücke*" is called by one translator the phenomenon of the "gap," and by another the phenomenon of the "hiatus"; of the two, "gap" is perhaps preferable. The almost untranslatable word "*Schwelle*" is sometimes rendered as "limen," at other times as "threshold"; of these two, "limen" sounds best. It is also a pity that the editor has not settled whether the opposite pole to the anode ought to be spelt with a "c" or a "k": cathode and kathode are pretty equally distributed throughout this volume. As to the arrangement of the different memoirs, they all follow each other in natural sequence, with the exception of the second and third papers of Tigerstedt, which ought to have been transposed, as the author assumes in No. 2 that No. 3 has already been read.

Not the least prominent part of the work is the preface, in which the editor briefly links together the various memoirs. As it will seem to many that this should be the most important part of the whole book, it is to be regretted that it is not fuller and also more critical; for undoubtedly any collection of translations of foreign memoirs upon a special biological subject would be very much more valued by English readers if the recognized English authority on that subject prefaced the translations with a critical commentary embodying his own views.

Also, as the object of the book is presumably to enable students and physiologists who do not read German easily to understand the present position of the physiology of nerve and muscle without having to refer to the original papers in the German journals, it would have been better to afford more indication as to the nature of the contents of previous papers which are referred to by the authors but have not been included among the translations. This might have been done by the more copious use of footnotes, or by the translation of one or two more papers. Thus in Biedermann's paper the whole argument is so bound up with his previous papers on the heart of the snail and the muscle of the anodon, that it would have been better to translate the three papers rather than only one. If this had been done, the book would have been large enough without Part III. and there would have been no harm in that, for in the first place the physiology and histology of the electrical organ might well be treated of in a separate volume of memoirs, and in the second place, the papers included in Part III. are all by the same author, and can hardly be considered so important as those in Parts I. and II. Thus Du Bois-Reymond's statement as to "irreciprocal conduction" is based upon an error of observation, according to the recent paper of Gotch in the Proceedings of the Royal Society, and is therefore hardly worthy of a place in these memoirs.

The book is essentially of the nature of an experiment, and it is to be hoped that the demand for it will be sufficient to repay the editor for the time and trouble which he has spent in carrying out his task, and to encourage him to bring out a series of similar volumes dealing with a number of the most important of those biological questions which are the subject of controversy at the present time.

OUR BOOK SHELF.

Examples in the Use of Logarithms. By Joseph Wolstenholme, Sc.D. (London: Macmillan and Co., 1888.)

Practical Logarithms and Trigonometry. By J. H. Palmer. (London: Macmillan and Co., 1888.)

THESE are two books for the use of mathematical students from the hands of practical teachers, who may be assumed to be well acquainted with the difficulties met with by students.

The first one consists entirely of rules and their application to examples. The earliest examples are on the extraction of logarithms from tables. Then follow the reductions of $\sqrt{a^2+b^2}$ and $\sqrt{b^2+c^2-2bc\cos A}$, after which come the various solutions of triangles, finally concluding with a lengthy series of examples on the calculation of the parts of tetrahedra when the sides are given. The author's experience has indicated that examples of the latter class are especially useful in teaching the habitual accuracy which it is desirable to attain, and another reason for their introduction is that they afford preliminary practice in the solution of spherical triangles. Such, then, is the first book, and no one will contradict us when we remark that a student who carefully works out the examples given cannot fail to become perfectly familiar with logarithms and their applications.

The range of the other book is a little wider. Beginning with involution and evolution treated arithmetically, and following with geometrical and trigonometrical definitions, the author leads to logarithms and their simpler applications. The aim of the book is to give a thorough practical knowledge of the use of logarithms and the solution of plane triangles and problems in trigonometry. The general plan adopted is first to state a rule, give worked examples, and then to give a number of exercises on each rule, which may be tested by the answers given.

The omission of the proofs of the rules and formulæ renders the book liable to be looked upon as a cram book, but it is apparently intended mainly for the use of those who simply wish to know how to use logarithms

and trigonometry, and to whom proof is of secondary importance.

The exercises given will make both books specially valuable to teachers, for the working of numerous examples is indispensable in the teaching of mathematical subjects.

Elementary Statics. By the Rev. J. B. Lock, M.A. (London: Macmillan and Co., 1888.)

WE gladly welcome another addition to Mr. Lock's excellent series of text-books. To the new terms already introduced by him, another is now added. This is the term "resolute" as a substitution for "resolved part," the argument for the change being that "the idea is so important in the subject that a definite *name* will be found useful." Those who have already become familiar with the older expressions may not be willing to accept the changes, but there can be no doubt that the new expressions are appropriate, and will be of great service to beginners.

The treatment adopted is based upon Newton's laws of motion, the author's opinion being—and we quite agree with him—that this greatly simplifies the subject. The parallelogram of forces is assumed, the student being recommended to postpone the proof until he commences his study of dynamics. The working of examples, as every teacher knows, is the only way to obtain a thorough knowledge of any subject which requires mathematical treatment, and Mr. Lock has fully recognized the importance of this. Typical examples, excellently selected, are worked out at full length, and numerous others are given as exercises. There is also a selection of papers from some of the Oxford and Cambridge examinations. A new departure is the introduction of a short chapter on graphic statics, which we highly approve of. The teaching of this subject has made rapid strides during the last few years, and the methods are so simple, and applicable in cases which would involve laborious calculations, that the introduction of the subject into text-books is very desirable.

The whole subject is made interesting from beginning to end, and the proofs of the various propositions are very simple and clear. We have no doubt that the book will be appreciated by all who have an opportunity of judging of its merits.

Catalogue of the Fossil Reptilia and Amphibia in the British Museum (*Natural History*). Part I., containing the Orders Ornithosauria, Crocodilia, Dinosauria, Squamata, Rhynchocephalia, and Proterosauria. By Richard Lydekker, B.A., F.G.S., &c. (London: Printed by order of the Trustees, 1888.)

THIS work forms a very valuable addition to the series of British Museum Catalogues, and will be welcomed by all palæontologists as giving a full and complete account of the specimens of fossil reptiles in the National Collection, many of which have an especial interest as being the "type-specimens" on which so many classical monographs have been based.

Mr. Lydekker adopts, with some alterations, the classification proposed in 1885 by Cope, with the modifications recently suggested by Baur. The reasons for the changes he has introduced are fully discussed in the introduction.

Full descriptions of the orders, families, genera, and species, are given in most cases, and the book is illustrated by sixty-nine woodcuts, many of which are taken from the works of Marsh, Dollo, and others. The introduction of the names of many of the larger groups which are not represented in the British Museum collection renders the work more complete, and the addition of so much descriptive matter, and of copious references to the bibliography of the subject, also increases its value far beyond that of an ordinary Catalogue.

The History of Australian Exploration. By Ernest Favenc. (Sydney: Turner and Henderson, 1888.)

THE author of this volume does not profess to give a complete history of the exploration of Australia. Much of the work of exploration has been done by private travellers and adventurers; and it is of course impossible that their labours can ever be adequately recorded. For the fulfilment of such a task the co-operation of hundreds of old colonists would be necessary; and the work, when completed, would not only fill many volumes, but, as Mr. Favenc says, would prove most monotonous reading. He has therefore confined his attention to public expeditions, dividing his subject into two distinct parts—land exploration and maritime exploration. His narrative covers a period of one hundred years—from 1788 to 1888. The book is issued under the auspices of the Governments of the Australian Colonies, and it is in every way worthy of this distinction. Mr. Favenc has invariably gone to the best sources for information, and has produced a record which is not only trustworthy, but full of interest. The value of the book is considerably increased by several maps and facsimiles.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Protest in *The Nineteenth Century*.

THE present age is eminently a sensational one. Everybody deals in superlatives and universals. Morning and evening the newspaper bills vie with each other in appealing to that particular form of curiosity which feeds upon alarms. Our civilization is declared to be altogether wrong. Dr. Pangloss's doctrine is reversed—nothing that is is right. We are incessantly invited to take stock of our arrangements political and social, and treated to denouncements of almost every detail of them. We are too serious, too frivolous, a prey to panics, stolidly blind to dangers, distrustful, credulous. To crown all, what was fondly supposed to be one of the greatest of modern improvements is roundly declared to be a sham; to be worse—a lure to destruction, mental and physical. Loud were the pæans sung some forty years ago over the then new system of competitive examinations which so vexed the soul of the author of "Gryll Grange." Now we are assured that the whole examinational system is utterly stupid, and, in effect, that it were better at once ended than in any way mended.

But literary rhetoric, however brilliant, in these days produces but a momentary impression. We have so much of it that we have come to regard it with the contempt bred of over-familiarity. After the first shock of delight or astonishment has passed off, we begin to look for the facts and criticize the logic. Sweeping phrases, sounding invective, the vigorous style in general, cease to convince. There is too much of the scientific spirit abroad for the roar of the old lions or of the young lions to cause more than a passing alarm. Denunciation is always easy, though not, of course, of the forcible and brilliant kind with which Prof. Freeman and Prof. Harrison have made us familiar, perhaps a thought too familiar. I shall look forward with interest, and with the certainty of some instruction, to the statement which will, no doubt, be forthcoming of the facts the *Nineteenth Century's* protest is based upon, but as a competitive examinationist I look forward to it without anxiety.

Meanwhile, I venture to offer one or two remarks upon a single sentence in the protest. "Again and again," it is said (p. 620), "brilliant young men once full of early promise go down from the Universities as the great prize-winners, and do little or nothing in the after years." The reason, it is added, is that "they have lost their mental life before they are five-and-twenty"; in other words, that the examinational system, *quâ* examinational, has killed in them the love of knowledge by that age—a sad fact enough, if true.

But is it true? Brilliance of parts is not always, I am almost inclined to assert not commonly, accompanied by a disinterested love of knowledge, though often enough by ambition, which is a very different thing; nor, unfortunately, is a love of knowledge always associated with the capacity to gratify it. To many men, again, opportunities fail, or health, or energy of character, or perseverance, or the means enabling them to wait for success in the career chosen, or, lastly, circumstances may have compelled them to adopt an unsuitable career, and so their intellectual lives are wrecked. It is only in respect of the residue remaining after elimination of these cases that the reproaches addressed to the examinational system are capable of being justified. What proportion that residue may bear to the totality of brilliant failures it may be difficult to determine. My impression is that it is a very small one. At any rate, it is so in the University to which I belong—the University of London. So large a proportion of the men who have taken high degrees at that University have in after life fully maintained, to say the least, the distinction of their University record, that the failure of the residue—if such failure there be—may be justly ascribed to causes of the nature above indicated rather than to any ill effect of the examinational system. The assertion may easily be verified by reference to the Honours Lists, more especially in the Faculties of Science and Medicine. I mention these Faculties because it is much easier to trace the after life of graduates in them than in the other Faculties. But on looking over the list of M.A. medallists, I do not find a single name which suggests any lack of after-life response to earlier promise.

Finally, on turning to the University record of many, probably of most, of the eminent men of the day, the very reverse of the alleged disparity between promise and performance upon which the protest is based will be found to exist.

I am, for my part, fully convinced after several years' daily familiarity with the working of a purely examinational system, that in examinations we have the best means yet devised of testing the general ability and attainments of young men and women. And I have shown above that competition does not produce any of the evil results complained of in the protest. On the contrary, I believe it to be in most cases—but certainly not in all—a most useful discipline.

But I have no faith whatever either in piecemeal examinations, or in examinations in technical or special subjects, or in mere manipulative matters. I admit, too, that nothing like sufficient attention is paid to the progressive improvement of examinations in accordance with the advance and increasing volume of knowledge. In particular, the range of optional subjects at higher examinations should be greatly extended, that the test applied to each candidate may better correspond with his opportunities and with his mental structure. Above all, the tendency which unfortunately exists to increase beyond measure the difficulty of examinations requires to be carefully guarded against. Too highly pitched syllabuses necessarily involve a low standard of performance, with the result that the successful candidate and the public are equally deceived.

F. VICTOR DICKINS.

Burlington Gardens, November 6.

Gresham College.

THE communication of Mr. R. D. Roberts states that the article of Prof. Ray Lankester "is based entirely upon a misapprehension as to the purpose and function of the London Society for the Extension of University Teaching and its position with regard to Gresham College."

I beg to be allowed to state that I can indorse Prof. Ray Lankester's statements with regard to the London Colleges. It is nine years since my connection with King's College ceased, but for nine years I was a lecturer in the Evening Class Department of the College. I know that the College staff, often at great personal sacrifice on the part of some of its members, threw such energy into the teaching of evening classes that their efforts can best be described as thoroughly educational in the highest sense. The number of lectures in the winter courses were twenty-five to thirty, in the summer ten. They were, as a rule, as complete and advanced as similar courses in the Universities, some of them more complete than such courses elsewhere.

At the time when the Society for the Extension of University Teaching was first established, it appeared to me to be a superfluous and mischievous institution. The leading idea which it communicated to the public by very extensive advertisements and reports of meetings was, that there was no such form of education in London, and that the teachers subsidized by the Society were bringing enlightenment from the Universities of Oxford and Cambridge. For several years there were courses consisting of only three or four lectures, delivered in districts widely separated, as, for instance, in Mile End, Kensington, and Dulwich, while a long course consisted of only ten lectures. There was no curriculum in any one centre in either arts or science. The courses of lectures were not even grouped into Departments or Faculties, such as modern languages and literature, Latin and Greek, ancient history and archæology, pure and applied mathematics, experimental science, or biological science. Desultory instruction, not education, appeared to be the object of the Society. The lecturers were, as a rule, qualified for the duties they undertook; some were eminent men, even of the highest eminence; but I do know that others from the Universities should certainly not have been intrusted with the duty of public lecturers until they had undergone an additional term of instruction and training of at least three to five years as assistants to Professors. The Society provided employment for a number of unemployed graduates from Oxford and Cambridge; and at the time, no doubt, it was considered politic and conciliatory to make an assumption of carrying culture to the masses. The young men were willing to take up such duties, for they gained opportunities for practice in the art of teaching which led to possibilities in the way of promotion. There is little doubt that the Society has improved latterly, and it may or may not deserve to be supported by public subscriptions; but it would be a monstrous injustice to King's and University Colleges to place the funds of Gresham College in the hands of this Society. The injustice would be the greater in the case of King's College, because, as I understand, University College has discontinued its Evening Class Department; but for at least twenty years before this London Society for the Extension of University Teaching was in existence or thought of, the staff of King's College, without endowment, were teaching by night as well as by day, and with inadequate remuneration doing more than fulfilling the intentions of Sir Thomas Gresham. The City clerks, engineers, and manufacturers left their places of business to attend these lectures, and obtained sound theoretical and practical instruction in art and science, ancient and modern literature and languages. I have in mind many who have risen to distinction; and there are, no doubt, thousands who are ready to acknowledge the benefit they derived from the evening classes of King's College.

I doubt whether these facts were placed before Mr. Goschen on the occasion when he made his speech on the subject of this Society and Gresham College. It seems to me that the matter should be put before him and all others concerned in a true and proper light, and without partiality.

In conclusion, I will state it as my carefully-considered and deliberate opinion that the Lecture Society called the London Society for the Extension of University Teaching has done no educational work which for extent or solidity is worth consideration in comparison with that of King's and University Colleges.

W. N. HARTLEY.

Royal College of Science, Dublin, November 9.

Divergent Evolution.

SOME of your readers may possibly remember a paper of mine on "The Variation of Species as related to their Geographical Distribution," which appeared in NATURE, vol. vi. p. 222. About the same time I prepared a paper on "Diversity of Evolution under One Set of External Conditions," which was published in the Linnean Society's Journal—Zoology, vol. xi. pp. 496–505. I refer to these papers simply to say that the problems there discussed have occupied my attention more or less ever since.

Part of my paper relates to the subject discussed by Mr. Romanes in his paper on "Physiological Selection"; but as it has been independently worked out, I believe it will be of interest to all who have followed the discussion on the "Origin of Species." The abstract of Mr. Romanes's paper given in NATURE, vol. xxxiv. pp. 314, 336, 362, did not come into my hands till the following January, when my theory of "Divergent Evolution through Cumulative Segregation" was, for the most part, written out in its present form. Since then, and with reference to the discussion on "Physiological Selection," I have worked out the algebraic formulæ given in the fifth chapter, and have introduced explanations of the same.

My "segregate fecundity" and Mr. Romanes's "physiological selection" are the same principle; and our theories still further correspond in that we both insist on the prevention of intercrossing as a necessary condition for divergent evolution. This conclusion was reached by me through investigations made many years ago, and was maintained in my paper on "Diversity of Evolution under One Set of External Conditions," and in still stronger language in articles in the *Chrysanthemum* (Yokohama), January 1883, and in the *Chinese Recorder* (Shanghai), July 1885. In the first of these papers I used the word "separation" to indicate the phase of the principle that results from migration; but for a fuller discussion of the subject I found it necessary to introduce "segregation" as the more significant term; and in the second paper I maintain that "While external conditions have power to winnow out whatever forms are least fitted to survive, there will usually remain a number of varieties equally fitted to survive; and that, through the law of segregation constantly operating, ... these varieties continue to diverge till separate species are fully established, though the conditions are the same throughout the whole area occupied by the diverging forms;" and in the third paper I said, "I am prepared to show that there is a law of segregation rising out of the very nature of organic activities, bringing together those similarly endowed," and causing "the division of the survivors of one stock, occupying one country, into forms differing more and more widely from each other." Since then, my nomenclature of the subject has been worked out with that word as the central symbol of my theory. It is therefore a pleasure to find that Mr. Romanes uses the same word to express the same general idea, giving to his theory the alternate name of "segregation of the fit" (Linnean Society's Journ.—Zool., vol. xix. pp. 354, 395), and in one place at least describing it as "physiological segregation" (see letter on "Physiological Selection," NATURE, vol. xxxiv. p. 408).

As I have explained in chapter iv., I at first thought of using "physiological segregation" in place of "industrial segregation," but finally concluded that it was a term of such wide significance that it could not be well used as the name of any one kind of segregation, while at the same time it was not broad enough to serve as a general term for all kinds. I therefore greatly prefer the term "segregation of the fit." I would, however, so define it as to cover all forms of segregation.

Though our use of this fundamental word is undoubtedly due to our having the same general truth to express, several divergences appear in the development of our respective theories, tending, we may hope, to a fuller elucidation of the subject.

26 Concession, Osaka, Japan. JOHN T. GULICK.

Alpine Haze.

THE peculiar haze mentioned by Prof. Tyndall is no doubt identical with what is commonly met with in some parts of the Mediterranean. During the hottest and driest weather of the summer, and when no wind is blowing, perfectly horizontal strata of haze can be seen occupying the Gulf of Naples. The peaks of the Sorrentine Mountains, with Solara of Capri, Ischia, Vesuvius, Camaldoli, &c., stand out above this haze. The height of the strata rarely reaches 2000 feet, and is more often about 1500 feet. The same facts that led Prof. Tyndall to consider it other than water vapour, and of micro-organic nature, had produced in my mind similar conclusions. This haze, when looked at near the sea, has often a beautiful pink tint, due, no doubt, to a complementary effect from the sea-water colour, as the colour is more marked on the limestone rocks, where the white sea-bottom makes the water look much greener When, however, the observer is cut off from a view of the green sea for some time, the haze has then a light buff colour. The opacity of this haze is so great as sometimes to resemble a slight London fog.

Anyone who would count the number and study the characters of the organisms and other solid contents of the air here at different times would soon settle the question what this phenomenon is due to, and whether there is any truth in the old *blight*. H. J. JOHNSTON-LAVIS.

Naples, November 4.

The Astronomical Observatory of Pekin.

IN your number of November 8 (p. 46), you gave an account of a lecture by Mr. S. M. Russell, of Pekin, on the instruments in the old Observatory there. May I mention that the late Alex. Wylie, about nine or ten years ago, published a full account of them (with illustrations) in the "Travaux de la 3me Session du Congrès International des Orientalistes," vol. ii. Having had my attention drawn to them by some photographs kindly sent me by Mr. Russell, I pointed out the scientific interest of Ko Show-King's instruments (which anticipated the ideas of Tycho Brahe by three hundred years), in a paper published in the Proceedings of the Royal Irish Academy, vol. iii., 1881, and in *Copernicus*, vol. i. J. L. E. DREYER.

Armagh Observatory, November 12.

AN HISTORICAL AND DESCRIPTIVE LIST OF SOME DOUBLE STARS SUSPECTED TO VARY IN LIGHT.

THE light-changes of double stars are, for the most part, of an intermittent character. Unmistakable at one epoch, they may completely evade detection at another. Hence observations of them which, by the nature of the case, cannot be repeated are apt to incur discredit for lack of confirmation. They should, on the contrary, if properly authenticated, be carefully borne in mind, as testifying to an incident in the history of the stars they refer to which, however apparently isolated, must be extremely liable to recur. We have therefore thought that it would be useful to put together, as concisely as possible, a few facts bearing on the supposed variability of some stars which we may reasonably consider to be physically double, referring those of our readers who desire fuller information on the subject to the original authorities we shall cite for their convenience.

γ Virginis = Σ 1670.—The first observation is by Bradley in 1718. The components, normally of the third magnitude, were regarded as equal by all observers until W. Struve, May 3, 1818, noticed the preceding star as slightly the fainter. It continued so for several years; the difference was obliterated from 1825–31, and reversed, doubtfully 1832–33, certainly in 1834 ("Mensuræ Micrometricæ," pp. lxxii. 4). O. Struve's observations, 1840–74, showed decided variability in a double period, oscillations of half a magnitude in a few days being superposed upon a fluctuation extending over many years. An investigation of the law of change, begun in 1851, led to no result, owing to the low altitude of these stars at Pulkowa ("Obs. de Poulkova," ix. 122). Dawes found them equal, 1840–47; but each alternately about a quarter of a magnitude brighter than the other, 1847–54 (Memoirs R. Astr. Soc., xxxv. 217–19). Similar swayings of lustre were constantly apparent to Dembowski (*Astr. Nach.*, Nos. 1111, 1185, 1979). Each star is given as of 3·5 magnitude (combined 2·8) in the "Harvard Photometry" (see also "Harvard Annals," xiv. 454). Gould assigns to them the combined magnitude of 3·1, Pritchard of 2·67; Gore thought them nearer to the second than to the third magnitude, April 5, 1883 ("Cat. of Suspected Variables," p. 362). (The combined magnitude of two third magnitude stars is 2·25.) Owing to their uncertainty of shining, the angle has often been reversed in measuring these stars. They are of a pale yellow colour, and show a spectrum of the Sirian type. They revolve in a highly eccentric orbit in a period of 180 years, and emit fully sixteen times as much light proportionately to their mass, as the sun.

44 (*i*) Boötis = Σ 1909.—On June 16, 1819, Struve noted a difference of two magnitudes between the components; of one invariably 1822–33, but of only half a magnitude 1833–38. Argelander found them exactly equal, June 6, 1830 ("Mens. Microm.," p. lxxii.). To Dawes, in April 1841, the attendant star seemed a shade brighter than its primary, which was rated as of fifth magnitude (Mems. R. A. Soc., xxxv. 232). Dunér's observations at Lund, 1868–75, confirm their relative variability, causing the disparity between them to range from 0·4 to 1·3 magnitude; and he points out that they appeared to Herschel consider-

ably unequal in 1781, but perfectly matched in 1787. Both stars were yellow in 1875, but the tint of the smaller was at times less deep than at others (" Mésures Microm.," 1876, p. 74). Admiral Smyth marked it as "lucid gray" in 1842; Webb and Secchi respectively found it blue in 1850 and 1859; Webb and Engelmann reddish in 1856 and 1865. The principal star has often been considered as pure white. The spectrum belongs to Class I. The photographic magnitudes of the pair, as determined at Paris in 1886, are 5·3 and 6. Engelmann concluded the smaller component to vary from magnitude 5 to 7, the larger from 5 to 6 (*Astr. Nach.*, No. 1676). They revolve in a period of 261 years, the plane of their orbit passing nearly through the sun. The periastron passage was in 1783. They possess at least four times the solar luminous intensity.

δ Cygni = Σ 2579.—The chief star remains steadily of the third magnitude; its companion varies probably from the seventh to about the ninth. Discovered by Herschel in 1783, it was invisible to him in 1802 and 1804, as well as to his son in 1823, and to South and Gambart, under exceptionally favourable conditions, in 1825 (Phil. Trans., cxiv. 339, cxvi. 376). Struve re-detected it in 1826, since when it has been continuously observed. The fact of its variability has even been doubted (Dunér, " Mésures Microm.," 1876, p. 118; Sadler, *Observatory*, ix. 307). Its changes of colour are, however, unquestionable. Struve marked it as ashen gray, 1826–33, but as remarkably red in 1836 (" Mens. Microm.," p. 297). Dawes found it blue, 1839–41; Secchi, red, 1856·62, blue, 1856·98, violet, 1857·53 (Engelmann, *Astr. Nach.*, No. 1676). Dunér saw it always red, except on one occasion, when it seemed olive. The primary is of a greenish white, and exhibits a Sirian spectrum. The period of 415 years attributed to the pair by Behrmann is probably too long; Hind's, of 179 years, is certainly too short. With Behrmann's elements, the light-power relative to mass comes out *one hundred times* that of the sun.

The three couples just described are the only variable double stars of which the orbits have been computed. We shall now mention a few which have so far described arcs too small to serve as the bases for investigations of the complete ellipses.

ζ Boötis = Σ 1865.—The following component was found the brighter by Herschel in 1796, and by Struve, in general, until 1833, when the order was reversed. They were pretty equal 1821–24 (Pickering, "Harvard Annals," xiv. 458). Their alternating fluctuations were confirmed by O. Struve's observations, 1840–63, since when, until 1878, the preceding star had always the advantage (" Obs. de Poulkova," ix. 143). F. Struve estimated their magnitudes at 3·5, 3·9, adding the remark, "Splendor in altera stella est variabilis" (" Mens. Microm.," p. 21). Their photometric magnitudes were determined at Harvard as 4·4, 4·8, the following star being the brighter. Dawes considered them as equal at 4 or 4·5 magnitude in 1847–48, but each star in turn took a slight lead (Mems. R. A. Soc., xxxv. 229). Dunér noted them sometimes as both of fourth, sometimes as of third magnitude, the changes occurring, as a rule, simultaneously (" Mésures Microm.," p. 68). The colour of these stars is white, or yellowish, and their spectrum well marked of the first type. The period of their revolution must be enormously long, and their mass proportionately small.

π Boötis = Σ 1864.—Gilliss's estimates varied from 4 to 5·6 magnitude for one component, from 5 to 6·7 for the other. Schmidt independently suspected fluctuations (Pickering, " Harvard Annals," xiv. 458). As one object, they were ranked by Abdurrahman Sûfi of fifth, by Lalande of sixth, by Harding, Argelander, and Heis of fourth magnitude. Their combined photometric magnitude was determined at Oxford as 4·1, at Harvard as 4·59. Herschel and South marked them in 1822 "nearly equal" (Phil. Trans., cxiv. 199); Admiral Smyth noted a disparity of 2½ magnitudes (" Cycle," p. 411, Chambers's ed.); and Struve found them of 4·9 and 6 (" Mens. Microm.," p. 97), and they were photographed at Paris as of 5 and 6 magnitudes in 1886. They emit white light of the quality of that of Sirius. An arc of about 4° has been described by the companion since 1781.

ε Arietis = Σ 333.—F. Struve had no doubt of the variability of these stars. His estimates of magnitude ranged from 4·5 to 6·5 for one, from 5 to 6·5 for the other component (" Mens. Microm.," pp. lxxii. 1; Pickering, " Harvard Annals," xiv. 434). Struve in 1827, and Dawes in 1845, found them equal; Secchi recorded a difference of one magnitude in 1855 (Engelmann, *Astr. Nach.*, No. 1676). Measured at Harvard as of 5·2 and 5·5 magnitudes, giving combined magnitude 4·6, they together showed to Piazzi and Bode as a fifth, to Harding as a fourth magnitude star. An arc of 10° has been traversed since 1827 (Crossley, "Hand-book of Double Stars," p. 204). The colour of these stars is white.

S (15) Monocerotis = Σ 950 was discovered by Winnecke in 1867 to vary from 4·9 to 5·4 magnitude in a period of 3d. 10h. 38m. (Gore's "Catalogue," No. 41). A ninth magnitude companion at 2″·8 seems to be in very slow orbital revolution. Struve called their colours green and blue (" Mens. Microm.," p. 65). The spectrum is of the first type.

α Piscium = Σ 202.—The magnitudes of these stars were estimated by F. Struve at 2·8 and 3·9 (" Mens. Microm.," p. 43), by O. Struve at 4 and 5 (" Obs. de Poulkova," ix. 17). Harvard determinations brought them out 4·4 and 5·3, but showed relative variability to the extent of half a magnitude. The larger star has been rated from 2·5 to 5·5 magnitude (" Harvard Annals," xiv. 433), and there is scarcely any doubt that the light of both (which is of the Sirian quality) fluctuates to some extent. The colour of the attendant star changes from blue to ashen olive and tawny (Webb, " Celestial Objects," p. 378, 5th ed.; Flammarion, " Cat. des Étoiles Doubles," p. 12). Slow revolution in a plane nearly coincident with the visual line is probable.

OΣ 256 = Lalande 24098, catalogued at Pulkowa in 1853 as 7 and 7·8 magnitudes at 0″·6, but subsequently found to vary respectively from 7 to 7·8, and from 7 to 8 magnitude. Dembowski thrice noted the preceding star as half a magnitude fainter, while four Pulkowa observations, 1842–61, showed it as much brighter than its companion, equality being twice recorded (" Obs. de Poulkova," ix. 327). Their variability was still more plainly evident by the manner of their occultation, as observed by Mr. Tebbutt, August 22, 1887. Three-fourths of their combined light disappeared instantaneously, leaving the semblance of a "blurred ninth magnitude star," representing, nevertheless, the chief component of recent measures, to be extinguished a little later (*Observatory*, x. 391). The stars have described an arc of 17° since 1842 (Crossley, "Hand-book," p. 287). Their spectrum is given by Von Konkoly as doubtfully of the solar type.

38 Geminorum = Σ 982.—Struve observed differences of lustre ranging from 1·5 to 4 magnitudes (" Mens. Microm.," p. lxxiii.). The inference of variability was ratified by Engelmann (*Astr. Nach.*, No. 1676; "Harvard Annals," xiv. 443). The combined magnitude in the "Harvard Photometry" is 4·8. A spectrum of the Sirian type was registered by Vogel in 1883. Colour-fluctuations seem pretty certain in the small star, which has retrograded 18° since 1782. The system has a common proper motion (Crossley, " Hand-book," p. 233).

Σ 1517.—Struve ranked each member of this close pair as of 7·3 magnitude, with slight alternate superiority (" Mens. Microm.," p. 286). Their variability was confirmed by O. Struve, who estimated their magnitudes at 7 and 7·8. A slow retrograde, and a rapid common proper

motion, prove their systemic connection ("Obs. de Poulkova," ix. 106).

The remaining stars on our list are relatively fixed.

Σ 2344.—Magnitudes 8·5 and 10 when first measured by Struve; 8·5 and 12 in 1829, distance = 1″·38. The companion was not again seen until 1835 ("Mens. Microm.," pp. 37, 296). The instability of its light was further attested by its invisibility to Secchi in 1859, to Engelmann in 1865 (Engelmann, *Astr. Nach.*, No. 1674).

Σ 2718.—Components intrinsically equal, but by turns slightly superior. Period of change probably short (Struve, "Mens. Microm.," p. 142).

61 Geminorum.—The brighter component varies from 6 to 7·5 magnitude, the fainter from 9 to seeming extinction (Flammarion, "Les Étoiles," p. 320). The larger star, which is of a deep yellow colour, was recorded by Piazzi as of 7·8, by Heis and Argelander as of sixth magnitude (NATURE, xii. 27; "Harvard Annals," xiv. 445). It was photometrically determined at Harvard as of 5·7 magnitude. Its attendant eluded Webb's search in 1855, Knott's in 1861 and 1871, but was recovered in 1875, when of 12·5 magnitude, by H. Sadler, using a 6½-inch Calver's reflector (Smyth, "Cycle," p. 202; Webb, *Popular Science Review*, xiv. 309). Since these stars are 60″ apart, the probability of their physical connection rests chiefly upon their agreement in exhibiting marked fluctuations of light.

ρ (5) Ophiuchi, described by Admiral Smyth as of 5 and 7·5 magnitudes at 4″; yellow and blue colours ("Cycle," p. 457). But Herschel at the Cape, 1834–37, and Jacob at Madras, 1846, found them exactly equal. Herschel and South in 1824, Secchi in 1856–57, give a difference of one magnitude. Main called them 4 and 4·3, and they were measured at Harvard as 5·3 and 6 magnitudes (Sadler, *Astronomical Register*, xvii. 73; Pickering, "Annals," xiv. 461).

β Scorpii is No. 489 of Gore's "Suspected Variables." F. Struve assigned to the components magnitudes 2 and 4; Pickering, 3 and 5·2, combined, 2·9. J. Herschel found a difference of only one magnitude, Webb of 3½, Gore of 2½ magnitudes (Webb, "Celestial Objects," p. 386; see also NATURE, vol. xxiii. 206, 362). Their colours are yellowish-white and lilac, or (according to Dembowski) ashy green, and they belong to the first spectroscopic class. They are separated by an interval of 14″, but Burnham detected in 1881 a close, faint attendant upon the principal star (Memoirs R. A. Society, xlvii. 193).

θ Serpentis = Σ 2417.—The components are 21″·6 apart, and relatively fixed. Both emit yellowish-white light marked by the Sirian quality of absorption. They were together ranked by Tycho and Bayer as of third magnitude, by Montanari as of fifth, but, with noticeable subsequent brightening (J. Cassini, "Élémens d'Astr.," p. 74). Gould's estimates wavered from 4·1 to 4·6 magnitude, and gave strong evidence of variability in one of the stars ("Uranometria Argentina," p. 322). Gore thinks that its changes may prove to be modelled on those of Algol (Journ. Liverpool Astr. Soc., v. 110). The separate photometric magnitudes registered at Oxford were 3·9 and 4·2; at Harvard, 4·7 and 5·1, where, however, the difference of lustre between the stars was perceived, in 1878, to fluctuate from 0·34 to 1·69 magnitude ("Harvard Annals," xi. 136, xiv. 463). Dunér considered the principal star to be steadily of 4, the companion to vary from 4·2 to 4·7 magnitude ("Mésures Microm.," 1876, p. 112).

Σ 1875 is composed of two white stars at 3″·2, the preceding of about ninth, the following varying from 8·5 to tenth magnitude. Dunér had no doubt of the reality of these changes ("Mésures Microm.," p. 70; Struve, "Mens. Microm.," p. 73).

Atlas Pleiadum = Σ 453.—Found double at 0″79 by Struve in 1827, doubtfully "wedged" in 1830, single with a power of 800 in a clear sky in 1836 ("Mens Microm.," p. 283). As single it has been seen by every subsequent observer, including Burnham, who at intervals during five years searched vainly for the companion detected and always fully believed in by Struve (Mems. R. A. Soc., xliv. 230). But during the passage of the moon across the Pleiades, January 6, 1876, Hartwig recorded the immersion of Atlas as non-instantaneous, a faint remnant surviving the chief part of the light for six-tenths of a second. He did not then know that the star had been marked at Pulkowa "duplex difficillima" (Winnecke, *Astr. Nach.*, No. 2074).

72 Ophiuchi = OΣ 342 is a somewhat similar example. An eighth magnitude companion at 1″·5 was discovered by O. Struve in 1842, but could rarely afterwards be seen, and excited vehement suspicions of pronounced variability ("Pulkowa Catalogue," No. 342). It was last observed at Pulkowa in 1876, and never elsewhere than at Pulkowa except once by Father Secchi at Rome in August 1859 (NATURE, vol. xvi. p. 194). Newcomb could find no trace of it with the Washington 26-inch on two exceptionally fine nights in 1874, nor Hall in 1876. Burnham was equally unsuccessful, and after much fruitless scrutiny recorded the star as "certainly single" in a "first-class night" of August 1880 (Mems. R. A. Soc., xliv. 276). Its spectrum, like that of Atlas in the Pleiades, is conspicuously of the first type.

β Cygni was found by Klein variable from 3·3 to 3·9 magnitude in 1862–63 (*Astr. Nach.*, No. 1663). Espin holds this star to belong to a distinct class of variables (exemplified by 63 Cygni) which change less than one magnitude in a period of several years (*Monthly Notices*, xliii. 271). Webb and Gemmill agreed, in 1881–82, in finding β Cygni much waned from its former brightness (*Astr. Reg.*, xx. 14, 46). The magnitudes of its components were determined at Harvard as 3·1 and 5·2—conjointly, 3. Their photometric difference, however, appeared, from Oxford measures of November 6, 1882, to be only 1·82 magnitude (*Monthly Notices*, xliii. 102). Although 34″ apart, and immovable, their physical union is decisively affirmed by the splendid contrast of their golden and azure tints, to which complementary absorption-spectra correspond (Huggins, Phil. Trans., cliv. 431).

δ Cephei forms, with a "cærulean blue," seventh magnitude companion at 41″, a pair resembling β Cygni (Webb, "Celestial Objects," p. 270). The large star varies regularly from 3·7 to 4·9 magnitude in 5d. 8h. 48m. The maximum of May 6, 1868, was, however, stated by Schmidt to have been barely indicated (*Astr. Nach.*, No. 1745). A minute attendant at 19″ was discovered by Burnham in 1880. The spectrum of the variable is of the solar type. It has virtually *no* proper motion.

α Herculis = Σ 2140 was divided by Maskelyne, August 7, 1777. The variability of the primary, discovered by Herschel in 1795, ranges from 3·1 to 3·9 magnitude in a period fluctuating between 26 and 103 days (Gore's "Variables," No. 129). The attendant is generally rated at the sixth, but Struve found it to change from fifth to seventh magnitude ("Mens. Microm.," p. 97). The colours of the pair are vividly contrasted orange and emerald. The large star shows a magnificent banded spectrum of III.*a* type; the smaller, one analogous to that of the companion of β Cygni in having its absorption almost wholly below the green (Huggins, Phil. Trans., cliv. 432). The common proper motion of the pair carries them in a century over a space nearly equal to the interval separating them (4″·5).

h 1470 = Lalande 38428.—Both stars are supposed to be variable, but have been little observed. Secchi estimated them of 7 and 8 magnitudes in 1856, and measured their distance at 23″·8. Physical relationship is indicated by their "superb" coloration in red and blue ("Catalogo di 1321 Stelle Doppie," p. 117; Webb

"Celestial Objects," p. 294). The spectrum of the red component resembles that of *a* Herculis (Espin, *Astr. Nach.*, No. 2825).

U Cygni = Schjellerup 239*a* was discovered by Knott, in 1871, to vary from 7·7 to below 11 magnitude in a period of 466 days (Gore's "Variables," No. 163). It had previously been remarked by Birmingham for its deep ruby tint (*Astr. Nach.* No. 1809). An attendant at 62″ appears to fluctuate in light from 8 to 8·7 magnitude, in colour from a decided blue to white and reddish (Birmingham, Trans. R. Irish Academy, xxvi. 300; Tarrant, Journ. Liverpool Astr. Soc. vi. 124; *English Mechanic*, xliv. 368; Gemmill, *ibid.*, xlvi. 340). The spectrum of U Cygni is of III.*b* type, but the zones are feeble (Dunér, "Étoiles de la 3e Classe," p. 73).

U Cassiopeiæ = OΣ (App.) 254.—A pair very similar to the preceding. The red star (= Schjellerup 280) varies from 7 to 9, the blue from 8 to 10 magnitudes, both in uncertain periods. Their distance, as measured by Dembowski in 1873, and by Burnham in 1881, is 58″·84 ("Publications of Washburn Observatory," i. 157).

U Puppis = Lalande 14551, found by Espin in 1883 to vary from 6 to 6·8 magnitude in 14d. 0h. 21½m. (*Monthly Notices*, xliii. 432). Burnham resolved it January 28, 1875, into two components, respectively of 6·5 and 8·5 magnitudes at 0″·8 (*Astr. Nach.*, No. 2062). Colour yellowish; spectrum of the solar kind. Proper motion insensible (Sadler, Journ. Liverpool Astr. Soc., v. 142). With a ninth magnitude star at 20″, it forms the fixed pair Σ 1097.

U Tauri is no longer included in lists of variables, the fluctuations noticed by Baxendell, 1865–71, having ceased to be perceptible (Schönfeld, *Jahresbericht*, Mannheim, xl. 51). It is unknown whether they affected one or both of two nearly equal components of 9·7 magnitude (distance 4½″), into which Knott divided the star, December 4, 1867 (Mems. R. A. Soc., xliii. 78). This interesting object has received little or no attention of late.

η Geminorum was discovered by Burnham at Mount Hamilton, November 11, 1881, to be made up of a third and a ninth magnitude star at 0″·96. "A splendid unequal pair," he remarked, "and likely to prove an interesting system" (Mems. R. A. Soc., xlvii. 204). He re-examined it at Dearborn a couple of months later, but we are not aware of any subsequent observation. The variability of *η* Geminorum in a period of 229 days was noticed by Schmidt in 1865. Its greatest extent of one magnitude is rarely attained, and the phases often seem nearly obliterated (Schmidt, *Astr. Nach.*, Nos. 1745, 1988, 2297.) The spectrum is an ill-marked specimen of Class III.*a*.

Y Virginis = Lalande 25086 was found by Schmidt in 1866 to vary from fifth to eighth magnitude in an undetermined period (*Astr. Nach.*, No. 1597). Ptolemy marked it of fifth, Abdurrahman Sûfi as approaching sixth magnitude (Schjellerup, "Description des Étoiles," p. 160). Piazzi catalogued it eighth, but observed it 6·7 and 7 magnitude. It figures in Lalande as of 6·5, in the Madras and Brisbane Catalogues as of sixth magnitude (NATURE, vol. xx. p. 248; "Harvard Annals," xiv. 456). Photometrically determined at Harvard, it came out of 5·7 magnitude. It is the only "Sirian" star showing considerable irregular fluctuations. Its duplicity was detected by Burnham in 1879, the components (0″·48 apart) being estimated as of 6·2 and 6·5 magnitudes. Re-measurements on three nights of 1881 gave no conclusive evidence of change (*Observatory*, iii. 92; Mems. R. A. Soc., xlvii. 190).

A. M. CLERKE.

NOTES.

THE medals of the Royal Society have been this year awarded as follows:—The Copley Medal to Prof. Huxley, for his investigations on the morphology and histology of vertebrate and invertebrate animals; the Rumford Medal to Prof. P. Tacchini for his investigations on the physics of the sun; and the Davy Medal to Mr. W. Crookes, for his investigations on the behaviour of substances under the influence of the electric discharge in a high vacuum. The Royal Medals have, with the approval of Her Majesty, been awarded to Baron Ferdinand von Mueller, for his investigations of the flora of Australia, and to Prof. Osborne Reynolds, for his investigations in mathematical and experimental physics. The medals will be presented at the anniversary meeting on November 30.

FREQUENT application having been made to Mrs. Spottiswoode for copies of papers by her late husband, the President of the Royal Society, she has decided to have them published in a collected form. The collection and editing of the mathematical papers she has intrusted to Mr. R. Tucker.

THE tone of the debate on the Education Estimates last Friday was eminently satisfactory. All who took part in it seemed to recognize that our system of elementary education is still very far from perfection. Sir John Lubbock evidently expressed the general feeling of the House of Commons when he complained that "the great faults of the present system were that it was too bookish and too dry." Mr. Mundella had a good deal to say—and said it well—on the necessity of the education of children being carried on to a much more advanced stage than that at which it now usually stops. "So long as the school life of the child was so short and limited," he said, "it was no use, in his judgment, talking about improved methods or an improved curriculum. So long as a child could enter a factory as a half-timer at ten years of age, or, as was the case in 8000 or 10,000 parishes in England, children were allowed to leave school after passing Standard IV., it did not matter what their curriculum was, or what their methods were, they could have no good results. It was impossible for them to force a number of compulsory subjects into a child who was to follow the ploughtail before he was eleven years of age. In the counties around London it was found that children left school after passing Standard IV., which they generally did about ten. There could not be a greater waste of money than to educate a child up to ten years of age at the expense of the State, and then turn him out into the world, the eventual result being that by the time he had reached thirteen he had forgotten everything he had learnt." After quoting from the report of Mr. Matthew Arnold as to the curriculum in force in Germany, showing that in Hamburg, for instance, there are thirteen obligatory subjects taught in the elementary schools, English being one of the subjects, Mr. Mundella pointed out that in Prussia no child leaves school until he is fourteen. Even after he leaves school, unless he can satisfy the school authorities, he must attend the continuation schools until he reaches sixteen or seventeen years of age.

ADMIRAL MOUCHEZ has received a magnificent set of photographs sent by the French Embassy at Pekin, illustrating Mr. Russell's lecture on the Pekin Observatory, of which we gave an account last week. These photographs will be exhibited in the astronomical museum of the Paris Observatory.

DURING the recent meeting of the British Association at Bath, Mr. G. J. Symons found in the Jenyns Library a manuscript meteorological register of considerable importance—namely, the original daily records kept by the Rev. James Cowe, at Sunbury Vicarage, Middlesex, from 1795 to 1839. It gives barometer, maximum and minimum temperatures, wind, rain, and remarks for each day. This record covers a period respecting which there has been much uncertainty as to both temperature and rainfall, and several meteorologists are of opinion that it should

in some way be rendered generally accessible. The Rev. Leonard Blomefield (formerly Jenyns), to whom the manuscript belonged, has given his consent to its being copied, and an estimate has been obtained of the cost of copying it, and of preparing a lithographed reproduction as a foolscap folio volume of about 530 pages. This can be done, if sixty persons will subscribe a guinea each. As the volume will be one of great interest to all students of meteorology, Mr. Symons should not have much difficulty in obtaining the necessary funds. The sum of £24 has already been promised by various scientific societies and individual subscribers. Not one copy more than those subscribed for will be printed, so that the volume can only be obtained by subscription, and can never become cheap or common.

THE *American Meteorological Journal* for October contains an article by Mr. A. L. Rotch on the organization of the Meteorological service in Russia. Observations were first made there by the Academy of Sciences in 1726, but it was not until 1833 that a regular system was established. Weather telegraphy was begun in 1864, but little was done until 1872, when a daily weather report was commenced, but forecasts are not issued even now. The same number contains a summary of experiments by MM. C. Montigny and F. Dechevrens on the inclination of the wind, in contradistinction to its horizontal motion usually referred to. One interesting fact has come out of these experiments, viz. that the positive inclination (ascent) of the wind increases in proportion as the barometer falls, and *vice versâ*. We regret to see that the closing of the stations on Pike's Peak and Mount Washington, at least for the winter season, is an accomplished fact. The reason is said to be lack of funds, and the impossibility of using the observations for weather predictions. The detailed observations on Pike's Peak are about to be published by Prof. Pickering in the Annals of Harvard College.

THE Pilot Chart of the North Atlantic Ocean for October, shows that the month of September was characterized by the occurrence of three West Indian hurricanes. By far the most notable of these was the great Cuban hurricane of September 1-7, referred to in NATURE, vol. xxxviii. p. 485. The first indications of this disturbance were noticed in the north-east trade-wind belt, east of the 60th meridian, on August 30 and 31. Taking a west by north course, the centre reached the Cuban coast on the night of September 3, spreading devastation on every side. After passing south of Havana on the 5th, a most remarkable feature was developed by a change of direction to the south of west, in violation of the usual law of motion, the storm reaching Mexico, near Vera Cruz, on the 7th. This unusual change of direction was apparently due to the fact that another well-defined storm originated in the same locality about the same time. This storm was central over the Bahamas on the 6th and 7th, while an area of very high barometer prevailed over the middle Atlantic States. There was a marked decrease in the amount of fog along the Transatlantic routes, which is attributed to the fact that only three depressions of any extent passed over those regions during the month. One of these, which developed hurricane force on the 12th in longitude 37°, subsequently passed north of the British Islands, after having crossed the entire ocean.

THE *Russian Gazette* of September 10/22, 1888, announces that the Committee of the Caspian Fisheries has deposited in the Astrakhan branch of the Imperial State Bank the sum of 5000 roubles (about £500) as a prize to be awarded for the discovery of means for the protection of fish against infection, and for the treatment of people suffering from the effects following the consumption of poisonous fish. The prize will be awarded to the person who will accomplish the following: (1) determine by careful analysis the physical and chemical nature of fish poison; (2) investigate by experiments on animals the action of fish poison on the heart, circulation of the blood, digestive organs, and nervous system; (3) determine the rapidity of the absorption of the poison in the digestive channels; (4) ascertain and describe the symptoms which distinguish healthy fish from those in a diseased condition; (5) indicate the measures to be adopted for preserving fish against the development of fish poison in them; (6) discover an antidote for, and the nature of the medical assistance to be rendered in cases of fish poisoning. Both Russian and foreign men of science may compete for the prize. Essays on the subject may be written either in the Russian, Latin, French, English, or German languages, and may be sent in print or manuscript not later than January 1/13, 1893, to the Ministry of Imperial Domains. The non-solution of questions Nos. 4 and 5 of the foregoing questions will not be considered an obstacle to the award of the prize in full, should the other four questions be satisfactorily dealt with. In case it be considered that not one of the essays or works submitted has solved the problem in its most essential parts, the Commission to which the essays will be submitted may award as a secondary prize the sum which shall have accrued as interest during the five years on the sum of 5000 roubles to the author of the work which shall have satisfactorily dealt with a portion of the programme, and which may facilitate a further study of the nature of fish poison.

THE following figures give some idea of the number of animals killed every year in Siberia for the sake of their furs. At the last summer fair of Irbit, which is a market for only a part of the furs exported from Siberia, no less than 3,180,000 furs of squirrels were offered for sale. Of these, 1,018,000 were killed in the forests of Yeniseisk, 455,000 in the Altai Mountains, 200,000 in Yakutsk, and 300,000 in Transbaikalia. A considerable number of the furs of squirrels killed in the last two provinces are exported directly both to China and to Russia, without passing through Irbit. It is worthy of note that this year there was a considerable decrease in the furs of the black squirrel. Only half a million of these furs were brought to the fair, as against more than a million in 1887. Of other furs there were 11,000 blue fox (*Canis lagopus*), from Obdorsk and Berezoff; 140,000 marmots, chiefly from the Altai; 30,000 polecats, 10,000 badgers, 1,300,000 hares, 2000 foxes, and numbers of bears and wolves. The extermination of fur-bearing animals goes on with such rapidity that there are whole regions where hunting has been completely abandoned in consequence of the complete disappearance of the Mustelidæ and the rarity of the squirrel.

THE November number of the *Kew Bulletin* contains valuable papers on Lagos rubber, Liberian coffee at the Straits Settlements, tea oil and cake, Demerara pink root, food grains of India (continued), Yoruba indigo, Trinidad ipecacuanha, treatment of vines in France, huskless barley, and ramie.

THE current number (vol. xxii. New Series, No. 5) of the Journal of the North China Branch of the Royal Asiatic Society is wholly occupied by the first part of a paper, by Mr. A. Henry, on the Chinese names of plants. This instalment contains 565 items. The names are those in colloquial use at Ichang, a town on the Yang-tsze, and the neighbourhood. First we get the Chinese name in Latin letters, then in Chinese characters; these are followed by the scientific name, which is in nearly every case taken from lists compiled at Kew, and by a general description of the plant, and occasionally the use to which it is put by the people. The second part will contain additions to the list, and notes concerning the plant-names that occur in Chinese botanical works.

THE zinc, manganese, and cobalt salts of molybdic acid, H_2MoO_4, have been obtained pure and in fine crystals by M. Coloriano, of Bucharest. Although molybdenum is so interesting an element, forming as it does such a beautifully graduated series of oxides and their corresponding salts, it is rather remarkable that so little is yet known concerning the most important of these latter, the molybdates. On attempting to obtain the zinc, manganese, and cobalt molybdates by double decomposition, using solutions of known strength of the nitrates of the metals in question, and of ammonium molybdate, amorphous precipitates were thrown down, consisting of hydrated acid salts of molybdic acid. On digesting these precipitates for a short time with water, it was found that they became rapidly converted into a mass of crystals, which were eventually separated from the remaining amorphous substances, washed and dried. They were then subjected to analysis, and were found to be normal molybdates, containing water of hydration so firmly combined that the first traces only commence to be expelled at 150° C. and perfect dehydration cannot be effected lower than the temperature of boiling sulphur (447°). Hence it is concluded that the water present is of "constitution." The crystals of zinc molybdate, $ZnMoO_4 . H_2O$ or $MoO_2 \begin{cases} OH \\ O{-}Zn{-}OH \end{cases}$, are transparent and colourless, forming acicular stellar groups. They dissolve but sparingly in water, readily, however, in dilute acids. The manganese salt, $MnMoO_4 . H_2O$, is probably isomorphous with the zinc molybdate just described, the crystals being very similar in form, but distinguished by possessing a bright yellow colour. The cobalt compound, $CoMoO_4 . H_2O$, is also similar in constitution, but the crystals are very much more beautiful. They exhibit a magnificent violet colour, and are almost insoluble in pure water, though readily dissolved by even very dilute acids. All three salts are probably isomorphous, and are similarly attacked by alkalies. An analogous nickel salt was also obtained, the crystals being of precisely the same habit as the three salts above described; but owing to the extreme slowness with which the amorphous acid salt is converted into the crystalline normal one, sufficient quantities have not yet been obtained pure for analysis. From the description given, however, by M. Coloriano, it may be accepted that zinc, manganese, cobalt, and nickel form isomorphous normal molybdates each containing one molecule of water of hydration.

AN interesting paper on "the nephrite question," by Dr. A. B. Meyer, is printed in the *American Anthropologist*. Dr. Meyer is of opinion that too much has been made of the fact that objects of nephrite and jade have been found in districts where these minerals in their natural state have not yet been discovered. It is rash, he thinks, to conclude that the objects must have been brought from a great distance. Prehistoric men may, he suggests, have found nephrite and jade in places where they have escaped our notice. "It may be that the people of prehistoric times continually sought the valuable material in a way quite different from the one we adopt. That boulders in the rivers formed their main source of supply is proved by the fact that a large number of the hatchets show the boulder characteristics, and they certainly left no stone unturned in their endeavour to find them, while we never think of looking for them in rivers. They may also have searched for still greater finds, the last view being supported by the fact that finds have been made as late as the last century in North Germany, in the sand near Potsdam, at Schwensal, near Merseberg, and at Leipzig, and these were evidently nephrite boulders of the North German diluviums. The remarkable block at Leipzig weighed over 38 kilogrammes; it is looked upon by Prof. Fischer and others as having been accidentally lost at that place, and they think that the raw material is of Asiatic origin. Considering that the block weighed nearly 100 pounds, this is not very likely, and I am of the opinion that this nephrite boulder came from Scandinavia, and that it was transported by ice."

LIEUT. D. BRUUN, of the Danish army, having had a moss dug out in Finderup, in Jutland, has made some interesting discoveries. In the moss were found trunks of oak, beech, and fir trees, from 6 to 30 inches in diameter. The branches had in some cases been cut off, but the bark remained. By the side of one of the oak trunks two earthen vessels were discovered, and near another a third, shaped like an urn. In the latter lay a sandal cut from a piece of leather, with flaps, and leather straps for tying to the ankle, the length of the sandal being 7 inches. It seemed as if the trunks of trees had been placed in a certain position for some purpose or another. About 20 feet further to the south, and at the same depth, viz. 6 feet, a yoke of oak was found, $5\frac{1}{8}$ feet long and 3 inches thick, being fairly cylindrically cut out in the centre. At each end were holes, in one of which remained a strap of leather. Other implements of oak were also found, evidently used for carrying. Some of them seemed part of a wheel. Close to the yoke another earthen urn was discovered, which, like the three referred to, was surrounded with sprigs of heather and bramble. Formerly some horns of bullocks and the skeleton of a man in a fur coating were found in the moss. The various objects are now in the Copenhagen Museum, and are said to date from the early Iron Age.

DURING the past summer a great "kitchen-midden" at Grenaa, in Jutland, was the subject of research by the authorities of the Norse Museum at Copenhagen. It is situated a couple of miles from the sea in the midst of a wood. The layers in the midden were clearly defined, the numerous places of cooking being made conspicuous by ashes and remains of charcoal, together with fragments of coarse pottery. A large quantity of animal remains were found, viz. knuckles and bones, which had been split in twain or crushed and the marrow extracted. There were also many shells of oysters and other mollusks. The bones were those of deer, boar, seal, fox, wolf, dog, swan, and goose, with some of fish. Many flint weapons and implements were found, as well as some of horn and bone. Several of the latter had traces of ornamentation.

WE have received the Report of the Survey of India for the year ending September 30, 1887. During the year, the trigonometrical party has extended the triangulation on the Madras coast 170 miles, this being part of the general scheme to complete the triangulation on the coasts of India and Burmah. The aggregate results of the topographical surveys amounted to 17,510 square miles, and the areas of forests to 34,289 miles. In Basti, close upon 2,000,000 plots were cadastrally surveyed, the extent of each only averaging 0·27 of an acre; in Gorakhpur, in a considerably larger area, less than 1,000,000 plots were surveyed. After a little preliminary difficulty, it was found possible to advantageously employ village natives in this work. Altogether, 4273 square miles were cadastrally surveyed. A new map of Calcutta, on a scale of 50 feet to an inch, has also been undertaken. The longitude determinations have been suspended, but the latitudes of five stations on the meridian of 80° have been completed. Advantage has been taken of the various military operations in Upper Burmah to record additional geographical information. The total number of maps issued amounted to 178,398. At Dehra Dun, photographs of the sun were taken for the Solar Physics Committee, South Kensington, on every available day—in all, sixty-three days being lost. Attempts were also made to photograph the corona by means of stained plates, but their success seems somewhat doubtful. As usual, the Report is accompanied by maps showing the extent of the various operations.

THE Sebastopol Biological Station, under Dr. Sophie Pereyaslavtseva, continues to bring out important biological works. In the last number of the *Bulletin* of the Moscow Society of Naturalists (1888, No. 2), Mrs. Pereyaslavtseva publishes (in French) the first instalment of a most valuable work which she has written in association with Miss Marie Rossiiskaya, on the embryogeny of the Amphipods, being a continuation of her previous studies in the embryogeny of Rotifers. The development of *Gammarus pœcilurus* is described in the first part of the work, and an idea of its detailed character may best be given by mentioning that the various stages of development of that one species are illustrated by no less than one hundred and twenty microscopical sections beautifully printed in colours. Two more representatives of Amphipods (*Caprella* and *Orchestia*) have been studied in the same way, while the lady students who work at Sebastopol under the learned lady-director of the station are now studying other species of Amphipods, and especially of *Gammarus*; so that a complete work on the embryogeny of that important zoological division is expected to be ready by the end of the autumn. For the present, Mrs. Pereyaslavtseva refrains from suggesting general conclusions, but at the end of her monograph she points out that throughout the embryonal development of *Gammarus pœcilurus* the cells of its tissues are endowed with amœboid movements. Those movements are less pronounced in the ectodermic and mesodermic layers, and yet the cells of the former are moving and protruding pseudopods even when the endodermic layer has taken the shape of a fully formed pouch, and its constitutive cells may be considered as epithelium. As to the cells of the mesoderm, they maintain the capacity of both locomotion and overlapping (*chevauchement*) even at very advanced stages of the development of the embryo—that is, until the elaboration of the muscular tissue has been completed. These phenomena have been noticed in all the three genera of Amphipods already studied, and most probably they are common to all Amphipods.

MESSRS. MARION AND CO. send us an account of a "detective camera" which has been planned to meet the requirements of the inexperienced as well as the experienced in photography. It has the appearance of a leather dressing-case or despatch-box, and has the special advantage that the person using it sees the exact picture he is to get on his plate, the same lens giving the image on the screen and taking the negative. Another "detective camera" of which Messrs. Marion and Co. have issued a description is in the form of a book, and can be used secretly, since there is nothing to indicate its real purpose.

IN an article on "Irregular Star Clusters" (NATURE, November 1, p. 13), it was stated, with regard to an apparent member of a scattered group in Ophiuchus, that its position "was found, by the comparison of photographs taken by M. von Gothard in 1886 with Vogel's measures of eighteen years previously, to have changed to the extent of 45″, or at the rate of 2½″ annually (*Astr. Nach.*, No. 2777)." Dr. H. Kreutz, of the Kiel Observatory, writes to us to say that more recent measures of Dr. B. von Engelhardt (*Astr. Nach.*, No. 2859) have proved this to be incorrect. The difference between Gothard's photographs and Vogel's measures was due to an error in Vogel's work.

THE additions to the Zoological Society's Gardens during the past week include two White-tailed Eagles (*Haliœtus albicilla*), British, presented by Mr. R. H. Venables Kyrke; two Short-eared Owls (*Otus brachyotus*), captured in the Red Sea, presented by Captain John Marr; a Little Grebe (*Tachybaptes fluviatilis*), British, presented by Mr. Howard Bunn; two Spotted Ichneumons (*Herpestes nepalensis* ♂♂), an Indian Otter (*Lutra nair* ♂) from India, a Sclavonian Grebe (*Podiceps auritus*), British, deposited; four Knots (*Tringa canutus*), European, purchased.

OUR ASTRONOMICAL COLUMN.

THE TOTAL SOLAR ECLIPSE OF AUGUST 29, 1886.—Part 5 of vol. xviii. of the Annals of the Harvard College Observatory, contains an account by Mr. W. H. Pickering of his expedition to Grenada in 1886 in order to observe the total eclipse of August 29; and some points in his report have recently been commented on by Mr. W. H. Wesley (*Observatory*, October 1888) and Mr. Ranyard (*Knowledge*, November 1888). Mr. Pickering's original plan of work had been a very wide one, and he took out a great variety of instruments with him, but no assistants besides his wife and a lady friend. It was very late in August before he arrived at Grenada, and this circumstance and the frequent obscuration of the sun before totality on the day of the eclipse caused several items of his programme to result in complete failure. The long focus photoheliograph and the actinometer under Mr. Pickering's own superintendence gave no results, but Mrs. Pickering secured three photographs with a couple of short-focus cameras, and Mr. Glean one with a telescope of 4 feet focus. One of Mrs. Pickering's photographs supplies some very curious features in the shape of some very faint extensions of the corona on the western side of the sun. One of these is a prolongation of a bright synclinal mass, and rises in a narrow jet to a height of 48′ from the limb, and then divides into three parts, two falling back towards the sun right and left of the centre ray, which attains a total height of 60′, then to bend over in a precisely similar fashion. Another extension further to the north rises to about the same height, 60′, and then curves downward again.

Mr. Pickering's spectrum photographs afforded little fresh information, but confirmed Prof. Tacchini's observation of "white" prominences; and two of his small coronal photographs were used to give a determination of the brightness of the corona. These gave the total actinic brilliancy of the corona with the surrounding sky as 700 units, or ten times that of the full moon with surrounding sky. But the *intrinsic* actinic brightness of the brightest part of the corona was only 0·03, whilst the average intrinsic brightness of the sky 1° from the sun on a fine day was determined to be 1200 times as great.

COMET 1888 *f* (BARNARD).—Dr. R. Spitaler has computed the following elements and ephemeris for this comet from observations made at Mount Hamilton, October 30, at Vienna, November 2, and at Hamburg, November 5:—

T = 1888 September 10·82914 Berlin M.T.

$$\left.\begin{aligned} \pi &= 65^\circ\ 0'\ 12'' \\ \Omega &= 137\ 34\ 17 \\ \iota &= 55\ 17\ 10 \end{aligned}\right\}\ \text{Mean Eq. 1888·0.}$$

$\log q = 0{\cdot}16873$

Error of middle place (O – C).

$\Delta\lambda \cos\beta = -4''$; $\Delta\beta = 0$.

Ephemeris for Berlin Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
Nov. 16	10 5 37	12 25·4 S.	0·2197	0·2414	0·96
20	10 9 56	11 34·3	0·2141	0·2487	0·95
24	10 13 50	10 39·0	0·2081	0·2561	0·94
28	10 17 46	9 38·2 S.	0·2018	0·2636	0·94

The brightness at discovery is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *NOVEMBER* 18–24.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 18

Sun rises, 7h. 26m.; souths, 11h. 45m. 26·0s.; sets, 16h. 4m.: right asc. on meridian, 15h. 37·2m.; decl. 19° 25′ S. Sidereal Time at Sunset, 19h. 57m.

Moon (Full on November 18, 15h.) rises, 4h. 32m.; souths, 0h. 5m.*; sets, 7h. 49m.*: right asc. on meridian, 3h. 59·3m.; decl. 16° 30′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	5 28	10 31	15 34	14 22·4	11 43 S.
Venus ...	10 27	14 7	17 47	17 59·6	25 7 S.
Mars ...	11 41	15 34	19 27	19 26·1	23 28 S.
Jupiter ...	8 50	12 53	16 56	16 45·5	21 54 S.
Saturn ...	22 15*	5 41	13 7	9 31·6	15 38 N.
Uranus ...	3 56	9 23	14 50	13 14·7	7 15 S.
Neptune..	16 22*	0 7	7 52	3 57·1	18 41 N.

* Indicates that the rising is that of the preceding evening and the southing and setting those of the following morning.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
S Ceti	0 18·4	9 57 S.	Nov. 19,		*M*
U Cephei	0 52·4	81 16 N.	,, 20,	0 47	*m*
Algol	3 0·9	40 31 N.	,, 19,	22 11	*m*
			,, 22,	19 0	*m*
λ Tauri	3 54·5	12 10 N.	,, 18,	22 59	*m*
			,, 22,	21 52	*m*
ζ Geminorum	6 57·5	20 44 N.	,, 18,	23 0	*m*
			,, 23,	23 0	*M*
R Canis Majoris	7 14·5	16 12 N.	,, 18,	5 5	*m*
W Virginis	13 20·3	2 48 S.	,, 23,	1 0	*M*
S Coronæ	15 16·8	31 46 N.	,, 22,		*m*
β Lyræ	18 46·0	33 14 N.	,, 18,	0 0	*m*
R Lyræ	18 51·9	43 48 N.	,, 18,		*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 21,	0 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 19,	2 30	*m*
			,, 22,	2 24	*m*
δ Cephei	22 25·0	57 51 N.	,, 18,	4 0	*m*
			,, 19,	19 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near κ Leonis	140°	27° N.	Very swift.
,, θ Ursæ Majoris	143	50 N.	,, ,,
,, λ Ursæ Majoris	154	40 N.	Swift; streaks. The Leo Minorids.

GEOGRAPHICAL NOTES.

WE are glad to learn from Denmark that Dr. Nansen has been successful in crossing Greenland. Dr. Nansen, it will be remembered, left the ship in a boat off the south-east coast of Greenland, 65° 2′ N., on July 17. He knew his party had to sail south among the ice for twelve days before they succeeded in landing to the north of Cape Farewell in lat. 61°. As he came out at Godthaab, on the opposite coast, in October, he has taken about three months on the journey, which was made in a line about sixty miles south of that he intended to follow. The section crossed by Dr. Nansen's expedition is in the south and narrow part of Greenland, Nordenskjöld's route having been much farther north, and almost in the centre of the land. Unfortunately, Dr. Nansen just missed the last ship from Greenland to Europe, so that he will have to remain at Godthaab till May next. Until then we must wait for full details.

THE paper read at the first meeting this session of the Royal Geographical Society, on Monday night, was by Mr. H. H. Johnston, H.M. Vice-Consul for the Oil Rivers, on the Niger Delta. The "Oil Rivers," Mr. Johnston said—so called from the fact of their producing the bulk of the palm-oil exported from West Africa—are the main rivers, creeks, and estuaries lying between the eastern boundary of the British colony of Lagos and the northern frontier of the German Protectorate of the Cameroons. They are chiefly branches of the Niger, and form the Niger delta, but some few of them have sources independent of that great stream; although close to the sea-coast, within tidal influence, the estuaries of these rivers are interconnected by a wonderful network of more or less navigable creeks. This system of natural canalization is here and there blocked with vegetable growth, sandbanks, fallen trees, or artificial obstacles constructed by quarrelsome or timid natives; but with a relatively small amount of labour and at a moderate cost, the creeks in places might be deepened and cleared, and inland navigation rendered practicable between Dahome and the Cameroons Protectorate. Mr. Johnston then gave a graphic description of these rivers as they present themselves to one arriving on the coast from Europe. Arriving from Europe by sea, it is generally by the soundings and discoloured appearance of the water that we become aware of the near approach to land, rather than by sighting any part of the shore. When within a few miles of the mouth of one of these rivers, the low coast-line is at first indicated by isolated trees, which appear islets of forest unconnected with each other, and distorted by the mirage of each horizon. Gradually these islets, which are really the loftier trees of the fringe of coast forest, become united in one line of purple green, divided only by the imposing gate of the estuary, for which our ship is bound. The bar of the river may be—as in the case of Old Calabar and Bonny—so deep as to be without danger, or it may be relatively shallow, as at Opobo or Akasa. Once over the bar and within the estuary, we find ourselves surrounded by a lake-like expanse of smooth water, the shores of which are fringed with lofty mangroves with their ghastly white blood-streaked trunks—streaked where the bark has been torn or frayed—and their graceful poplar-like foliage of a sad, dull, yellow-green. Behind the mangroves, however, generally show the dark and dense masses of inland forest, growing where the land has acquired firmness and lies just above the limits of high tide. As far as can be seen from the ship's deck, all and everything that is not yellow water is unvarying mangrove. As you ascend the river further and further from the sea, the mangrove loses its exclusive possession of the shores, even if this possession be not here and there broken by little islets of firm land covered with varied vegetation, and generally the sites of villages. Almost before the water has ceased to be brackish, the Pandanus or screw-pine begins to oust the mangrove, and below its fantastic whorls of spiny leaves the lovely Lissochilus orchids conceal the black mud with their leaves, and rear their stout flower-stems to a height of 6 or 7 feet. As the river is ascended still further, though the banks continue marshy, the now perfectly fresh water enables a varied forest to replace the mangrove and Pandanus, and here perhaps the most extravagant development of vegetation may be seen, recalling past geological epochs rather than the poor and mediocre aspects of Nature at the present time. There is not one prominent kind of tree, but an infinite variety of kinds. There is every type of foliage and every shade of green. At the base of the forest on the water-line grow great Arums of the genus *Cyrtosperma*, with flower spathes of pale green streaked with purple red. Above the Arums gleam out the white bracts of a species of *Mussaenda*, while higher up another *Mussaenda* exhibits huge creamy-white flowers without any bracts at all, and yet another species of this beautiful genus has blossoms of a vivid scarlet. Over the lower branches of the trees hangs a thick green veil of convolvulus, dotted at intervals with large mauve flowers. The Raphia palms are also a characteristic of this river-side forest. Ascending this typical river still further, the marshy banks gradually become firm dry land, and the ground even rises from the water into wooded heights. Gradually the river narrows, and the banks increase in height, and red clay now gives place to outcropping rock. Looking interiorwards beyond the vista of the winding river is the exhilarating prospect of a faint blue range of hills. All influence of the tide has ceased, and the current becomes more rapid. It may be hours, or it may be days or weeks, before we reach the outlying spurs of the first range of hills, the first ascent to the central plateau, over the rapids and falls which mark the change from the interior to the coast region. Here you are out of the forest region of West Africa, in the great park-lands of the interior. Mr. Johnston then went on to describe in detail some of the more important places and districts comprised within the British Protectorate of the Niger Territories.

IN a paper read before the last meeting of the Berlin Geographical Society, Dr. von der Steinen described his second exploration on the Xingu, which began at Rio Janeiro in February 1887, and ended at Cuyaba, the capital of Matto Grosso, on December 31 last. The traveller summed up the main results of his journey thus: the topographical survey of the region through which he passed, numerous physical measurements, a complete grammar of the Bakairi of the Xingu, various vocabularies, and a rich collection of the most varied ethnological objects. During his long residence amongst the Xingu Indians, with whom he was on the most friendly and familiar terms, he was enabled to obtain a deeper insight into the manners and ideas of primitive man in the early stages of his culture than any other traveller. Unfortunately, a chest containing his geological specimens was lost, and many of the photographs were injured.

To the November number of *Petermann's Mitteilungen* Herr von Hesse-Wartegg contributes a paper on Lake Tacoragua, in Northern Venezuela, one of the few fresh-water lakes in South America. The oscillation in the extent of the lake is undoubted, ac-

cording to the writer. Humboldt found it 56 kilometres long, and Herr von Hesse-Wartegg only 49. Yet, while the former estimated the area of the lake at 424 square kilometres, the latter gives it at 587. The author gives many interesting details, not only about the lake, but also about the region in which it is situated. To the same number Dr. O. Krümmel contributes a paper, in which he endeavours to solve the old problem of the Euripus.

THE last supplementary issue (No. 91) of *Petermann's Mitteilungen* contains, according to its title, an account of a journey from Hankow to Soochow, and of journeys in Central and Western China between 1879 and 1881. The contents of this particular paper are misdescribed, for it contains only the record of a journey in 1875 from Shanghai to Hankow on the Yang-tsze, thence by the Han River through the Hupeh, Honan, and Shensi provinces to Lanchow in Kansu, and thence to Soochow, close to the Great Wall and the Mongolian deserts, where Herr Michaelis, the writer, remained for some time, and carried out certain explorations in the neighbourhood. Possibly another part or other parts are to follow, of which there is at present no indication. Herr Michaelis was employed in 1874 as a mining expert by the late Viceroy and General Tso Tsung Tang, who had just then chased the Mahommedan rebels out of the Shensi and Kansu provinces, and was about to begin his famous march to Kashgar. He was to investigate the region both within and without the Great Wall for mineral deposits, and especially for gold. Herr Michaelis met Count Szchenyi and his party in Soochow, and naturally a good deal of the ground he traversed has already been described by Lieut. Kreitner, who was surveyor to the Szchenyi Expedition, in his well-known book, "Im Fernen Osten." The paper is accompanied by three excellent route maps.

MOLECULAR PHYSICS: AN ATTEMPT AT A COMPREHENSIVE DYNAMICAL TREATMENT OF PHYSICAL AND CHEMICAL FORCES.[1]

IV.

§ 16. *Electrical Actions.*

IT follows from the principle of the conservation of energy that the processes which give rise to electrical excitation can themselves be called into play by electrical action.

The heating of a conductor by the passage of an electric current is easily explained on the author's theory that electrical conduction is effected by means of molecular vibrations. The electric spark he considers to be due to the separation of particles of the conductor heated in this manner.

The author explains the Peltier effect in the following manner. Let a closed metallic circuit be formed, consisting of two metals, soldered together at the points I. and II., and suppose the circuit to be traversed by a current flowing through the junction I., from the less easily excited metal A to the more easily excited metal B. The molecules of the metal A will then, by hypothesis, easily be thrown into vibration; the metal A will therefore be more heated than B, and will, moreover, be a worse conductor of heat than B. The heat excited in A at the junction I. will therefore be carried to warm the junction II. in the same direction as the current; it will then accumulate at this junction, for A, being the worse conductor, will carry away less heat from the junction II. than is carried to it through B. The junction II. will therefore be heated, while the junction I. will be cooled.[2]

The direct production of light by electrical action has already been considered in § 14 (October 11, p. 581). It is clear that secondary luminous phenomena may also come into play.

Both chemical combination and decomposition may be effected by means of electrical action. The author selects, as an example of the former, the combination of a mixture of oxygen and hydrogen to form water when traversed by electric sparks, which he considers to be due to the absorption by the molecules of the radiant electrical energy proceeding from the positive pole. The motion of the atoms would be accelerated, and the number of impacts increased, giving rise to a series of phenomena similar to those described in § 8[3] (September 6, p. 460). The internal vibrations of the newly-formed molecule will tend towards a steady state, in which the internal energy is as small as possible. Hydrogen and oxygen will unite to form water, supposing the molecules of the latter to be less electrically sensitive than those of its constituents. We should therefore conclude, from the fact that combination occurs under these circumstances, that water is only very slightly sensitive to electrical excitation, which is in agreement with the observed fact that pure water is an exceedingly bad conductor of electricity.

The decomposing action of electricity is exhibited in electrolytic phenomena. These occur in the inverse order to the chemical actions which serve to produce the current. The action is supposed by the author to take place as follows. The fluid receives electrical energy from the positive electrode, which excites electrical vibrations in the molecules immediately surrounding it. These vibrations are transmitted through the fluid according to the ordinary laws of hydrodynamics, but this would not necessarily give rise to an electric current through the liquid, for an accumulation of electricity may take place even in nonconductors. In consequence of these vibrations, however, the molecular impacts will occur more frequently, and a new steady state will be set up, provided such is possible, in which the internal energy has a smaller value than before. Decomposition will therefore take place if the separate constituents are less sensitive to electrical vibrations than when in combination, as their electrical energy will then be less than that of the compound. One of the constituents will, however, be excited to a greater extent than the other, and the one which is least excited will be attracted more strongly by the anode than the more highly excited one. The latter constituent will not, therefore, move towards the kathode with a definite velocity, but will remain where it is, and re-enter into combination with the opposite constituent of a neighbouring molecule. This would appear at first to be in contradiction with the assumption that the compound is more sensitive to electrical vibrations than its constituents, but the apparent contradiction is explained by the consideration that the internal energy lost during the decomposition of the first molecule must reappear in the form of external energy—that is to say, in the form of heat; and the heat thus set free will supply the electrical energy necessary to cause the recombination. This gives an explanation of the "migration of the ions." From particle to particle during this migration, alternate transformations of electrical energy into heat, and of heat into electrical energy, take place. A certain amount of electrical energy will be lost during the process—namely, the amount transformed into heat during the decomposition of the first molecule, and the heat developed in the solution will raise its temperature to such an extent as to cause a recombination between the products of the decomposition set free at the electrode—a result which is in agreement with observation.[1]

§ 17. *Rotation of the Plane of Polarization.*

One of the principal arguments in favour of Maxwell's electromagnetic theory of light is, that it gives an explanation of the rotation of the plane of polarization by an electric current on the assumption of the existence of molecular vortices. It is therefore of considerable importance to determine how far the author's theory is capable of explaining the same phenomenon. Suppose a right-handed spiral to be wound round the axis of X, proceeding from the origin in the positive direction. A current flowing through the spiral away from the origin will then produce a north pole at the origin and a south pole at the other extremity of the spiral. Let a ray of plane-polarized light traverse the solenoid in the direction of the axis, then every point on the axis will move in a short rectilinear path perpendicular to it. Now an electric current has been defined as consisting in a disturbance of the molecular equilibrium of the conductor, propagated along the conductor with great velocity by radiation from molecule to molecule through the intervening ether. The electrical vibrations may be assumed to take place in the conductor in every direction, as in the case of heat-waves; and, as a special case, a disturbance of equilibrium taking place in a single direction only must give rise to an electric current, so that every motion of the ether in a definite direction must be equivalent to an electric current. Motions of any great extent do not come into consideration, for every disturbance in the equilibrium of the ether must consist in vibrations; but, however small the light-vibrations may be, they must be considered,

[1] A Paper read before the Physico-Economic Society of Königsberg, by Prof. F. Lindemann, on April 5, 1888. Continued from vol. xxxviii. p. 581.

[2] In the original, some confusing misprints occur in this paragraph, viz. p. 38, second line, *besser* should be *schlechter*, and in the third and fourth lines A and B should be interchanged.—G. W. DE T.

[3] Since the oxygen and hydrogen molecules are electrically excited to different degrees, they will attract one another. A hydrogen molecule will therefore impinge upon an oxygen molecule more often than upon another hydrogen molecule, thus increasing the chemical action.

[1] See von Helmholtz's "Wissenschaftliche Abhandlungen," vol. ii. p. 958, *et seq.*

in virtue of previous assumptions, to be extremely large relatively to electrical vibrations. The path described by an ether particle originally lying in the axis must therefore be regarded as an alternating electric current of finite length perpendicular to the axis. It must therefore be a current of varying velocity, the velocity having its maximum value when the particle is crossing the axis, and becoming zero at the extremities of its path. Such a current will be deflected by the north pole of the solenoid with a force proportional to the velocity, and therefore the half-vibration will assume the form of a semicircular arc as in the case of an electric arc deflected by a magnet, the ends of the arc coinciding with the ends of the original rectilinear path. The particle during the second half of the vibrations will be deflected in the opposite direction, and therefore will return along the other half of the circle. The effect of the electric current in the solenoid will therefore be to transform the plane-polarized ray of light into a circularly-polarized ray.[1]

The circular motion of the ether particles will be in the opposite direction to that of the current in the solenoid; the small circular current will therefore correspond to a small magnet with its south pole directed towards the origin. The north pole of the solenoid will therefore attract these circular currents, and diminish the rate of propagation of the wave of light along the axis, as each of the circles will tend to approach the north pole of the solenoid. The effect of this motion, again, will be to produce an induction current in the circle in the opposite direction to the former one, and the circle will come to rest in a new position determined by the condition that these two currents shall be in equilibrium. The induced current will be repelled from the north pole, and all these induced currents will form a second ray circularly polarized in the opposite direction to the former one, and having a greater rate of propagation than the original plane-polarized ray.

To obtain a mathematical representation of these results, suppose that we are looking from the origin along the axis of X with the axis of Y horizontally to the right and the axis of Z vertically upwards. The plane of XY may be taken as the plane of vibration of the incident ray, which will therefore be represented by the expression—

$$y = a \sin \frac{2\pi}{\lambda}(x - vt) = a \sin 2\pi\left(\frac{x}{\lambda} - \frac{t}{\mathrm{T}}\right) \quad . \quad . \ (37)$$

This is equivalent to two opposite circularly-polarized rays of equal wave-length determined by the equations—

$$y_1 = a \sin 2\pi\left(\frac{x}{\lambda} - \frac{t}{\mathrm{T}}\right),$$

$$z_1 = -a \cos 2\pi\left(\frac{x}{\lambda} - \frac{t}{\mathrm{T}}\right);$$

and

$$y_2 = a \sin 2\pi\left(\frac{x}{\lambda} - \frac{t}{\mathrm{T}}\right),$$

$$z_2 = a \cos 2\pi\left(\frac{x}{\lambda} - \frac{t}{\mathrm{T}}\right).$$

Owing to the change in the velocity of propagation, λ will be altered, and therefore also T if the medium is isotropic; so that it will be more accurate to put—

$$y_1 = a \sin 2\pi\left(\frac{x}{\lambda_1} - \frac{t}{\mathrm{T}_1}\right),$$

$$z_1 = -a \cos 2\pi\left(\frac{x}{\lambda_1} - \frac{t}{\mathrm{T}_1}\right);$$

and $\quad . \quad . \quad . \quad . \quad . \ (38)$

$$y_2 = a \sin 2\pi\left(\frac{x}{\lambda_2} - \frac{t}{\mathrm{T}_2}\right),$$

$$z_2 = a \cos 2\pi\left(\frac{x}{\lambda_2} - \frac{t}{\mathrm{T}_2}\right).$$

Since the stationary node of the ray remains unaffected, we shall have—

$$\frac{1}{\lambda_1} + \frac{1}{\lambda_2} = \frac{2}{\lambda},$$

$$\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \ (39)$$

$$\frac{1}{\mathrm{T}_1} + \frac{1}{\mathrm{T}_2} = \frac{2}{\mathrm{T}}.$$

[1] A circularly-polarized ray will therefore behave like a solenoid, and deflect a magnetic needle, affording an example of a direct action of light upon magnetism.

The electric current, consisting in the circular motion of one of the ether particles, will be of a special kind, as its velocity will be variable, the effect of which will be to increase its self-induction. Any one of these circles will be acted on by the other circles, which are indefinitely near to it, and the resulting attractive and repulsive forces will affect the elastic force of the ether which originally determined the vibrations, so that a current of variable velocity must be considered as equivalent to a series of distinct currents following each other in succession, and having their velocities determined by the corresponding accelerations. To investigate this action, consider the two rectilinear components (38) of each of the circular currents. The induction of any rectilinear element on the parallel elements can be neglected, as the actions on each side of any element will be equal in amount, so that it will only be necessary to consider the vertical component of the induction. This is proportional to the change in dz/dt—that is, to d^2z/dt^2. The elastic force on the point x in the direction $x + dx$ is therefore no longer of the form $\mathrm{P} \,.\, dy/dx$, but of the form $\mathrm{P} \,.\, dy/dx - \mathrm{B} \,.\, d^2z/dt^2$, and therefore the force in the opposite direction obtained by replacing x by $x - dx$ is—

$$\mathrm{P}\frac{dy}{dx} - \mathrm{P}\frac{d^2y}{dx^2} + \mathrm{B}\frac{d^2z}{dx^2} + \mathrm{B}\frac{d^3z}{dxdt^2}.$$

So that the differential equation of the light-vibrations will be—

$$\rho\frac{d^2y}{dt^2} = l\frac{d^2y}{dx^2} - \mathrm{B}\frac{d^3z}{dxdt^2}.$$

This equation will, however, be modified by the action of the molecules on the light-vibrations, and introducing the corresponding term from (6) (August 23, p. 405), we have—

$$\rho\frac{d^2y}{dt^2} = l\frac{d^2y}{dx^2} + 4\pi^2c_1(x_1 - y) - \frac{\mathrm{B}}{2\pi}\frac{d^3z}{dxdt^2} \quad . \quad . \ (40)$$

and similarly—

$$\rho\frac{d^2z}{dt^2} = l\frac{d^2z}{dx^2} + 4\pi^2c_1^1(x_1^1 - z) - \frac{\mathrm{B}}{2\pi}\frac{d^3y}{dxdt^2} \quad . \quad . \ (40a)$$

The action of the molecule is the same along both the axes, so we may put $c_1^1 = c_1$, and $x_1/y = \psi(\mathrm{T}) = x_1^1/z$, where $\psi(\mathrm{T})$ is a known function of T^2 determined from equation (4) (August 23, p. 405).

Both the above equations will be satisfied by the functions y_1 and z_1 of (38), provided—

$$\frac{\mathrm{T}_1^2}{\lambda_1^2} = \frac{\rho}{l} + \frac{c_1}{l}(\psi(\mathrm{T}_1) - 1) + \frac{\mathrm{B}}{l\lambda_1} \quad . \quad . \quad . \quad . \ (41)$$

And y_2 and z_2 will also be solutions of—

$$\frac{\mathrm{T}_2^2}{\lambda_2^2} = \frac{\rho}{l} + \frac{c_1}{l}(\psi(\mathrm{T}_2) - 1) - \frac{\mathrm{B}}{l\lambda_2}. \quad . \quad . \quad (42)$$

The quantities λ_1, λ_2, T_1, T_2, are determined by the four equations (39), (41), and (42).

The two waves (38) give together a new wave determined by the equations—

$$\eta = y_1 + y_2 = 2a \cos \phi \,.\, \sin 2\pi\left(\frac{x}{\lambda} - \frac{t}{\mathrm{T}}\right),$$

$$\quad . \quad . \ (43)$$

$$\zeta = z_1 + z_2 = -2a \sin \phi \,.\, \sin 2\pi\left(\frac{x}{\lambda} - \frac{t}{\mathrm{T}}\right),$$

where $$\phi = \pi\left(\frac{x}{\lambda_2} - \frac{x}{\lambda_1} - \frac{t}{\mathrm{T}_2} + \frac{t}{\mathrm{T}_1}\right).$$

This new vibration will take place in the plane—

$$\eta \sin \phi + \zeta \cos \phi = 0,$$

which makes an angle $-\phi$ with the plane of XY—that is, with the plane of the original vibration.

If, therefore, d be the length of the solenoid, the plane of vibration will be rotated in the positive direction through the angle—

$$\psi = \pi d\left(\frac{1}{\lambda_1} - \frac{1}{\lambda_2}\right) + \pi t\left(\frac{1}{\mathrm{T}_1} - \frac{1}{\mathrm{T}_2}\right).$$

As the plane is determined by the value of $\tan \phi$, this equation shows that as the time increases ψ will oscillate between certain fixed limits, the period of which being equal to $1/\mathrm{T}_1 - 1/\mathrm{T}_2$ will be too small to be observed. For all

practical purposes we may assume $T_1 = T_2$, which will give as the angle of rotation—

$$\psi = \pi d\left(\frac{1}{\lambda_1} - \frac{1}{\lambda_2}\right).$$

The assumption $T_1 = T_2$ is not in exact agreement with the foregoing equations for (41) and (42) give respectively—

$$\frac{T_2}{\lambda_1} = \frac{B}{2lT_1} + \sqrt{\mu_1^2 + \frac{B}{4l^2T_1^2}},$$

$$\frac{T_2}{\lambda_2} = \frac{B}{2lT_2} + \sqrt{\mu_2^2 + \frac{B^2}{4l^2T_2^2}}, \quad \ldots \ldots (45)$$

where μ_1 and μ_2 represent the indices of refraction of the medium for vibrations of the periods T_1 and T_2 respectively.

For $T_1 = T_2$, and therefore $\mu_1 = \mu_2$, it follows that—

$$\psi = \pi d\left(\frac{1}{\lambda_1} - \frac{1}{\lambda_2}\right) = \frac{\pi B d}{lT^2} \quad \ldots \ldots (46)$$

that is to say, the angle of rotation is inversely proportional to the square of the period of vibration, a result which is in approximate agreement with observation.

We may bring this result into still closer agreement with experiment by observing that B itself is a function of T. Now B determines the inductive action of the solenoid on the small circle, and must therefore be the differential of a potential, and therefore inversely proportional to the square of the velocity of light. The latter is, however, not constant in the medium, as it is the reciprocal of the index of refraction, and therefore a known function of T.

We may therefore put—

$$B = \mu^2 C, \; \psi = \pi \frac{Cd}{lT^2}\mu^2 \quad \ldots \ldots (47)$$

where C is a constant. The function μ^2 can be determined as in § 2 (August 23, p. 404). If we take Cauchy's approximate formula—

$$\mu^2 = a_0 + \frac{a_1}{T^2} + \frac{a_2}{T^4} + \ldots$$

we obtain an expression for ψ of exactly the same character as that which Boltzmann deduced from his experimental results.

The direction of the rotation depends upon the relative values of λ_1 and λ_2. For $T_1 = T_2$, (41) and (42) give $1/\lambda_1 > 1/\lambda_2$, and therefore $\psi > 0$. If, however, $T_1 > T_2$, and

$$\mu_i^2 = \frac{\rho}{l} + \frac{c_1}{l}\,(\psi(T_1) - 1),$$

then $\mu_1^2 < \mu_2^2$ for media of normal dispersive power, while for media of anomalous dispersive power $\mu_1^2 > \mu_2^2$, and *vice versâ* for $T_1 < T_2$. Now, it was found that the velocity of propagation, v_1, of the first wave was diminished, while that of the second wave, v_2, was increased, so that $v_2 > v_1$.

The equations (41) and (42) may be written—

$$\frac{1}{v_1^2} = \mu_1^2 + \frac{B}{l\lambda_1},$$

$$\frac{1}{v_2^2} = \mu_2^2 - \frac{B}{l\lambda_2}.$$

Therefore when B is small enough we shall always have $\mu_1 > \mu_2$, and therefore $T_1 > T_2$ in the case of anomalous dispersion, and $T_1 < T_2$ in the case of normal dispersion.

In the latter case—

$$\frac{1}{\lambda_1^2} = \frac{\mu_1^2}{T_1^2} > \frac{1}{\lambda_2^2} = \frac{\mu_2^2}{T_2^2}.$$

But in the former case it may be the reverse. The rotation of the plane of polarization will therefore be generally in the positive direction, being negative only in media of anomalous dispersive power, always supposing that B does not exceed a certain fixed limit. This result is confirmed by the observed fact that the lævo-rotatory substances, chloride of iron, and chromic acid, give anomalous dispersion. According to Kundt,[1] iron is dextro-rotatory in spite of its anomalous dispersion, which may be explained by a large value of B, and, as a matter of fact, the angle of rotation is exceptionally large. Maxwell's theory also led to the result that the angle of rotation is approximately inversely proportional to λ^2, but it gave no explanation of the decomposition of the ray into two circularly-polarized rays. Maxwell starts with the assumption of a circularly-polarized ray, and his λ appears to represent the wave-length of this ray, and is therefore different from the λ of the author. His theory rests on the assumption of the existence of molecular vortices, and therefore his differential equations are not the same as Lindemann's (40) and (40*a*).

It has been suggested that the electric current may possibly produce a structure in the medium, similar to that already existing in crystals which rotate the plane of polarization. Hitherto, however, this hypothesis has not been of much use in explaining the phenomenon, as no explanation has been given of the manner in which the ray is decomposed into two circularly-polarized rays such as occur, for example, in a crystal of quartz. The author endeavours, on the other hand, to explain the action of quartz on the hypothesis of an electro-magnetic effect of the ray of light passing through the crystal. There is, however, a difference between the two phenomena which has to be accounted for—namely, in the crystal the rotation is reversed when the direction of the ray is reversed, so that if the ray passes through the crystal, and returns along its own path, the total rotation is zero, while in the case of the solenoid the effect is to double the angle of rotation. The molecules of quartz must therefore be oppositely related to opposite directions, which seems to suggest that the arrangement may be due to the passage of the ray itself causing electric excitation in the quartz, and this is confirmed by the observed fact that quartz can be electrified by the action of light. Now suppose that the molecules of quartz are unequally susceptible to the electrical action of the ether in different directions, and suppose further that the molecules most sensitive to a ray in the direction of the axis are arranged in a spiral, having for its axis the principal axis of the crystal. Then a ray traversing the crystal in the direction of the axis will successively produce an electrical excitation at every point of this spiral, which will therefore act exactly as if the spiral were a conductor carrying a current. The effect on the ray will therefore be to decompose it into a pair of circularly-polarized rays differing in wave-length and in rate of propagation, and the plane of polarization will therefore be rotated. If the direction of the ray is reversed, the direction of the current in the spiral will be reversed, causing a rotation in the opposite direction.

§ 18. *Paramagnetism and Diamagnetism.*

According to the theory of the rotation of the plane of polarization which was developed in the last paragraph, an electric current traversing a medium excites small molecular currents, each one of which acts like a magnet. These currents as a whole cannot give rise to any magnetic action, since alternate currents flow in opposite directions, but this will not be the case if one set of currents is absorbed by the medium, and not the other, which will happen if one and only one of the wave-lengths λ_1 and λ_2, which together give the wave of length λ, is one of the critical wave-lengths for the molecules of the medium, while the other is not.

The existence of Amperian currents in magnets can be explained in a similar manner. Here the currents cannot be excited by the action of light, but it may be assumed that the molecules, even of rigid bodies, continuously perform small steady vibrations about their positions of equilibrium, and therefore come into collision with the neighbouring molecules on every side, thereby exciting the internal critical vibrations, which are visibly communicated to the ether when the substance attains the temperature of redness.

If such a substance is placed within a solenoid, the light-vibrations in the direction of its axis will remain unaffected, while the perpendicular ones will be decomposed into opposite circular vibrations; and the same thing will happen to the components perpendicular to the axis of vibration in any other direction, the components parallel to the axis remaining unchanged. If one of these currents is absorbed, and the remaining one is in the same direction as the current in the solenoid—that is, with a right-handed rotation—the substance will be paramagnetic, while if the rotation is left-handed the substance will be diamagnetic. In order that a sufficiently large number of vibrations should be absorbed, the substance must have a large number of critical periods, and therefore its spectrum must contain a large number of lines, a result which agrees with the fact that the spectrum of iron contains a larger number of lines than that of any other known element.

[1] *Sitzungsberichte der Berliner Akademie*, February 1888.

Whether a substance will be paramagnetic or diamagnetic will depend, in the first place, on the distribution of lines in its spectrum, and also upon the relative values of T_1 and T_2, calculated from equations (39) to (42), and therefore upon the other molecular constants which determine the relation between the wave-length and the period of vibration. The fact of the magnetic behaviour of a substance being partly determined by the values of these molecular constants would appear to make it impossible to predict its magnetic properties from the nature of its spectrum only. It is clear that, according to the author's theory, similar effects might be produced by mechanical vibrations, as by heat, for any excitation of the molecules to a sufficient extent must give rise to phenomena of the kind described.

The results obtained may be formulated in the following statement :—

If a body is traversed by an electric current in the positive direction, it will give rise to a series of pairs of oppositely-directed currents in the neighbourhood of any molecule, and each of these currents will be equivalent to a luminous vibration of definite wave-length. The body will be paramagnetic, when, in consequence of internal absorption, the excess of the right-handed current over the left-handed one is positive; and it will be diamagnetic if this excess is negative.

When a body is magnetized the internal energy of its molecules is increased, and therefore it will become heated—a result which is in agreement with observation. The magnetic saturation will increase as long as such an increase of internal energy can continue; but the limit of saturation will be attained when the molecular impacts have excited currents of the same kind as those which were absorbed, and of equal intensity.

The author explains permanent magnetism by assuming that the molecules of steel can have internal vibrations more easily excited by molecular impacts in some directions than in others. Suppose the sensitiveness to be very small in the direction of the axes of the molecules and very great in perpendicular directions, then vibrations perpendicular to the axis will be the most easily excited by magnetization, and will be transformed into circular vibrations. The planes of these circles will, in general, be inclined obliquely to the axis, and therefore every molecule will give rise to several circular vibrations in its neighbourhood, and the centres of these circles will lie upon a straight line nearly coinciding with the axis of the molecule. Every such circular vibration will be equivalent to a small magnet with its axis perpendicular to the plane of the circle, and the poles of these magnets lie in a straight line. There is therefore a molecular rotation in the direction indicated by Weber's theory. Again, when the axes of all the molecules have become parallel, there will be more frequent collisions between neighbouring molecules in a direction perpendicular to the axes than in the direction parallel to them. The critical relations will therefore be excited and communicated to the ether, giving rise to circular currents, which will again be partially absorbed. The magnetization will be permanent when the mutual action of the molecules and the ether is a steady one. This stationary motion requires a certain supply of external energy, which is continuously transformed into small vibrations of the molecules about their positions of equilibrium. It is an experimental fact that when this supply of energy is considerably diminished by cooling the magnet to a sufficient extent, the magnetism is greatly weakened.

According to this theory, the amount of light absorbed will be equal to the amount emitted, but the latter will have a different vibration-period from the former, since T may be different from T_1 and T_2, and the critical vibrations most easily excited by the molecular collisions will in general be of different period from those which are most easily absorbed. Therefore rapidly succeeding molecular impacts may give rise to luminous vibrations, the periods of which may be different from those proper to the molecules.

§ 19. *Lorenz's and Maxwell's Electro-dynamic Theories of Light.*

The author observes that he has preferred to base the explanation of electrical phenomena upon those of optics rather than the reverse, because optical phenomena are much more completely understood than electrical phenomena. Lorenz and Maxwell both endeavoured to explain optical by means of electrical phenomena. Lorenz[1] bases his speculations upon the resemblance between the differential equations of the motion of electricity and those which represent vibrations of the ether, which can be made identical by the introduction of certain very small terms. His theory has not been sufficiently developed to admit of its application to the discussion of any definite problem. He comes to the general conclusion that the motions of light consist in electric currents, and that the latter consist essentially in rotatory vibrations of the ether about certain axes. In this point Lorenz's theory presents a certain similarity with that of the author, but in the former no distinction is assumed between the magnitude of electrical and luminous vibrations respectively.

[1] *Poggendorff's Annalen*, vol. cii. 1856.

Maxwell has developed his theory to a much greater extent. He, too, starts from the similarity in the differential equations, which are different from those of Lorenz, and also from the author's. Magnetic and electrical actions at a distance are attributed to the energy of an intervening medium, and explained by the assumption of the existence of a strain in this medium. The assumption of the identity of the electrically excitable intervening medium with the luminiferous ether receives strong confirmation from the fact that the ratio of the electro-magnetic to the electro-static unit of quantity is the same as the velocity of light. Maxwell arrives at the result that electrical as well as luminous vibrations are entirely transverse to the direction of propagation, but he does not obtain any further analogy between electrical and optical phenomena, and his explanation of the electro-magnetic rotation of the plane of polarization involves a series of complicated hypotheses respecting the action between matter and ether. Maxwell expressly excludes the consideration of molecular structure, and supplies its place by the hypothesis of molecular vortices.[1] A further important difference between the two theories is that while the author assumes that the material molecules suck up energy from the ether, Maxwell deduces the repulsive actions between two similarly charged conductors from an accumulation of electricity in the intervening medium, especially in the case of an optical excitation of the medium. He does not appear to have arrived at any definite distinction between electrical and luminous energy.

In the preceding investigations the molecules have always been assumed to be of the same size. If there should be any great difference in the sizes of molecules in the case of different substances, then the difference between optical and electrical phenomena would be entirely relative to the size of the molecules of the body considered, so that an ethereal vibration which would give rise to electrical excitation in one body might produce only optical effects in another. The different behaviour of different substances with regard to light and electricity may perhaps depend partly upon this condition as well as on the values of the critical periods and other molecular constants. An interesting question which arises is, What would be the effect of ether vibrations neither very large nor very small in comparison with the size of a molecule? The author has not suceeded in obtaining any definite answer to this question.

§ 20. *Concluding Observations.*

In concluding the paper the author observes that the only hypothesis which he has made use of is that space is filled with a continuous elastic medium—namely, luminiferous ether, the density of which is so small that it may be neglected in comparison with that of matter. The existence of this is sufficiently established from the known phenomena of light.

It is not found necessary to assume a difference in the elasticity of the ether in crystals in different directions, the existence of a special force of chemical affinity, of electric or magnetic fluids, or of molecular vortices.

Thomson's assumption with respect to the constitution of molecules and their relation to the ether explains the most diverse phenomena of physics and chemistry from a single standpoint—namely, the transference of energy between the molecules and the ether, in obedience at every stage to the law of conservation of energy.

The author then suggests that the theory may possibly provide a means of escape from the conclusion, known as the "Dissipation

[1] Maxwell's "Electricity and Magnetism," Arts. 111, 645, 794, 830, 832. In Art. 111 he says: "I have not been able to make the next step—namely, to account by mechanical considerations for these stresses in the dielectric." In Art. 806 the analogy which the author deduces between a solenoid and a circularly-polarized ray is characterized as faulty on the ground that two opposite circularly polarized rays do not neutralize each other, but produce a plane-polarized ray. The author points out, however, that it is only necessary that the electrical actions should neutralize one another. The simple relation deduced by Maxwell between the specific inductive capacity of a medium and its index of refraction does not follow from the author's hypothesis, and this relation has been shown to be only very roughly true.

of Energy," that the total energy of the universe will ultimately assume the form of uniformly-diffused heat of low temperature.

The attractions between the heavenly bodies must upon this theory be ascribed to their being electrically excited to different extents, and continually sucking up electrical energy from the ether. When, then, any one of them loses heat by radiation, it will take up electrical energy which may be transformed within it into other forms. The sun may thus receive compensation for the light and heat which it emits. In this way it seems quite possible that the universe may really be a conservative system. Indeed, the sun may receive a direct accession of light and heat from the electrical energy diffused throughout space, as this would take place if it receded from some other star with a velocity exceeding by a finite amount the velocity of light. This accession would take place when the relative velocity exceeded a certain value, and its effect would be to diminish this relative velocity until the accession of light or heat ceased, when the velocity would again increase, as in the phenomena of the vacuum tube.

The author considers that this might explain many hitherto unexplained changes going on in the sun, especially as it would necessarily involve the inequality in the intervals from maximum to minimum and from minimum to maximum, which is actually observed. It might also give an explanation of the phenomena of variable stars, as seems suggested by Secchi's observation that all red stars are variable.

The author states that he makes these suggestions with diffidence, as speculation upon cosmical phenomena based upon the limited data at our disposal is apt to be misleading; witness, for example, the limitation to which Weber's law was found to be subject.

He points out that, if Newton's law of gravitation be considered only as a first approximation to the law of attraction between the electrified bodies of the universe, then every case of gravitational attraction, including the weight of terrestrial substances, may be considered as due to electrification. The molecular attractive forces may also be due to the same cause. The differences in the electrical excitation of the molecules of various substances would then play an important part in the phenomena of chemical combination (see footnote to § 16).

The rigidity of a body would then be determined by the differences in the electrification of its molecules. These differences would naturally be determined by external circumstances, and would be greatest in the direction of the normals to the surface.[1]

G. W. DE TUNZELMANN.

[1] The foregoing paper in the original form is itself a very condensed abstract of an extensive research, the author only having a limited amount of space placed at his disposal in the journal in which it was published. This may account for the reasoning, in some parts of the paper, appearing somewhat general and difficult to follow.—G. W. DE T.

LEARNED SOCIETIES IN RUSSIA.

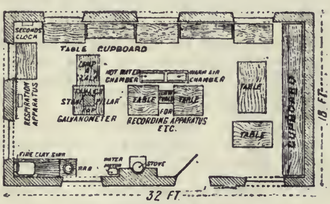
At a recent meeting of the French Geographical Society, M. M. Venukoff read a short paper on the learned Societies of Russia. Besides the Geographical Society, the Army Staff, the Academy of Sciences, and other Government institutions, there are in Russia several learned bodies engaged in the exploration of those countries which are still but little known. Though many of the explorers do not go for geographical purposes properly so called, yet these non-geographical explorers frequently obtain results of the greatest interest to geography. M. M. Venukoff is a member of many of these Societies, and at the outset of his paper he proceeds to name some of his colleagues who have in recent years rendered great service to geography; amongst the members of the Naturalist Society of St. Petersburg, MM. Korotneff, Nicolsky, Lidsky, Yaschenko, and Kouznetoff. The first-named has travelled in the Malay Archipelago, where he has studied chiefly the invertebrate animals, but has at the same time made scientific observations of every kind. In the month of June 1887, he visited the country around Krakatão, where already several little hamlets have sprung up on the site of the town of Anjer, which was destroyed by an earthquake in 1883. These poor huts were surrounded by a luxurious vegetation, while the neighbouring portions of the sea were still covered with pumice-stone and altogether deserted by fish. At Billiton Island the traveller met the interesting tribe of Secasses, the fishermen of their state, who, with rare exceptions, inhabit floating-houses—that is, their junks—and even those among them who possess huts build them on the sea on piles, and never on *terra firma*. They are distinguishable from the Malays by their tall figure, their curly hair, and projecting cheek-bones; finally, strange to say, they almost all stammer. They are a very honest race, gentle, kind, joyous, and hospitable, and it is said that robbery is unknown among them. M. Korotneff describes the tides of the Sunda Sea, which are very complicated, and several other interesting phenomena. M. Venukoff then passes to M. Nicolsky, a famous Russian zoologist, who has pursued his researches in Lake Balkash. He assigns as the cause for the remarkable difference between the fish faunæ of the two districts of Tchui and Ele that the basin of Lake Balkash is separated from the Tchui valley by plateaux and mountains of a very ancient formation. Besides, Balkash is 280 metres above the sea-level, the Sea of Aral is scarcely 50 metres, and the height of the plateaux between Balkash and Tchui is 370 metres at least, and so it is difficult to see how the two great lakes were formerly part of one sea. Balkash, Sassyk-Kul, Ala-Kul, and even Ebi-Nor probably formed, and within the modern epochs, a single vast basin of fresh or slightly brackish water, for their fish fauna is identical with that of our days. In spite of its great extent and its latitude, which is the same as that of Bordeaux and Venice, Lake Balkash freezes every year from the month of November up to the middle of April, and the ice sometimes is as thick as 80 centimetres. A fact worthy of observation is that the steppes which surround the lake vary very much according to their position. Those on the north-west are clayey, and completely bare during the summer, and covered with pools in the spring; those on the south-east are formed of beds of sand, in which there are no pools, but where water is to be found below a certain depth. Thus the desert in the latter case is not so dry as it is to the north and to the west. From the point of view of a zoologist, M. Nicolsky finds that the north and west of Lake Balkash are marked by the presence of jerboas and of larks, whilst at the south of the lake there are numerous reptiles and tortoises; some hares and mice dwell there also, but there are no birds. M. Venukoff does not follow M. Nicolsky into the remainder of his report, as it deals chiefly with the natural sciences; but he remarks that M. Nicolsky shows all the qualities of Humboldt and Mr. Wallace —abundance of well-established facts, and great breadth of view in explaining them. M. Lidsky travelled in Karateghin and in part of Bokhara. Having arrived in the month of June at Schahrisiabz, M. Lidsky wished to journey to Hissar by the Sangardak Hill, but this being prevented by the snows, he was forced to make a detour and enter the valley of the Sourkhan by another route. From this vast prairies stretch away as far as the Oxus, inhabited not by men, but by jackals, for the waters of the Sourkhan flood the plain each year. In rising from this valley, he soon arrived at Garma, and then at Karatag, the summer residence of the Bey of Garma, which is usually hidden from the heat and the fevers which prevail in Garma in the hot season. There, and at Fezabad, M. Lidsky saw fish the skin of which was of exactly the same shade as the water which holds them, and which abounds in clayey soils—that is, of a red colour. Beyond Fezabad the traveller pushed into the high valley of Dachti-Bidona, which is really a plateau separating the basin of the Sourkhâb from that of the Kiafirningan. M. Lidsky describes Karateghin, which is 150 kilometres in length and 50 in breadth, as a fertile country in its lower parts, and thickly covered with forests in the mountainous regions. Unfortunately this oasis is separated from all the neighbouring countries by high peaks, so that the journey from Garma to Samarkand, for example, passes over Mount Pakchif, which is at least 3850 metres above the sea-level. The cold is so great at the top of the mountain that beasts of burden and even men are frequently overcome by it; travellers are often compelled to throw before them long strips of felt, on which they walk—a singular and a very slow and painful mode of progression. In 1877, M. Yaschenko made a journey in Russian Lapland, between Kola and Kandalaschka. According to him the lakes of this region belong to the basin of the White Sea or to that of the Arctic Ocean, and have identical fauna; but the terrestrial animals are not everywhere the same. There are places where bears abound; there are others where the principal enemy of man is the glutton. In latter years the inhabitants have remarked that the reindeer are changing their habits, and are beginning to prefer the forests to the *tundras*, or spaces covered with lichens, which make their favourite food. The reason of this change is to seek a more favourable shelter from the hunters; in the open, whole herds may be taken, but in the forest it is only possible to hunt one or two at a time.

M. Kouznétoff has pursued zoological and physical geography researches on the Sea of Azov. This little basin, of which the length does not exceed 350 kilometres, and its breadth 170 kilometres, and its depth scarcely 14 metres, abounds in fish, and attracts continually to its shores crowds of fishermen. Its water is brackish rather than salt, for its percentage of salt is only 1·19, while that of the Black Sea is 1·75, and the Mediterranean more than 2·3 per cent.; and consequently the real sea-fish are not to be found in the Sea of Azov. *Gourmets*, however, would find that the sturgeon is very numerous here, and has delicious flesh. We can see by this short account that the study of geography is making great strides in Russia. Three years ago, General Tillo, in drawing up his magnetic charts of Eastern Europe, discovered certain anomalies in the distribution of the magnetic elements around Koursk and Kharkov. During the summer of 1887, M. Piltchikoff, Professor at Kharkov, made inquiries into these anomalies, and he has just published a book in which the theory of terrestrial magnetism started by Gauss is developed.

RESEARCH LABORATORY OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.

FOR some years the question of equipping a research laboratory occupied a very prominent position in the discussions of the Royal College of Physicians, Edinburgh, and last year the Committee appointed by the College was able to throw the plans into a feasible and at the same time thoroughly acceptable shape. Within a very short time suitable premises were acquired, the necessary structural alterations were at once commenced, a Superintendent was appointed, and apparatus was ordered and fittings were put in hand to be ready for use as soon as the building should be prepared for their reception. The premises are well adapted for the purpose for which they were acquired. They consist of a three-storied house, No. 7 Lauriston Lane, near the Royal Infirmary, to which had been added a large detached room in the back court. There are also commodious out-houses and a plot of ground of considerable size at the rear of the building.

The room in the back court is set apart for experimental physiology. It is 32 feet in length, 18 feet in breadth, and 14 feet high, and is well lighted by seven windows, three of which, facing to the west, are fitted with tables for microscopic work.



EXPERIMENTAL ROOM

FIG. 1.

Near the south end of this room is a stone pillar bedded in the ground, so arranged as not to be affected by movements in the room. (There being no thoroughfare in the lane, no disturbance can arise from wheel traffic.) Around it is fixed a table to which the galvanometer wires are attached. The galvanometer is placed on the stone pillar in a glass case with a hinged door, and is always kept ready for use, short wires being carried from the table to the instrument. A hinged lamp table and brass rods over which curtains are hung complete the galvanometer fittings. Work-tables occupy the remainder of the centre of the room. Electrical, time-marking, and other apparatus, tuning-forks, perfusion apparatus, shunts, compensators, constitute the greater part of the instruments in this room. The sink and drainage apparatus in the room may be taken as a type of those throughout the whole house. It consists of a large earthenware sink, on one side of which is a grooved draining-board covered with lead, the grooves all leading to the sink. A swan neck tap supplies the water; to this tap are two nozzles, to one of which is wired a piece of india-rubber tubing, used to connect the Geissler exhaust-pump, and similar apparatus; the other nozzle gives a steady unbroken jet of water three-eighths of an inch in diameter. The wall behind the sink is leaded for about 3 feet up; at the upper part of this are a couple of shelves, the upper one perforated, for draining flasks and bottles, the lower one grooved, and with a gentle slope to carry all moisture to the sink. Below these shelves are a couple of rows of wooden pegs fixed into the wall at an angle of 45. These are very useful for draining all kinds of glass vessels. In the main building in the lower flat is a large entrance lobby, to the left of which is a part of the laboratory assistant's quarters.

A large room on the first floor, set apart for Committee meetings, is used as a library and museum. On the second floor is the chemical room, fitted with a good supply of water and gas. On the top floor are three splendidly-lighted rooms, all of which are devoted to microscopic work. In the south room the apparatus necessary for bacteriological research is collected. Two large projecting roof or dormer windows face east and west respectively. Each is fitted with a table covered with a sheet of plate-glass, on the under surface of which are painted three strips, the first, 4 inches broad, black; then a similar white band, and then a

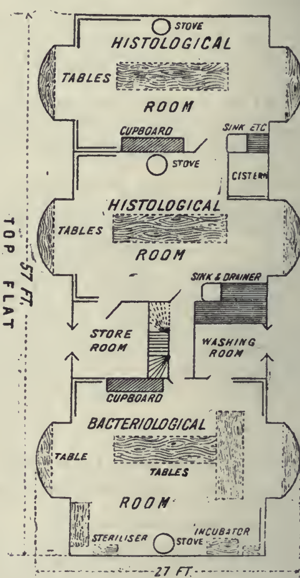


FIG. 2.

broad black band extending to the back of the table. On each side are shelves from the floor for about 5 feet up. These are within reach of anyone sitting at the table. On each side is a drawer $3\frac{1}{2}$ inches deep; but the remainder of the space under the table is left quite open, in order that earthenware jars for the reception of chemicals, washings, and *débris* may be accommodated. On the left side is fitted a rack for test-tubes, and in front and to the right are stands for ordinary histological reagents. Above the level of the table in front are four small shelves, on which are placed covered vessels for clean and dirty slides and cover-glasses. A syphon arrangement for distilled water, a bell-jar with counterpoise running on a brass rod, a Bunsen burner, and a lamp, complete the fittings at this table. One of Browning's microspectroscopes has also been fitted up in this room. Racks, for series of Hesse's tubes, and shelving complete the fittings here; but opening out from it is a small room with a sink and large sloping drainer, at which most of the glass apparatus is washed. The other two rooms in this flat are fitted up for histological work, with window tables, sinks, cupboards, spirit vessels, and shelving, each for two workers. In connection with the histological department, apparatus for micro-photography has been fitted up by Mr. Forgan.

The arrangements for conducting the work are somewhat as follows:—The College has established and will maintain the laboratory for the prosecution of original research. To facilitate such work the Council of the College "appoint a scientific Superintendent, who must devote such portion of his time as may be determined by the Council to the work of the laboratory, where, under the supervision of the Curator and Committee, he shall himself undertake the prosecution of original research, and be prepared to assist, if required to do so, in the work of other investigators. Under like supervision, he shall also be prepared to furnish the Fellows of the College with reports upon such matters as the histology of morbid specimens, and of the chemical and microscopic characters of urines," in which work he is assisted by the resident assistant.

The laboratory is open without fee to Fellows and Members of the College, "to any Licentiate who shall obtain the sanction of the Curator and Committee to use the laboratory for the purpose of scientific research," and "to any medical man or investigator who shall obtain the sanction of the Council of the College, as well as of the Curator and Committee, to use the laboratory for the purpose of scientific research."

The whole of the expense of establishment and maintenance has been and will be defrayed from funds placed at the disposal of the Committee by the Council of the College. Of this, an initial grant of £1000 was made with which to adapt and furnish the house, and buy apparatus, instruments, and chemicals. In addition to this, an annual grant of £650 is made, from which all salaries, rent, and taxes are paid, and stock is kept up. Of these sums, only about £830 of the original £1000, and £600 of the annual grant, were spent during the first twelve months, so that the whole equipment and fittings of the laboratory, together with the current expenses during that period, cost only £1430.

CYCLONES AND CURRENTS.

MR. S. R. ELSON, an experienced pilot of the Hooghly Pilot Service, and author of "The Sailor's East Indian Sky Interpreter," writes as follows with reference to the article on the incurvature of the winds in cyclones, published in NATURE, vol. xxxviii. p. 181:—

So deeply is [the] "old and exploded error of facts," the eight-point theory of storms, rooted in the minds of some, that, ignoring the reiterated warning voice of science, they will have none other. Do they lean towards it because it is so very simple to look at on paper, and so easy of application? I fear that is about the truth of it. So very easy, that Piddington, somewhere in his writings, says of a certain old salt whose ship had been dismasted in a cyclone, that if even a junior P. and O. Company's midshipman had had the handling of his vessel, she would have come through the storm scatheless (the P. and O. midshipman, it must be presumed, having been schooled in Piddington's theory)—a reflection which we, with our more extended knowledge, now perceive was very hard on the old experienced captain. Yet there is the proclaimed peril of using this theory staring mariners sternly in the face.

But there is one more cogent element of trouble and danger besetting the anxious mariner, which, although taken note of in Mr. Pedler's recent Report on the Meteorology of the Bay, is not generally considered when judging, as Piddington used to do, of a shipmaster's proper or improper management of his vessel in a cyclone, and which will probably account for the numbers of vessels, perhaps widely separated before the cyclone came on, which unaccountably get foul of the comparatively small space called the "eye of the storm" as it progresses on its fell course, and so have to bear the brunt of the dreaded rear hurricane wind from south-west or west—that is, the great indraught towards the very centre of the waters in which they float.

This whirling indraught, drift, or set of the sea is on the move long before even the air motion above has gained force enough to impel it, as is so well shown by the westward set at the Hooghly Pilot Station, which usually gets up some time before every cyclone in the Bay, whether far or near. But the worst of it is, when the vessel is out of sight of any fixed object, or the skies are overcast so as to preclude sights being taken, the force and direction of this inset cannot be calculated and allowed for in the dead reckoning as a "course and distance." And it is only after the gale is over, and a sight can be taken, that the captain is very much astonished to find his vessel's position is so far out of her dead reckoning.

I myself, as a pilot, have experienced this perplexity on more than one occasion at the head of the Bay; and, besides, the published records and logs of vessels involved in these storms show this whirling inset of the sea most conclusively.

Mr. Blanford's rules for finding the bearing of the centre of storms are evidently calculated to suit all winds; but some account should be taken of the fact that, in and off the Hooghly River at least, whether the cyclone is passing up to the eastward towards Chittagong, coming straight on towards the Hooghly, or passing across the Bay to the westward towards False Point, or Balasore, the first wind blows invariably from north-east until the hard part of the storm is close upon you. No special reason has yet been advanced as to why this should be the case; yet so it undoubtedly is, as was noticed first by the late Mr. Wilson concerning a cyclone some years back, and as the meteorological registers and logs of ships during later storms well show, and which, years ago, I drew attention to in my little book, "The Sailor's East Indian Sky Interpreter."

Some authorities of the present day advise, when caught in a cyclone, that vessels should run with the wind more or less on the starboard quarter in the northern hemisphere; but, taking into consideration the now generally acknowledged wind's incurvature, and the great inset of the sea which I have drawn attention to above, there is no safety but with the wind on the starboard beam; always provided, of course, that circumstances of smooth water and sea-room allow of it. As a decisive proof of the advisability of this plan, I may mention that I was in pilotage charge of an inward-bound sailing-ship on the immediate advent of, and during, the Midnapur cyclone of June–July 1872, in which my brother, also a pilot, lost his life, on the foundering of his storm-battered ship, the *Rothsey*, in Balasore Bay. Starting from the Pilot's Ridge on the morning of June 27, under close-reefed topsails and with squared-in yards, we stood away on a south-south-east course, with a hard west-south-west gale blowing (wind on starboard beam), for thirty-six hours, and by so doing raising the rapidly-falling barometer from 29·30 to 29·50 inches, and, as I expected, getting into more moderate weather.

"Look to leeward for the weather," is the old Dutch sailor's advice, and doubtless there is a power of wisdom in the old saw, which seems to chime in better with the modern theory of eleven to twelve points rather than with the old eight-point theory. And, whilst thanking Mr. Blanford for his latest valuable contribution on marine meteorology, as set forth in his letter above alluded to, and looking forward to his promised forthcoming work on the weather and climates of India, I would point out that his directions about finding the bearings of the centre of cyclones of the Bay of Bengal seem to be just a little perplexing to some who read them, when he speaks, as he does, of the wind being three and four points before the beam, while referring to a human being standing with his back to the wind, &c. Of course, what is meant is, supposing a vessel has her stern to the wind, or running with the wind right aft, the centre will be three and four points before the "port" beam; or, in other words, if the wind is, say, north-east by north, the centre of the storm will bear south-south-east or south by east, and not south-east by east, or south-east, as it appears is still stubbornly taught by those who should know better.

A vessel in the northern hemisphere on the starboard tack, unless she happens to be sailing on the same course as the storm, and slower than it is travelling, is invariably going out of bad weather into finer, and out of bad into worse weather when on the port tack.

But much has to be said with regard to this rule of keeping the wind on the starboard beam, with a view of hastening the vessel's distance from the centre and from the hurricane belt of a cyclone. In the first place, on the left-hand semicircle, each squall, as we have above noticed, bursting down from aloft, comes from the right hand of the surface wind, which it displaces, and the vessel necessarily comes up in it, provided the storm is stationary, or is not fully developed; but if it has obtained much velocity, its onward progress will counteract this effect, and the wind will remain stationary in direction, or the ship will actually "break off," and, consequently, be more and more in the "trough of the sea"—a position sometimes critical for a ship if she is deep laden, and a high cross-sea is running, as there probably will be under the circumstances. In this case the only alternative left open to the shipmaster is to so reduce

his sail that the vessel will not forereach (or lay to) on the port tack, and wait until the storm passes on.

But on the right-hand semicircle the case is very different, and the starboard-tack rule is the proper one to adopt both with regard to the wind-shifts and also to the fact of the vessel always coming more and more "head on" to the sea—an all-important consideration.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. Francis Darwin, F.R.S., of Trinity College, has been appointed Reader in Botany in succession to Dr. Vines. Mr. E. H. Douty, M.A., of King's College, has been appointed Senior Demonstrator in Anatomy; and Messrs. W. S. Melsome, Fellow of Queen's College, and Mr. R. W. Michell, of Gonville and Caius College, Junior Demonstrators of the same.

The elections to the Council of the Senate this year may be regarded as generally favourable to science; Dr. Peile, Prof. Macalister, Dr. Routh, Prof. Browne, and Mr. E. Hill, being six of the eight elected. Dr. Lea, however, was unsuccessful, this being his first candidature.

SCIENTIFIC SERIALS.

American Journal of Science, November.—On the deflection of the plumb-line and variations of gravity in the Hawaiian Islands, by E. D. Preston. The observations for gravity were carried out in 1887 on Mount Haleakala on the Island of Maui, which is rather over 10,000 feet high with one of the largest extinct craters in the world on its summit. From these researches it appears that deflections of the plumb-line are greater on insular than on continental mountains, presumably owing to the lighter surrounding sea-water; that gravity is not in defect, because it is here estimated from the true sea-level, and not from a sea-level elevated by continental attraction; that deflections are greater in the vicinity of extinct volcanoes than near active ones; and that the so-called "hidden causes," which in the Himalayas give a variation of gravity several times as great as those arising from the attraction of the mountains themselves, do not exist in the Hawaiian Islands.—Mineralogical notes, by S. L. Penfield and E. S. Sperry. Beryl and phenacite are here studied for the purpose of determining the presence of alkalies in these crystalline bodies. Analytical studies are also given of several other rare minerals, such as a specimen of monazite and oligoclase from North Carolina, sussexite from New Jersey, barium feldspar from Pennsylvania.—The absorption spectra of certain blue solutions, Part 2, by F. B. Pitcher. Here it is shown that blues and violets obtained by absorption in pigments and solutions, differ in several respects from those colours which approximate in hue to the longer wave-lengths of the spectrum. As a rule they are much less completely saturated, and they show irregularities of composition rarely met with in absorption reds and yellows.—An instrument for demonstrating the laws of transverse vibrations of cords and wires, by George S. Moler. The apparatus here described was designed to meet a want, felt in the laboratory, for an improvement over Melde's method of producing transverse vibrations of cords and wires.—Rhætic plants from Honduras, by J. S. Newbury. These fossils, chiefly from the San Juancito district, are clearly Upper Triassic, and greatly resemble those of the coal-bearing strata on the Yaki River, Sonora.—Energy and vision, by S. P. Langley. In this investigation the author has had mainly in view the assumption of H. F. Weber and others that the luminosity of a colour is proportionate to the energy that produces it, an assumption which is shown to be absolutely groundless.—Mr. J. H. Long has a paper on circular polarization of certain tartrate solutions, and Mr. W. E. Hidden sends some notes on some specimens of xenotime from New York and North Carolina.

Bulletins de la Société d'Anthropologie, tome xi. Série 3 fasc. 1 (Paris, 1888).—On aphasia, by M. Hervé, who draws attention to a case recorded by Larrey sixty years ago, of a soldier, wounded at Waterloo on the left frontal, who lost his memory of words, more especially nouns. After death the ball was found close to the dura mater, but separated from it by the portion of bone embedded with it at the moment of the accident. The case is curious as having been recorded so long before Broca's discovery of the localization of speech.—Monstrosity of the left upper extremity, by M. Variot. The relatively small but otherwise normally formed left hand appears to proceed directly from the stump of the flattened shoulder with no trace of arm, or forearm. The body presents no other anomaly.—The history of the various modifications effected in the ship's rudder, by M. O. Beauregard.—On certain customs, connected with phallic worship, common to the Abyssinians and the ancient Spartans.—On cannibalism and its assumed origin. The consideration of these questions at an earlier meeting by M. de Nadaillac has been again made the subject of an animated discussion between himself and M. Mortillet; for, while the latter believes that this practice must originally have emanated from some perverted religious idea, M. de Nadaillac refers it solely to the promptings of famine, which is capable of engendering in man, if not mania, a depraved taste, and bestial inclinations, which civilization has never been able wholly to eradicate. The absence of animals adapted for human food he considers to have been a powerful factor in widely remote lands, as Mexico, Tierra del Fuego, New Zealand, the Pacific Islands, &c., where the people under various stages of civilization and barbarism have alike practised cannibalism, whether as a national rite or a social custom. The discussion supplies an exhaustive treatise on the subject, which at a subsequent meeting of the Society was again considered at great length by Dr. Bordier, who concludes his comprehensive essay by showing that, as the dental system in man, as in the other Primates, does not allow us to assume that in his primitive condition he was carnivorous, we must consider cannibalism as an acquired and not an original custom.—Communication, by M. D'Acy, regarding Palæolithic mortuary deposits in rock-caves. This paper gave rise to a discussion as to the age of human remains found at Solutré, Furfooz, Spy, Mentone, &c., M. de Mortillet regarding them in opposition to M. D'Acy as Neolithic, rather than Palæolithic.—On the choice of a fixed point of departure for cranial measurements, by Dr. Fauvelle. This the writer considers is to be sought at the base of the cranium, at the cerebral extremity of the vertebral column, where alone one definite point can be found which is always the same in the entire series of the Vertebrata, being indicated in the embryo by the anterior terminus of the dorsal cord, and in the adult by the posterior portion of the first cervical nerve.—The present number of the *Bulletins* contains the ordinary annual report of the statutes, rules, &c., of the Society.

Fasc. 2.—Continuation of the discussion on cannibalism reported in the previous number, and treating specially of the character and adaptability of the dental system in man.—On woman in relation to cannibalism in Polynesia, by M. Letourneau. The exclusion of women from cannibal feasts in some members of this group is referred to a greedy desire on the part of the chiefs to reserve such enjoyments for themselves. Human flesh being early tabooed to women, they gradually acquired a strong distaste for it, which in course of time was transmitted as an hereditary characteristic even to their male descendants, some of whom, as the majority of the Tahitians, had begun to manifest a repugnance for this species of food as early as the time the islands were first visited by Captain Cook.—On the ethnology of Le Rouergne, by M. Durand de Gros. The author regards this district as chiefly Iberian in character, and considered that the whole of the Department of Aveyron, with L'Hérault and La Lozère, forms the eastern confines of a remarkable linguistic region, comprising the whole of ancient Aquitania. He points out that the Garonne is a phonetic frontier, to the north of which all forms of local *patois* possess the letter *f*, while on the opposite side that character is replaced by *h*, the *filha*, *ferre* (*fille*, *fer*), of the peasants on the right bank, being pronounced *hilha*, *herre*, by those on the left. The paper supplies much interesting matter in regard to the various linguistic currents that have been successively incorporated in the main stream of the vernacular through consecutive immigrations; Latin, Celtic and Teutonic suffixes being often associated with some alien root in the names of families and places. The brachycephalic character of the district is at present very strongly marked, while the crania belonging to ancient times, as those found in the dolmens of La Lozère, are without exception dolichocephalic.—On the stature of the Parisians, by M. Manouvrier. A comparative analysis of the results yielded for the twenty arrondissements of Paris shows that, other conditions being equal, affluence, and the absence of want and of the necessity for excessive labour, have a favourable influence on the stature

of a man. It is found, moreover, that while the mean height of Frenchmen belonging to families in easy circumstances does not notably alter, it is being very sensibly diminished among the poor.—On the skull of an adult gorilla, by M. Hervé.—On prehistoric discoveries in Portugal, by M. de Mortillet, who reports the recent inauguration at Lisbon of a course of lectures on archæology by M. da Silva, to whom we are indebted for the discovery of a deposit near Leiria, in which flat hatchets and other instruments have been found, all of which are of pure copper.—The Neolithic Age at Champigny, on the Seine, by M. E. Rivière. The finds at this station have been rich in flint and other stone instruments, with fragments of coarse pottery, but they contain few bone remains, these belonging moreover, with the exception of the elk, to ordinary domestic animals. No human bones have been obtained.—On certain anthropological researches in the Caucasus, by M. E. Chantre. This communication supplies an interesting summary of the author's important work on the anthropology of the Caucasian district, which is based on the result of personal observations, and a careful study of the human and other remains derived from numerous ancient tribal burial-places, and is copiously provided with tables of comparative cephalic and other anthropometric determinations.—On a prehistoric station at Aragua, Venezuela, by M. Marcano.—A prehistoric necropolis at Saint Ellier (Maine-et-Loire) by M. Bonnemère.—On the mammillated menhirs of Sardinia, by M. de Mortillet.—On aphasia, by M. Ploix. This paper, which is mainly based on the deductions of Broca, gave rise to repeated discussions, in which Dr. Fauvelle and others took an active part in defending their special views as to the localization of speech.—Communication, by M. Hervé, on his memoir entitled "Broca's Convolution in the Primates." The writer demonstrates the claims of Leuret to be regarded as the first who recognized in the brain of the Simiadæ the prototype of the convolutions of the human brain, his discovery of a cerebral type common to all the representatives of the group of Primates having preceded by thirty years the researches of Darwin, Huxley, Vogt, and Broca.—On the efficacy of the poisons used in olden times in Europe, and still employed by Negritos and others, for tipping arrows and other weapons, by M. Laborde.—On a case of congenital blindness and deafness, with mutism, reported in New York, and communicated by M. de Nadaillac.—On cannibalism in Madagascar, as recorded in the work of M. de Flacourt in 1650, by M. Beauregard.

SOCIETIES AND ACADEMIES.

London.

Linnean Society, November 1.—Mr. W. Carruthers, F.R.S., President, in the chair.—Prof. Bower exhibited and made remarks upon some adventitious buds on a leaf of *Gnetium gnemon*.—Mr. John Young exhibited (1) a rare bird (*Pluvianellus sociabilis*), unobserved for fifty years, and lately rediscovered by him in Patagonia; (2) a cluster of nests formed of lichen (*Usnea*) by a swift, as supposed of the genus *Collocalia*, from a cave in Eimeo, one of the Society Islands; (3) remarkably elongated tail feathers of domestic cock (11 feet in length), artificially produced by the Japanese; (4) nest and eggs of the snow bunting (*P. nivalis*), taken during the past summer in Scotland.—Mr. Thomas Christy exhibited a new method of transmitting light to a microscope by means of a curved rod of glass.—The Rev. R. Baron read a paper on the flora of Madagascar, in which he gave an interesting account of his explorations and collections in that country.—In another paper, entitled "Further Contributions to the Flora of Madagascar," Mr. J. G. Baker, F.R.S., described the principal novelties brought home by Mr. Baron, and paid a well-deserved tribute to his energy and ability as a botanical explorer.

Mathematical Society, November 8.—Sir James Cockle, F.R.S., President, in the chair.—At the commencement of the meeting the Chairman feelingly dwelt upon the loss the Council and the Society had sustained by the recent decease of Arthur Buchheim (see NATURE, vol. xxxviii. p. 515).—The gentlemen whose names were given in a recent issue having been elected on the new Council, the new President (J. J. Walker, F.R.S.) took the chair, and called upon the retiring President to read his address on the confluences and bifurcations of certain theories.—Other communications that were made were:—Cyclotomic functions, § 1, groups of totitives of n; § 2, periods of nth roots of unity, by Prof. Lloyd Tanner.—On a theory of rational symmetric functions, by Captain P. A. MacMahon, R.A.—The factors and summation of $1^r+2^r+\ldots+n^r$, by Rev. J. J. Milne.—Raabe's Bernoullians, by J. D. H. Dickson.—Certain algebraical results deduced from the geometry of the quadrangle and tetrahedron, by Dr. Wolstenholme.—On a certain atomic hypothesis, by Prof. K. Pearson.—On deep-water waves resulting from a limited original disturbance, by Prof. W. Burnside.

Entomological Society, November 7.—Dr. D. Sharp, President, in the chair.—M. A. Wailly exhibited a large and interesting collection of Butterflies recently received from the Gold Coast and other parts of West Africa. The collection included about forty-seven species belonging to the genera *Papilio*, *Diadema*, *Salamis*, *Romalæosoma*, *Charaxes*, *Harma*, *Eurypheme*, *Junonia*, *Aterica*, *Hypanis*, *Eurytela*, *Mycalesis*, *Cyrestis*, *Nepheronia*, *Mylothris*, *Belenois*, &c. M. Wailly stated that several of the species were undescribed, and were not represented in the British Museum collections.—Mr. Jenner-Weir exhibited four bred specimens of Ant-lions, two of which were from Saxon Switzerland, and the other two from Fontainebleau. He stated that he believed the specimens belonged to two distinct species. Mr. McLachlan said that the specimens all belonged to one species, viz. *Myrmeleon formicarius*, Auct. = *europæus*, McLach.—Mr. W. C. Boyd exhibited an example of *Pterophorus zetterstedtii*, taken at Sydenham. He remarked that this species had hitherto only been recorded from Lynmouth and Folkestone.—Mr. Enock exhibited specimens of *Cecidomyia destructor* (Hessian Fly), illustrating the life-history of the species, and made remarks on them.—Mr. Wallis Kew exhibited a specimen of *Dytiscus marginalis* having a small bivalve shell attached to one of its legs. The bivalve had apparently attacked the *Dytiscus* and refused to relax its grasp. A discussion ensued, in which Dr. Sharp, Mr. Stainton, and Mr. Kew took part.—Mr. W. E. Nicholson exhibited several specimes of *Acidalia immorata*, Linn., caught by him near Lewes. Mr. Jenner-Weir observed that the species had only recently been added to the British list, and that it was remarkable how so comparatively large a species could have been hitherto overlooked. It was also remarked that a specimen of this species from the collection of the late Mr. Desvignes had been exhibited by Mr. Stevens at the meeting of the Society in November 1887.—Dr. Sharp exhibited a large number of species of Rhynchophora, collected by Mr. George Lewis in Japan.—Mr. F. P. Pascoe read a paper entitled "Descriptions of New Longicorn Coleoptera."—Dr. Sharp read a paper entitled "The Rhynchophorous Coleoptera of Japan."

Paris.

Academy of Sciences, November 3.—M. Janssen in the chair.—Essay on the theory of the Belleville carriage-spring, by M. H. Resal. This spring, devised about twenty-five years ago, has yielded excellent results in its application to railway rolling-stock. Here the principle of its action is worked out theoretically.—On the advantages of the use of electric light in the observations of marine zoology, by M. de Lacaze-Duthiers. An account is given of the system of electric light now in use at the Arago Laboratory of the Banyuls station, by means of which the author has been enabled to carry out some of his most important recent observations on marine life. The transparent animals especially can be studied with great advantage in a luminous atmosphere, revealing even the embryonic organisms, which cannot be detected in ordinary light.—Positions of Barnard's comet (September 2, 1888) measured at the Observatory of Besançon, by M. Gruey. The observations were taken jointly with M. Hérique for the period from October 11–17.—Observations of Barnard's new comet (October 30), and of Palisa's new planet, 281, made at the Paris Observatory (equatorial of the west tower), by M. G. Bigourdan. The positions of the comparison stars and the apparent positions of the comet and of the planet for November 3 are given.—On a triple determination of the latitude of the Gambey circle, by M. Périgaud. These determinations, effected by means of the new mercury bath described in the *Comptes rendus* for March 16, 1888, show that the latitude of the circle is as nearly as possible 48° 50′ 10″·9. It also appears that the latitude does not vary with the seasons, the result obtained in October 1888 being identical with that previously determined by the same instrument in June 1887.—On a means of studying the slight deformations of liquid sur-

faces, by M. J. B. Baille. Fizeau's extremely delicate method of measuring minute distances is susceptible of a large number of applications, and is here employed accurately to determine all the deformations of a liquid surface, however slight be the actions causing them. By this process the author has been enabled to observe the surface deformation of magnetic and diamagnetic fluids under the action of a weak magnet. He also shows that, as a copper wire traversed by a strong current attracts iron, it also attracts the surface of the perchloride of iron in solution.—On the occlusion of gases in the electrolysis of the sulphate of copper, by M. A. Soret. The author's researches lead to the conclusion that the electrolyzed copper always contains a certain quantity of gas, almost exclusively hydrogen. It retains a little carbonic acid and sometimes a very slight quantity of carbon oxide. A certain relation exists between the quantities of gas occluded and the conditions of temperature and acidity; consequently the quantity of gas present in the metal is variable, and the proportion 4·4 volumes, given by Lenz (*Journ. prakt. Chem.*, cviii. p. 436), is applicable only to the particular case studied by that physicist.—On tin, by M. Léo Vignon. If a zinc plate be plunged into an aqueous solution of one of the tin chlorides, the latter metal is precipitated by the zinc according to certain known thermo-chemical relations. The tin so precipitated possesses some special properties, which are here studied for the first time. An inquiry is also made into the cause of this modification of the fundamental properties of tin. The result of this inquiry is that the modified tin, which is infusible, is a mixture of metallic tin and of the anhydrous protoxide of tin.—On the homopterocarpine and pterocarpine of red sandalwood, by MM. P. Cazeneuve and L. Hugounenq. These two extracts of red sandalwood, described in the *Comptes rendus*, civ. p. 1722, are here methodically studied under the action of the chief reagents. Their respective formulæ are now shown to be $C_{24}H_{24}O_6$ and $C_{20}H_{16}O_6$, and there can be no longer any doubt that pterocarpine is a lower homologue of homopterocarpine.—On a substance at once acid and basic contained in cod-liver oils, by MM. Arm. Gautier and L. Mourgues. This substance, to which the authors give the name of morrhuic acid presents considerable interest owing to its double function of an acid and an alkali, as well as for its relative abundance and its origin, which is probably connected with the vegetable lecithines. It is present in these oils under the form of an unstable and complex combination, behaving like the ordinary lecithines—that is, it is modified, especially when heated in the presence of acids and alkalies, liberating glycerine, phosphoric acid, and a complex acid. It corresponds to the formula $C_9H_{13}NO_3$.—M. V. Marcano describes a fermented drink (*yaraque*) extracted by the wild tribes of the Upper Orinoco from Cassava; M. Martinaud studies the analysis of the yeast of beer; and M. Émile Rivière reports on the human and animal remains found in the Caves of Baumias de Bails and Saint-Martin in the Alpes Maritimes.

BERLIN.

Physiological Society, October 26.—Prof. du Bois-Reymond, President, in the chair.—Prof. Wolff spoke on the growth of the lower jaw. Notwithstanding the opposition of some observers, Flourens's view of the growth of bone by apposition and absorption is still widely applied to the lower jaw, and Humphry's experiments on the growth of the ascending branch of the same have been advanced in support of this theory. The speaker had therefore made a large number of experiments on goats and rabbits, by firmly attaching two wire rings to the bone while the animals were still young; one ring was placed at the *pars incisiva*, the other at the angle of the lower jaw. His conclusions are based upon the results of forty-two experiments; of these twenty-three showed an increase of 7 to 9 mm. in the distance between the wire rings in three to six months, while in twelve other cases a distinct but smaller increase in the distance between them was observed, so that only seven cases yielded no positive result. Bearing in mind the value which must always be attached to a few positive results even when opposed by many negative, it appears that the above-mentioned large preponderance of cases in which an increase in the distance between the marks was observed fully justifies the conclusion that the lower jaw grows by expansion. This proof of the interstitial growth of bone, together with the proved adaptability of all bones to the statical conditions of the demands made upon them, will, Prof. Wolff hopes, put an end to the idea that bone-tissue is inactive, and replace it by the theory he has so long held that bone is capable of active vital growth even in old persons.—Dr. Hans Virchow gave an account of the results of his experiments on the development of blood and the blood-vessels in the chick. In especial he pointed out that the blood is developed very early in the mesoblast, and takes up a peculiar position in the same. He next spoke on the yolk-sac of the chick. After he had explained the chief points and results of his researches, he was obliged to defer the rest of his communication to the next meeting, owing to the lateness of the hour.

AMSTERDAM.

Royal Academy of Sciences, October 27.—M. Behrens discussed the origin of the volcanic lakes in the Eifel Mountains, and demonstrated that they could not have originated in the crumbling down of extinct volcanoes. He endeavoured to show that the Eifel Lakes must be regarded as incomplete volcanoes, and that they were formed by the softening and continuous blasting of the sedimentary rocks, only a little lava having been brought to the surface.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Butterflies of the Eastern United States and Canada, with special reference to New England, Part 1: S. H. Scudder (Cambridge, Mass.).—Index Catalogue of the Library of the Surgeon-General's Office, United States Army, vol. ix. (Washington).—The Invisible Powers of Nature: E. M. Caillard (Murray).—Thermodynamique: J. Bertrand (Paris, Gauthier-Villars).—Untersuchungen über Dammerungserscheinungen: J. Kiessling (Hamburg, Voss).—University College, Nottingham, Calendar 1888–89 (Nottingham, Sands).—An Introduction to Entomology, Part 1: J. H. Comstock (Ithaca).—Annual Report of the Secretary for Mines and Water Supply, Victoria (Melbourne, Brain).—Foreign Aviary Birds, &c.: Dr. K. Russ (Dean).—Macaws, Cockatoos, Parrakeets, and Parrots: Sir T. D. Lauder and Captain T. Brown (Dean).—Euclid's Elements of Geometry, Books I.–IV.: A. E. Layng (Blackie).—Questions and Examples on Elementary Experimental Physics: B. Lœwy (Macmillan).—Practical Metallurgy and Assaying: A. H. Hiorns (Macmillan).—Gleanings in Science: G. Molloy (Macmillan).—Sitzungsbericht der K. Akademie der Wissenschaften, Mathematisch-Naturwissenschaftliche Classe (Mathematik, Physik, Chemie, Mechanik, Meteorologie, und Astronomie, Heft 3–10), (Mineralogie, Botanik, Zoologie, Geologie, und Paläontologie, Heft 1–10), (Physiologie, Anatomie, und Theoretischen Medicin, Heft 1–10) (Wien).—The Meteorite of November 20, 1887: H. G. Fordham (Hertford, Austin).—Mineralogical Magazine, October (Simpkin).—Notes from the Leyden Museum, October (Leyden, Brill).—Journal of Physiology, vol. ix. No. 4 (Cambridge).

CONTENTS.

THURSDAY, NOVEMBER 22, 1888.

THE OPENING OF THE PASTEUR INSTITUTE.

"WE cannot refrain from expressing some regret that the encouragement of scientific research should be one of the things which they do better in France than among ourselves." With these words, trenchant enough if heeded by those in authority on whose ears they may fall, the *Times* concludes a leader on the inauguration of the Pasteur Institute by the President of the French Republic. Such a ceremony naturally suggests two distinct points for consideration: (1) the object of the institution thus inaugurated; (2) the interest attaching to the ceremony.

The Pasteur Institute is remarkable among all others in being the best form of monument ever erected, and at the same time in its being raised during the life-time of the distinguished man of science, in whose honour and for the furtherance of whose work it was designed. That the debt which the community owes to M. Pasteur will never be paid, nor even adequately acknowledged, needs no insistance; but we may be excused if we dwell upon this point a little, for in the multifarious and different battalions of the workers in the army of science there may well be some whose particular work has not quite brought home to them their obligation to him.

The most remarkable characteristic of M. Pasteur's work, the one which places it on so unique a pedestal, is the fertility of its results in every direction. To have elucidated at once the causation of most forms of fermentation, and the causation of most forms of acute febrile disease (this last leading to the infinitely precious invention by Sir Joseph Lister of antiseptic surgery), is on the chemico-biological side of natural science a feat of as great abstract value and of greater immediately practical worth to the community than any one, or even two, of the greatest epoch-making discoveries of physical science. If it were not for the lamentable consequences of the apathy with which the British public regard science and its contributions to their health and wealth, it would be sadly amusing to read, as anyone may do in even well-founded prints, the lay opinion that M. Pasteur is but a hydrophobia curer, and possibly a slightly more successful one than McGovern, the Irish quack. The flame of popular knowledge of current science always burns most unsteadily, and any sensational wind makes it flare for a short time, and then it sinks almost extinguished. It has thus been with the most recent work of M. Pasteur; and so we find at the inauguration of the Institute the wide subject of the chemico-biology of disease processes was subordinated to the representation of the existing condition of our knowledge of the treatment of rabies.

Although, considering the national importance of the general principles of M. Pasteur's work, this preponderance of attention given to one subject may be regretted, it nevertheless must be admitted that a specific instance is more easily "understanded of the people," and may consequently more energetically drive home the wedges of scientific truth. To M. Grancher was most justly accorded the very agreeable task of expounding in a few simple and unadorned sentences the results of the anti-rabietic treatment of M. Pasteur. Though rabies, or hydrophobia, has always occupied such a special position in the public mind, this has not prevented the application of the general principle of public ingratitude; and we are therefore in no wise surprised to find that the benefactor who arose, and, at his own risk and cost, attempted to remove such an evil, should have been received with calumny and misrepresentation. The consolation afforded by the unerring verdict of time rarely comes—as in the present case it fortunately has to M. Pasteur—before the benefit-conferring Prometheus is past receiving it.

M. Pasteur has always borne the monstrous attacks made upon him with such dignity and composure, that the summary by M. Grancher of the great works suggested by him must have been an intense gratification and recompense.

Our sympathy with his pleasure is unfortunately alloyed with regret that of recent years health has been denied him for the perfect enjoyment of his renown.

The announcement by M. Pasteur in 1885 (the year of the epidemic of rabies in London) that he had not only succeeded in rendering dogs refractory to rabies by means of prophylactic inoculations, but had also with the same material attempted, and apparently successfully, the curative treatment of two human beings, marked the commencement of a widespread application of his now fairly well-known methods.

From the first, M. Pasteur recognized the effect that such an announcement would have upon the public mind, and, in addition to forming a resolution only to treat assured cases of rabies (a resolution he had ultimately to abandon on the grounds of humanity), arranged the facts of his work in such a manner as to provide for complete statistical accuracy in his records.

By his prescience we are thus placed in possession of an overwhelming series of facts relating to persons bitten by rabid animals. He arranged those who came to him under these circumstances into three categories.

In the first (Class A) he placed persons bitten by animals indubitably proved to be rabid by the results of inoculation from the spinal cord into normal animals.

Secondly (Class B.), he grouped together those cases in which the state of the animal, though not tested by experiment, was nevertheless certified to have been rabies by a veterinary surgeon.

Finally, he constructed a third order (Class C.) in which were collected those cases in which, owing to escape, &c., of the dog or animal attacking, no precise information as to its condition could be obtained, but only a presumptive suspicion that it was rabid.

Before we review the figures derived from these three classes of patients, it is important to gauge the character of the statistics of the general mortality from the disease with which they have to be compared. It is only since special attention has been drawn to rabies through M. Pasteur's work that trustworthy statistics have been forthcoming. In former years estimates of various kinds were from time to time prepared, but while some authors took only cases of the most virulent kind, and consequently obtained exceedingly high death-rates among those bitten, others accumulated large numbers of instances the details of which were most imperfectly

ascertained, and the mortality percentages thus deduced consequently utterly untrustworthy. The severest test that could be conceived for genuine criticism of M. Pasteur's method is obviously the comparison of the death-rate in his Class A. with that among persons, not his patients, proved to have been bitten by rabid dogs by the fact of at least one of those attacked by the animal dying of the disease. Such a comparison is now fortunately possible. The probability of rabies following the bite of a rabid dog is now definitely ascertained to be from 15 to 16 per cent. of those attacked.

Now the death-rate in M. Pasteur's Class C. is no more than 1·36 per cent., even including every fatal case—that is, inclusive of those persons who develop the disease during the first fifteen days after the bite. The rigid comparison of these two death-rates may well afford M. Pasteur the satisfaction of feeling that he has saved a number (to be counted by hundreds rather than tens) of his fellow-creatures from the most agonizing of deaths, and an enormous number from the worst of apprehensions.

For general biological science the next most interesting statistics are those which seem to reveal the mode of action of the curative and prophylactic inoculations. M. Pasteur's explanations of the beneficial effects of the material inoculated was that the nerve-tissue contained not only the microbes, the causative factors of the disease, but also their metabolic products, and that these latter by accumulation inhibit the growth and spread of the organisms. If, therefore, these products were injected into the blood-stream in sufficient quantity, he believed that the animal so treated would be protected from the malady. In this country Dr. Wooldridge had already proved experimentally the occurrence of such a process in the case of anthrax or splenic fever. Now the accumulated experience of M. Pasteur's laboratory goes very far to establish this theory for rabies also. Thus in Russia, where rabies is frightfully prevalent by reason of its being endemic among wild (wolves notably) as well as among domestic animals, the figures obtained from the respective inoculation stations are most striking :—

	Odessa death-rate per cent.	Moscow death-rate per cent.	Warsaw death-rate per cent.
1886. "Traitement simple" (*i.e.* small quantities injected)	3·39	8·40	3
1888. "Traitement intensif" (*i.e.* large quantities injected)	0·64	1·60	0[1]

It is abundantly evident from these figures that successful protection is due to the energy and frequency with which inoculations are practised, or, in other words, to the quantity of protective material injected. While we cannot too heartily congratulate M. Pasteur on his triumph in finding a cure for this miserable disease, we feel very glad that, since his work has established the true nature of rabies and its mode of propagation among animals and men, the French authorities have at last awakened to the fact that there is no disease which can be more successfully prevented by legislation. M. Grancher exhibited a chart showing the immediate effect of preventive legislation in reducing the prevalence of the malady in the Department of the Seine. For us, our own experience of the measures whereby the disease was temporarily extirpated from London (though now, of course, reappearing since the relaxation of the restrictions) is so strong that we hope this additional evidence will induce our Privy Council to apply such measures throughout the country; and having thus stamped out the disease in England, prevent by suitable contra-importation measures the re-introduction of the disease.

[1] "Intensif" treatment for last sixteen months—no death.

So much for the work of the Institute as immediately in operation. The special interest of the inauguration ceremony is noteworthy. We have already referred to it as being in part due to the personal monument it establishes to the genius of M. Pasteur, but it has a more particular interest for British national science. It lies in the fact that here we see an institution erected for the national purpose of scientific investigation into the causes of diseases and their mode of prevention. We see, moreover, the head of the Executive Government, in company with the members of his Cabinet, personally giving to the movement his cordial interest and support. It must make us all wonder when our Government will cease to regard the social and political importance of scientific investigations with other than an absolutely ineffective interest.

At present, for scientific investigations of this kind this country and its Government are positively dependent upon the charity of a private laboratory, that of the Brown Institution, the income of which, utterly inadequate, is very imperfectly helped by the defrayal on the part of the Government of simply the immediate expenses of the work done for them. And at the same time we wonder when our Government will remove the disgraceful legislative hindrances to British scientific work. Finally, we may ask, When shall we see the scientific millennium of an English Ministry taking an immediately personal interest in the welfare and support of such an institution? We can only conclude in the spirit of the words of the *Times* with which this article begins; and hope that, if it is generally appreciated how the lead has been taken from this country by France, at least an effort will be made by those who are responsible for the discredit thus forced on us to remove the blot by organizing a somewhat similar institution in England.

PRACTICAL BOTANY.

A Course of Practical Instruction in Botany. By Prof. F. O. Bower, D.Sc., F.L.S. Part I. Second Edition. (London: Macmillan and Co., 1888.)

THE first edition of Part I. of Profs. Bower and Vines's "Practical Botany" was published in 1885 (see NATURE, vol. xxxii. p. 73); and during the three years that have elapsed the book has become familiar in all botanical laboratories, and has proved an important aid to the work of both teachers and students. This first part deals with the Phanerogams and Pteridophytes. Part II., completing the work, appeared only last year (see NATURE, vol. xxxvii. p. 28), and thus the former part has reached a second edition while its companion volume is

still a new book. In preparing the new edition, Prof. Bower has no longer had the active co-operation of Dr. Vines; though, as explained in the preface, the chapters originally contributed by the latter have been almost entirely embodied in the present work. A short account of the book as it now stands may be of service to the readers of NATURE, for as compared with the former edition it has undergone considerable rearrangement and extension. In both these respects the first three chapters, which are introductory to the study of the types, show important changes. The book begins with a list of apparatus, and of the more ordinary reagents, the mode of preparation of which is described. A more detailed list of reagents is now given in the first of the new appendices at the end of the volume.

Chapter I. deals with the making of preparations, and the adjustment of the microscope. Under both heads the directions are most practical and excellent, and cannot be too strongly commended to the attention of students. This chapter is essentially elementary; and only simple methods, such as are indispensable for all workers, are included. A very clear account of section-cutting is given, and in Fig. 1 a diagram is added in order to explain the meaning of "radial" and "tangential" sections, a point which is often puzzling to beginners. The introduction of diagrams is an important feature of the new edition. Though not very numerous (fifteen in all) they will be found a very useful help. Strictly diagrammatic figures have been used in all cases, in order that the student may not be tempted to make use of the illustrations as substitutes for the objects illustrated. The first chapter concludes with instructions on drawing from the microscope, and on measurement.

Chapter II. contains a number of "practical exercises." In the first of these the microscopic examination of the pulp of an apple serves to make the student acquainted with the general characteristics of vegetable cells.[1] The next exercise is on Spirogyra, and the third on the Fern-prothallus. A reference to the general account of the prothallus at p. 300 would here be useful to the beginner. Next comes the Beet-root, the first object of which sections have to be made. Here the chief osmotic phenomena are studied. Protoplasmic movements are illustrated by Trianea or Hydrocharis, and by Tradescantia. The last exercise (in small print) is on cell-division, as shown in the staminal hairs of Tradescantia. This, we think, would have been better omitted at this place. It is impossible in a few lines to give a satisfactory account of so complicated a process, and inaccuracies can scarcely be avoided. Thus the statement that the nuclear "fibres are ruptured in the equatorial plane" cannot be accepted in the light of our present knowledge.

The third chapter, headed "Micro-chemical Reactions," gives a series of further practical exercises of a more advanced character. These are only in part designed for the beginner, and those which he is intended to work through are indicated by a marginal line. The remaining parts of the chapter are designed to be used for purposes of reference, during the subsequent investigation of the types. The improved account of the chlorophyll-grains (p. 51) and the fuller description of vegetable oils (p. 59) may be especially noticed. Into this chapter an adequate account of the chief facts relating to the division of the cell and nucleus might perhaps have been introduced with advantage, as their satisfactory observation with the help of modern methods is by no means beyond the powers of the more advanced students.

The introductory portion of the work terminates with some useful remarks on staining, clearing, and permanent mounting (pp. 65–70).

The study of the Dicotyledonous types begins with an examination of the seed and embryo in the Bean, the Cucumber, the Sunflower, the Castor-oil plant, and the Marvel of Peru. In the reviewer's opinion it would have been better to begin at once with the seedling, as the structure of the seed cannot possibly be really understood until the reproductive organs have been investigated. This especially applies to the last example cited, namely, Mirabilis, the description of which will, we fear, be found very puzzling by the student.

The main outlines of the description of types remain as in the former edition. Only a few of the chief alterations need be mentioned. Two excellent diagrams of bundle-systems (after Reinke) are introduced at pp. 79 and 81, while the investigation of the seedling Sunflower by means of successive transverse sections is a most valuable addition to the treatment of the difficult subject of the distribution of vascular bundles. Attention may be called to the remark, on p. 96, that the work on the *young* stem of the Sunflower may with advantage be taken earlier. The structure of the stem before secondary thickening has begun must certainly be understood in order that the subsequent changes may be intelligently studied. Diagrams, after Sachs, are introduced at p. 98 to illustrate the origin of the cambium, and at pp. 100 and 102 to show the arrangement of the cells at the apex of the stem. In the account of the periderm (p. 107) the terminology has been revised, and now agrees with that of De Bary. This subject is now illustrated by diagrams. These are original, and so also are those on p. 122, by which the form of cambial cells is explained.

At p. 137, the intercellular space beneath the stoma is still called the "respiratory cavity." This term, though so generally used, is likely to mislead beginners as to the function of the stomata, and the colourless phrase, "air-chamber," seems preferable. To the account of the structure of the petiole, a description of the pulvinus in the French Bean is now added. The Holly has been substituted for the Cherry-Laurel as the type of a coriaceous bifacial leaf; while, as examples of "iso-bilateral" structure, the phyllodes of Acacia, and the leaves of *Eucalyptus globulus*, are introduced. The aquatic type of leaf is well illustrated by Hippuris, in which the sub-aërial and the submerged leaves are compared. Passing by several minor additions, the valuable new section on the fall of the leaf may be especially noted. An account of hairs and emergences has also been added, in small type.

The work on the root of Dicotyledons has been expanded, and the very clear original diagram of its transverse section, before and after secondary thickening, is likely to be of great service.

The stem of the Monocotyledons is now described in two additional types—the Hyacinth, as a bulbous plant, and Elodea, as an aquatic. The last-named plant is also

[1] A misprint on p. 27 (line 4) may be noticed, where the word "as" has been omitted.

used to illustrate the modifications shown by submerged leaves, while the leaf of Iris is introduced as the example, among Monocotyledons, of iso-bilateral construction. In the account of the *root* of Monocotyledons we regret that no mention is made of the exodermis or hypodermal layer. The great importance of this layer as a protective dermal structure to the older roots, especially in plants destitute of periderm, has been sufficiently shown by Olivier and others, and there seems no reason why it should any longer be ignored in elementary teaching. The exodermis is indeed mentioned (under the older, but now somewhat confusing, name of endodermis) in the new section on *aërial* roots (p. 195), but it would be a mistake to suppose that it is by any means confined to roots of this category.

The work on the reproductive organs of Angiosperms has been on the whole but little altered, the naked-eye observation of various typical flowers being the most important addition. In the account of the Blue-bell (p. 203) it would perhaps be well to define the *perianth*, as beginners often fail to see that it corresponds to both calyx and corolla. The Rhododendron is introduced (p. 215) for the study of pollen-tubes. The section on the development of endosperm, and on the continuity of protoplasm between its cells, is new (p. 218).

Going on to the Gymnosperms, we find that the description of the stem of Abies now precedes that of Pinus doubtless on account of the simpler external morphology of the former. In the histological examination of the wood of Pinus we are sorry that the expression "irregularities of structure called bordered pits" has been allowed to stand (p. 230), as it does scant justice to these singularly beautiful organs. The introduction of the leaf and the root of the Yew is a great gain to this part of the anatomical investigation, while the description of the reproductive organs of the same plant is of even greater value.

Among the Pteridophyta, the chapters on Selaginella and Lycopodium have scarcely been altered. It may be pointed out that it is not quite accurate to describe the phloëm in the vascular cylinder of Lycopodium as forming a matrix (p. 268), for the true phloëm is limited to the isolated bands of tissue which alternate with the groups of xylem.

In the account of the homosporous Ferns the most important change is the introduction of Pteris for the minute structure of the vascular bundle. For this purpose it is certainly the best easily accessible type. Several useful diagrams now illustrate this chapter, among which that of the vascular skeleton of the Male Fern, will be especially welcome.

Pilularia is added as a new type, representing the heterosporous Filicineæ. It is certainly well that the student should be made acquainted with this interesting group of plants, and this addition may perhaps be regarded as the most important in the book. The relation of the vascular bundle in Pilularia to that of the true Ferns might have been made somewhat clearer if that type of bundle had been described, in which the two ends, as seen in transverse section, are not completely confluent.

Three appendices and an index have been added to this edition, the index referring to both parts of the book. Appendix I. includes reagents, and the method of preparing them. Appendix II. gives the reactions of the various substances occurring in plants: and Appendix III. is a most useful list of material, with directions for obtaining it. This last appendix is in two divisions, the second of which contains the material required for Part II. (Bryophyta—Thallophyta).

The extensive changes which this volume has undergone have rendered it more than ever an invaluable aid to the study of plants in the laboratory. English students may be congratulated on their good fortune in possessing such a hand-book, and we may confidently hope that the present edition of Prof. Bower's work may render even greater services to scientific education than did its predecessor.

D. H. S.

THE SENSES, INSTINCTS, AND INTELLIGENCE OF ANIMALS.

The Senses, Instincts, and Intelligence of Animals, with Special Reference to Insects. By Sir John Lubbock, Bart., M.P., F.R.S., D.C.L., LL.D. "International Scientific Series," Vol. LXV. (London: Kegan Paul, Trench, and Co., 1888.)

MUCH consideration of the ways of ants has imparted to Sir John Lubbock so large a measure of the wisdom of industry, that even King Solomon himself could scarcely have failed to appreciate the result. The work which has just appeared under the above title may be regarded as a sister volume to the "Ants, Bees, and Wasps," in the same scientific series. Its scope, however, is wider, and, in consequence, its subject-matter is calculated to be of even more interest to the general public, notwithstanding that "Ants, Bees, and Wasps" is already in its ninth edition.

"The Senses, Instincts, and Intelligence of Animals" runs to close upon three hundred pages, of which only about the last fifty are devoted to instincts and intelligence. The book, therefore, is primarily a treatise on the organs of special sense throughout the animal kingdom. As such, it deserves to be regarded as a valuable contribution, not only to the library of the general reader, but also to that of the working biologist. For while, on the one hand, it does not presuppose even the most elementary knowledge on the part of its readers, on the other it constitutes an excellent hand-book of reference to the principal literature on the subject. Of course, in the latter respect it is by no means exhaustive, nor does it profess to be what we understand by a text-book. Nevertheless, it will prove exceedingly useful as a book to be consulted by any naturalist who, having previously worked in other lines, may have occasion to require an index to the more important literature of sense-organs, especially of the Invertebrata. Considerably over one hundred authors are alluded to, and the essay is illustrated by 118 woodcuts, derived from the original memoirs quoted.

A considerable portion of this essay is occupied with an account of the author's own experiments on the special senses of insects, &c., together with replies to criticisms which have been advanced by high authorities in Germany and France, both as against some of his facts and some of his inferences. Without going into particulars, we may

say that in every case these replies appear to us completely satisfactory, and are everywhere rendered in a manner the courtesy of which not many English naturalists could nowadays emulate. But, besides answering criticisms, he has in several cases important criticisms to make. For instance, we have a tolerably full republication of his research upon the colour-sense of Daphnia, whereby he so completely overturned the results previously published by the late M. Paul Bert. The following is a good example of the application of his criticism in another direction:—

"With reference to the power which insects possess of determining form, Plateau has recently made some ingenious experiments. Suppose a room into which the light enters by two equal and similar orifices, and suppose an insect set free at the back of the room, it will at once fly to the light, but the two openings being alike, it will go indifferently to either one or the other. That such is the case Plateau's experiments clearly show, and, moreover, prove that a comparatively small increase in the amount of light will attract the insect to one orifice in preference to the other. It occurred, then, to Plateau to utilize this by varying the form of the opening, so that, the light admitted being equal, the opening on the one side should leave a clear passage, while that on the other should be divided by bars large enough to be easily visible, and sufficiently close to prevent the insect from passing. . . . The insects seem to have gone most often to the trellised opening. M. Plateau concludes that insects do not distinguish differences of form, or can only do so very badly. I confess, however, that these experiments, ingenious as they are, do not seem to me to justify the conclusions which M. Plateau draws from them. Unless the insects had some means of measuring distance (of which we have no clear evidence), they could not tell that even the smaller orifice might not be quite large enough to afford them a free passage. The bars, moreover, would probably appear to them somewhat blurred. Again, they could not possibly tell that the bars really crossed the orifice, and if they were situated an inch or two further off they would constitute no barrier. I have tried some experiments, not yet enough to be conclusive, but which lead me to a different conclusion from that of M. Plateau. I trained wasps to come to a drop of honey placed on paper, and, when the insects had learnt their lesson, changed the form of the paper. . . . It certainly seemed to me that the insect recognized the change."

In the remaining portion of the book, or the portion which deals with "Instinct and Intelligence," we have three chapters. The first is an admirable discussion of one of the most wonderful instincts in the animal kingdom, viz. that of the Sphex stinging only the nerve-centres of the spiders, insects, or caterpillars, which she thus paralyzes without killing, before inclosing them with her progeny, whose food they are afterwards to constitute. Sir John has some good critical remarks to offer on the subject, and also some shrewd speculations upon the possible origin of the instinct. His hypothesis very much resembles that which was arrived at independently by the late Mr. Darwin, and which, therefore, is now in part quoted by Sir John. The quotation runs:—

"I suppose that the sand-wasps originally merely killed their prey by stinging them in many places, and that to sting a certain segment was found by far the most successful method, and was inherited like the tendency of a bull-dog to pin the nose of a bull, or of a ferret to bite the cerebellum. It would not be a very great step in advance to prick the ganglion of its prey only slightly, and thus to give its larvæ fresh meat instead of only dried meat."

Here, by the way, we have an excellent instance of the difficulty which we so often encounter in the domain of instinct, when we relinquish the so-called Lamarckian principle of the inheritance of acquired characters. The hypothesis in question goes upon the supposition that some of the ancestors of the Sphex were intelligent enough to notice the peculiar effects which followed upon stinging insects or caterpillars in the particular regions occupied by nerve-centres, and that, in consequence of being habitually guided by their intelligence to sting in these particular regions, their action became hereditary, *i.e.* instinctive. But if, in accordance with post-Darwinian theory, we relinquish this possible guidance by intelligence, and suppose that the whole of this wonderful instinct was built up by natural selection waiting for congenital (*i.e.* fortuitous) variations in the direction of a propensity to sting, say, the nine nerve-centres of a caterpillar—then it surely becomes inconceivable that such an instinct should ever have been developed at all.

A chapter on the supposed sense of direction among the Social Hymenoptera, and another on his now well-known experiments in teaching a dog the use of written signs, bring to a close one of the most instructive and entertaining of the works which have been produced even by Sir John Lubbock.

GEORGE J. ROMANES.

MASSAGE.

Massage and Allied Methods of Treatment. By Herbert Tibbitts, M.D. (London: J. and A. Churchill, 1888.)

It is seldom that a medical book of such inferior quality has been issued from the press, and the fact that it has found any purchasers is a striking proof how a catching title and an attractive exterior can still mislead the public. Anyone even slightly acquainted with the subject will at once perceive that the writer, whilst professing to teach massage, has not mastered the first principles of the treatment. His modest refusal to accept the office of "high priest of massage" has, indeed, complete justification.

It is not easy to adopt any method in criticizing a work devoid of all attempt at arrangement, but from the chaos of thought and diction we will select a few samples of what the writer has considered suitable food for the minds of his readers.

At the outset the author attempts to define massage, and with a dim consciousness that he has somehow failed, he plaintively declares that his definition is misty. Out of this verbal fog he never emerges, and as he pursues his erratic course it rapidly thickens around him. He has introduced illustrations and quotations from other writers, and in mercy to his readers also gives references to standard authors, who may be read with advantage. Unfortunately he at times becomes bold even to rashness, and launches out on his own account. A few samples of the inevitable result will suffice. On p. 27, whilst in the midst of giving directions for treating the lower limbs, he intercalates the following sentence: "You then massage the muscles from the waist downwards, working

upwards as before." This has no connection with what has gone before or what follows after. On the next page he says, "for the large and small intestine you massage the lower part" (of the abdomen), having evidently forgotten the position of the transverse colon, which anatomists still believe to be a part of the large intestine. Again, after giving all the less important uses of the saliva, he entirely omits its action in changing the starchy foods into sugar, an omission of which a second-year student would scarcely have been guilty. His readers are left in ignorance of the emulsifying action of the bile on fatty foods, and the pancreas is only considered worthy of mention. In fact the writer, after intimating that the functions of the body could be very well carried on without such an important gland as the spleen, with the modern physiology of which he does not acquaint his readers, leaves us under the impression that the organization of the human body would have been much better planned had Dr. Tibbitts been the designer.

The author claims for a battery he has invented certain qualities, which he declares to be unique, although they are possessed by other machines. He claims for his hospital the honour of being the only one to which a school for massage is attached, totally ignoring what is being done at other institutions. He is the forerunner of Apostoli, and modestly likens himself to Paul and Apollos, he does not say which. "Although Paul planted, Apollos watered," is his misquotation of the Scriptures. He robs Sir James Paget of the honour of a "discovery." Sir James "suggested," but Dr. Tibbitts "originated" afterwards! After claiming on very insufficient grounds to be a forerunner, a discoverer, and a prophet, he finally declares that all the authorities before him were as blind leaders of the blind. Charcot, Russell Reynolds, Hughlings Jackson, Gowers, and such small fry, are all wrong—for has he not looked into all the authorities?—and he now announces in defiance of them the tremendous fact that there is no such thing as hysteria! However, the apparent object of the book has been attained, and the great Holloway must hide his diminished head.

OUR BOOK SHELF.

Rock-forming Minerals. By Frank Rutley, F.G.S., Lecturer on Mineralogy in the Royal School of Mines. With 120 Illustrations. (London: Thomas Murby, 1888.)

THIS book appears to supply a real want among students of that now very popular subject of study, microscopic petrography. Many of the existing text-books, which are for the most part written in German and French, demand a larger acquaintance with the principles of crystallography and physical optics than many students of the subject possess. Mr. Rutley evidently possesses a considerable experience of the wants of students, and is familiar with the kind of difficulties which prove most troublesome to them. With the greatest patience he endeavours to remove these hindrances to their progress, pointing out the different senses in which the same term is sometimes employed, cautioning them against prevalent misunderstandings, and advising them as to the best method of forming just conceptions concerning the abstruse problems with which they have to deal. Very noteworthy and excellent are the numerous drawings, which, though severely diagrammatic rather than pictorial, are admirably suited for their object. The student who follows the advice of the author, and by the aid of cardboard, cork, and pins, constructs a series of models based upon these drawings, will be able to realize the essential peculiarities of the several mineral species in a way that no amount of description will enable him to do. In the general arrangement of this book, Mr. Rutley has followed the same excellent plan as Prof. Rosenbusch in the first volume of his excellent "Mikroskopische Physiographie." The first part of the book, comprising 104 pages, is devoted to general considerations, and the second part (144 pages) to a description of the crystallographic and optical peculiarities of the chief rock-forming minerals, these being grouped according to their system of crystallization. In every part of the book there is evidence of the most painstaking care and conscientious attention to accuracy of detail, and we can heartily recommend the book to those who seek for just such an amount of information on optical principles as will enable them to employ the modern refined methods of petrographical research.

A Text-book of Euclid's Elements for the Use of Schools. Parts I. and II., containing Books I.–VI. By H. S. Hall, M.A., and F. H. Stevens, M.A. (London: Macmillan, 1888.)

WE have here the completion of a work which in its first instalment (Books I. and II.) has already won a considerable amount of favourable notice from teachers. The "end" has "crowned the work" in a similar satisfactory manner; and, without entering into any "odious" comparisons with recent like editions, we consider this to be abreast of the best. Great attention has been paid to the arrangement and composition of the text, and the difficulties which delay beginners have been carefully smoothed and explained. The ordinary proofs have been adhered to as much as possible, and, in the words of the preface, "changes have been adopted only where the old text has been generally found a cause of difficulty."

Alternative proofs are given in many cases, which are less cumbrous than those in vogue already. The subject of proportion has been treated on the system advocated by De Morgan, and here great use has been made of the admirable exposition of it given in the Association's (A.I.G.T.) text-book. The principal propositions have been established in a clear manner, both from the algebraical and geometrical definitions of ratio and proportion, and the distinction between the two modes of treatment is well brought out. The whole of this part forms a good introduction to the sixth book.

The additional feature in the complete treatise is the free use in the third and subsequent books of the signs and abbreviations which are recognized by most teachers, and allowed in the University examinations.

The explanatory matter and additional sections contain all, or nearly all, that is looked for nowadays, and include articles on harmonic section, centres of similarity and similitude, pole and polar, radical axes and transversals. The exercises in the text are well graduated, and should bring out the pupil's acquaintance with, and mastery over, the propositions to which they are appended. More difficult problems are led up to by the solution of typical examples. In conclusion, we need only say the work before us contains all that is needful to a student, who, if he has this, will require no other text-book to become an expert geometer—*i.e.* in so far as outside aid can make one.

A Class-book of Elementary Chemistry. By W. W. Fisher, M.A., F.C.S. (Oxford: Clarendon Press, 1888.)

THE number of elementary books for students of chemistry has increased so greatly during the last ten years, that each new introduction gives rise to a question as to whether the author has justified his position in adding another

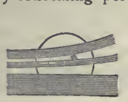
But, however far the supply exceeds the demand, there is always room for what is thoroughly good, especially if it has improvements that its predecessors lack. Though every author is apt to think his pet methods are the very best, and more or less inclined to regard his fads as steps towards perfection, if not indeed its full realization, there are a few who take a sounder view of things, and care nothing for novelty for its own sake. The author of the volume before us has shown that he is one of the few. This book is of sterling value, and will be welcomed by the teacher of elementary chemistry as a guide for his students that he will have pleasure and full confidence in placing in their hands. The volume is well got up, printed in clear type, and illustrated with a sufficient number of excellent diagrams, many from original drawings made by the author. Its 272 pages are not crowded with information or anything else, but the facts included are clearly described in a readable and concise manner. In scope, the book includes the principal non-metallic elements and their chief compounds, followed by the more important metals and their salts. The selection is good and not novel. The periodic law is briefly referred to, and the last eighteen pages are occupied with the chemical physics that it is usually considered well for elementary students to master, such as the relation between specific heat and atomic weight, critical temperature, diffusion of gases, effects of temperature and pressure upon gases, and so on.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Alpine Haze.

PROF. TYNDALL has done good service by drawing attention to Alpine haze, and is quite right in adding that it appears in horizontal layers. Such is its common form, but I have also observed a vertical part of it connecting two horizontal striæ rendered conspicuous by concealing portions of a setting sun,



Setting sun observed February 16, 1846, at Garuqqe. in lat. 8° 0'·6, long. 36° 51', at an altitude of 2175 metres.

just as thick boards might do. On another occasion I saw a rough column of it towards the north-west at a supposed distance of three or four kilometres. A few hours later, while I was noting down the phenomenon, a native exclaimed that it had changed its position, and on looking north-west I could see no trace of it, a column similar in size and distance being then in the north-east. It towered above my level on a rolling plain 2300 metres above the ocean. In another place I have seen it at a height of 3600 metres.

Although generally overlooked by meteorologists, this phenomenon has a special name in warm countries. Portuguese call it *neblita*; in Spain it is mentioned as *callina* or *calina*, and Basques name it *lañoa*. Nowhere have I seen it so frequent and thick as in Ethiopia, every different language of that wide region having a special word to express it. The Amara call it *tigag*; the Oromo, or Galla, *qayota*; the Tigray, *taqa*; while old Ethiopic employs the term *qobar*. I have used the latter in my published accounts, because *brume* in French and *haze* in English are generic and not specific names.

Qobar is gray, and of a livid hue when intense, verging sometimes to blackness. The Gascon-speaking population in the Pyrenees call it *bruma terranera*, *i.e.* earth-haze. Its edges are not jagged, like those of clouds, but quite smooth. At Quarata, in 1845, when I was at the level of Lake Tana, the Island of Daga, which rises suddenly 140 metres above the water at an angular height of 16' and a distance of 11·6 miles, was visible only by 4' or 5' of its upper part, the lower 11' or 12' being concealed by *qobar* thicker than usual, and seemingly spread on the lake. I have seen it often on the Red Sea, and sometimes even here in the Bay of Biscay towards the north.

Qobar is the surveyor's foe, and has made me lose several important bearings. It blurs the landscape, diminishes estimated distances, and in Ethiopia is often so thick that no feature of a country is visible beyond the space of a mile.

Fifteen years ago I published in my "Physique du Globe" all that I know about *qobar*. In Germany it goes by the name of *Heiderauch*, or by six other words all ending in "*rauch*." Ethiopians also compare it to but distinguish it from smoke. When commenting on chapter x. of Exodus, their native professors say that the darkness mentioned in verse 22 was an intense *qobar*, and go on to explain that the light enjoyed by the Children of Israel is fully borne out by the fact of *qobar* being sometimes prevalent in one place, yet absent in its neighbourhood. I have noted several instances of this partial occurrence. Without quoting them, I may mention that, according to my working hypothesis, *qobar* is only dry air, visible because in large quantity. On the other hand, astronomers well know that very moist air is the most transparent.

Natives are swarthy in countries where *qobar* abounds. Does it darken man's skin? At all events it is worth while to draw some hundred litres of it through suitable reagents. Chemists could thus test Kaemtz's notion that it is always smoke.

ANTOINE D'ABBADIE (de l'Institut).

Abbadia, Hendaye, November 10.

P.S.—I forgot to mention that, after crossing the three layers shown by the figure, the setting sun crossed two other layers, and finally disappeared behind the lower stratum of *qobar*, then 3° or 4° above the horizon.

Rankine's Modification of Newton's Investigation of the Velocity of Sound in any Substance.

PROF. EVERETT'S letter (November 8, p. 31) calls attention to a difficulty which is apparently felt by students over the attempted elementary method of deducing the general expression for the velocity of sound given in Maxwell's "Heat." Advanced students need feel no difficulty of the kind, because they arrive at it by another path; but inasmuch as the Rankine method seems the easiest available to intermediate students, it is desirable as a matter of pedagogy to put it in its simplest form; and so I venture to quote here the plan I have for some time adopted.

First lead up to the subject by considering the velocity of a hump on a stretched string. Explain the plan of imagining the string to move along at the same pace as the hump, but in an opposite direction, so as to keep the hump stationary in space, obtaining the velocity necessary to do this by equating the normal compound of the tension to the centrifugal force—

$$T \cdot \frac{ds}{r} = \frac{\lambda ds \cdot u^2}{r}, \quad \text{or } u = \sqrt{\frac{T}{\lambda}},$$

where T is the tension, and λ is the linear density of the string; and then actually show the experiment—running a light loose flexible endless cord on a pulley, and making a hump on it. The tension in a loose whirled endless cord free from gravity being that due to the centrifugal force only, viz.—

$$T = \frac{\lambda ds \cdot v^2}{r} \cdot \frac{r}{ds} = \lambda v^2,$$

it follows that $v = u$, and so the keeping of the hump still is automatic, except for a slight interference by the weight of cord hanging below the hump. This interference being less and less notable as the hump is initially made nearer the bottom of the loop of cord.

Next explain, and illustrate by moving diagrams, the simple harmonic motion of the particles of a medium conveying sound-vibrations.

Then proceed to consider a longitudinal pulse travelling along a substance contained in a tube of unit area, and imagine a wind of the substance blowing through the tube in the opposite direction with such a velocity, U, as just to keep the pulse stationary in space.

Except for an unessential disturbance due to friction, the pressure all through the tube is uniform so far as the wind-motion is concerned.

Erect across the tube a couple of imaginary partitions, and watch the substance streaming past them. The state of the substance at either partition, whatever it may be at one instant, remains permanently the same always; hence the mass of substance inclosed between the two must remain permanently the same—for it cannot be always steadily increasing—and therefore the mass of matter flowing through any one plane is constant.

In future attend to one of the planes only, and call the density of the substance at this place ρ. The plane may be at or near a condensation, it may be at or near a rarefaction, or, again, it may be where the substance has its ordinary density; whatever the state of the substance there, the same it remains. The longitudinal-pulse motion of the particles of substance (which has previously been illustrated and discussed at length) is superposed upon the wind-motion; and if we followed any one particle along the stream we should see it simply oscillating with a simple harmonic motion—

$$x = a \sin nt, \quad \text{or} \quad v = an \cos nt,$$

having at any instant the velocity v.

But we are not going to follow a single particle down stream; we are contemplating a procession of particles as they successively pass the fixed partition, and at the instant of passage they are all in a definite phase of their motion—they all have the same definite velocity, v, as they pass, in addition to their general wind-velocity, U. The vibratory velocity v may be in the same direction as U, or it may be in the opposite direction; it may have any value between $\pm na$, of course. So the resultant velocity of each particle as it passes the fixed partition is algebraically $\mathrm{U} + v$. This represents the length of cylinder of substance passing through the partition per second; and, since the partition is of unit area, the mass of substance flowing past it per second is

$$m = (\mathrm{U} + v)\rho, \text{ and is constant} \quad . \quad . \quad . \quad . \quad (1)$$

This of itself is an interesting result; for it shows that at the middle of condensations, where ρ is a maximum, v must have its greatest negative value; and the particles are therefore all in full swing back against the wind (*i.e.* travelling with the sound-pulse) at the middle of every condensation. At a rarefaction, v has its greatest positive value, and the particles are swinging with the wind (against the sound-pulse). Only at half-way places, where the density of the substance has its average or undisturbed value, are the particles quiescent as regards the sound-pulse.

Next consider the dynamics of the matter, and the force which must act to vary the motion of the particles.

If the pressure were the same on either side the partition, there could be no change of velocity for the particles as they pass. The charge of velocity, $d(\mathrm{U} + v)$, or dv, must be due to a difference of pressure existing on either side the partition; and if the slope of pressure $\frac{dp}{dx}$ is positive, the pressure is greater on the lee-side of the partition than on the windward side, and so the acceleration $\frac{dv}{dt}$ will be negative. Hence, equating the force acting and the momentum generated by it per second,—

$$dp = -m dv \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

This equation, along with equation (1), solves the problem, and determines the velocity U.

Differentiating (1)—

$$(\mathrm{U} + v)d\rho + \rho dv = 0.$$

Rewriting (2) by help of (1)—

$$(\mathrm{U} + v)\rho dv = -dp.$$

Substituting for ρdv from one of these into the other, we get—

$$(\mathrm{U} + v)^2 = \frac{dp}{d\rho} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This equation shows that $\frac{dp}{d\rho}$ is by no means constant all through the substance. It is greatest wherever v has its maximum positive value—that is, at the centre of every condensation; it is least at the centre of every rarefaction; it has an average value in the undisturbed portions of the medium, and it is there equal to U^2.

Hence we learn that the value of U is determined by calculating the ordinary value of $\frac{dp}{d\rho}$ for the medium in its uncompressed and unrarefied state.

So—

$$\mathrm{U} = \sqrt{\frac{dp}{d\rho}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This is the velocity with which the substance must flow through the tube in order to keep the sound-pulse stationary: this, therefore, is the velocity of sound in it.

The result is general, and applies to all substances. But for gases it may be written more explicitly by help of their characteristic equation $\frac{p}{\rho} = \mathrm{RT}$, and their adiabatic condition $p \propto \rho^\gamma$: viz.—

$$\mathrm{U} = \sqrt{\gamma \mathrm{RT}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

T here means the undisturbed temperature of the gas, but if one chooses to allow the equation to follow the fluctuations of temperature ($\pm t$) adiabatically produced in the condensations and rarefactions, one must write the more general form—

$$(\mathrm{U} + v)^2 = k\mathrm{R}(\mathrm{T} \pm t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

which gives us the relation between fluctuation of temperature and vibrational velocity. We may also write it—

$$\rho^2(\mathrm{T} \pm t) = \text{const.} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

which shows the connection between the elevation or depression of temperature, and the density, at any part of a sound-wave.

Speaking as a teacher, I believe one reason why we fail to make things clear is, because we are often in too big a hurry. One's natural tendency is to give such an investigation—as this, for instance, in Maxwell's "Heat"—in a few lines on the blackboard, taking perhaps half an hour or less over it, and forgetting that it embodies in concentrated form a great deal of difficult thought, though the actual mathematics may be simple. Gradually I am learning not thus to scamper over the ground, but to lead up to a thing in two or three or even more lectures, and then to devote a whole hour to the thing itself. By this means, students may ultimately be got to grip and feel the thing as a whole, instead of having to ascend step by step to it; but it is hopeless to expect them to thus grasp it straight off; and even if it were possible, it would not be really desirable for various reasons. The attempt to hurry them into the comprehension of difficulties leads them, I believe, into a vague notion that everything is hazy and half unintelligible. The best thing we can do for them is to get them to see some few things luminously, so that they may not feel inclined to rest satisfied with half-knowledge in other instances.

OLIVER J. LODGE.

November 12.

P.S.—Since writing the above, I have referred to Prof. Everett's note A in Deschanel, and have found it excellent, like all his notes; he happens to have employed just the same means as the above for obtaining equation (1), but for the latter part I prefer my statement. I trust no one will imagine that the above contains anything more than a way of putting things to students.

The slip of a wrong sign in Maxwell I had not distinctly noticed, but the simplest statement of it seems to be that in obtaining the second equation he has put, for the change of velocity of each particle as it passes a plane, du instead of $d(\mathrm{U} - u)$; that is, the change of absolute instead of relative velocity.—O. J. L.

A Simple Dynamo.

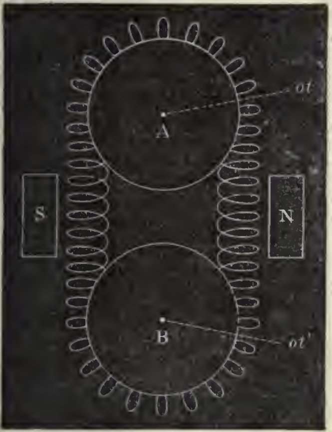
I VENTURE to send you a brief description of a simple electromagnetic instrument which I have recently devised for illustrating the principle of the Gramme ring.

Two pulleys, A, B, having semicircular grooves, are mounted, as shown in the figure, on a piece of board; round the two wheels is stretched a continuous coil of copper wire; a horse-shoe magnet is placed with its poles close to the vertical parts of the coil; the wheels are connected to the terminals t t': when the wheel A is rotated the whole coil moves, and a steady current is at once generated, which flows from terminal to terminal when they are

connected together, the direction of the current depending on the direction of rotation.

It will be noticed that, since the coil and wheels are always in contact, no undulations are produced as when brushes come in contact with successive ends of coils, as in the ordinary dynamo.

When the instrument is placed in circuit with a sensitive galvanometer, the rotation being constant, no variation in the current can be detected, even when the motion is very slow. The coil when arranged with one wheel at A, and a mercury contact at B, will revolve when a current is sent through it, becoming in this case a motor. If an iron chain, or an elastic band of iron such as a measuring tape, be placed inside the ring coil, it then becomes a



distorted Gramme ring, the wheels taking the place of the brushes, the way in which the current is produced being the same. If coils approaching N produce a current upwards, then those which are leaving N produce one downwards. The same takes place on the other side; coils leaving the S pole produce a current upwards, while those which approach it produce a current downwards; both of the ascending currents, being in the same direction, go to the wheel A, while both of the descending currents, being in an opposite direction, go to the wheel B.

The first coil made was of copper wire. Phosphor bronze wire answers better, being less easily distorted.

FREDERICK J. SMITH.

Trinity College, Oxford, November 12.

The Use of Rotifera.

CAN any of your readers inform me whether Rotifers are of any use in removing decaying organic matter, as Infusoria do?

C. L.

ON THE MECHANICAL CONDITIONS OF A SWARM OF METEORITES.[1]

I.

MR. LOCKYER writes in his interesting paper on meteorites[2] as follows:—

"The brighter lines in spiral nebulæ, and in those in which a rotation has been set up, are in all probability due to streams of meteorites with irregular motions out of the main streams, in which the collisions would be almost *nil.* It has already been suggested by Prof. G. Darwin (NATURE, vol. xxxi. p. 25)—using the gaseous hypothesis—that in such nebulæ 'the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines.'"

The whole of Mr. Lockyer's paper, and especially this passage in it, leads me to make a suggestion for the reconciliation of two apparently divergent theories of the origin of planetary systems.

The nebular hypothesis depends essentially on the idea that the primitive nebula is a rotating mass of fluid, which at successive epochs becomes unstable from excess of rotation, and sheds a ring from the equatorial region.

The researches of Roche[1] (apparently but little known in this country) have imparted to this theory a precision which was wanting in Laplace's original exposition, and have rendered the explanation of the origin of the planets more perfect.

But notwithstanding the high probability that some theory of the kind is true, the acceptance of the nebular hypothesis presents great difficulties.

Sir William Thomson long ago expressed to me his opinion that the most probable origin of the planets was through a gradual accretion of meteoric matter, and the researches of Mr. Lockyer afford actual evidence in favour of the abundancy of meteorites in space.

But the very essence of the nebular hypothesis is the conception of fluid pressure, since without it the idea of a figure of equilibrium becomes inapplicable. Now, at first sight, the meteoric condition of matter seems absolutely inconsistent with a fluid pressure exercised by one part of the system on another. We thus seem driven either to the absolute rejection of the nebular hypothesis, or to deny that the meteoric condition was the immediate antecedent of the sun and planets. M. Faye has taken the former course, and accepts as a necessary consequence the formulation of a succession of events quite different from that of the nebular hypothesis. I cannot myself find that his theory is an improvement on that of Laplace, except in regard to the adoption of meteorites, for he has lost the conception of the figure of equilibrium of a rotating mass of fluid.

The object of this paper is to point out that by a certain interpretation of the meteoric theory we may obtain a reconciliation of these two orders of ideas, and may hold that the origin of stellar and planetary systems is meteoric, whilst retaining the conception of fluid pressure.

According to the kinetic theory of gases, fluid pressure is the average result of the impacts of molecules. If we imagine the molecules magnified until of the size of meteorites, their impacts will still, on a coarser scale, give a quasi-fluid pressure. I suggest, then, that the fluid pressure essential to the nebular hypothesis is in fact the resultant of countless impacts of meteorites.

The problems of hydrodynamics could hardly be attacked with success, if we were forced to start from the beginning and to consider the cannonade of molecules. But when once satisfied that the kinetic theory will give us a gas, which, in a space containing some millions of molecules, obeys all the laws of an ideal non-molecular gas filling all space, we may put the molecules out of sight and treat the gas as a plenum.

In the same way the difficulty of tracing the impacts of meteorites in detail is insuperable, but if we can find that such impacts give rise to a quasi-fluid pressure on a large scale, we may be able to trace out many results by treating an ideal plenum. Laplace's hypothesis implies such a plenum, and it is here maintained that this plenum is merely the idealization of the impacts of meteorites.

As a bare suggestion, this view is worth but little, for its acceptance or rejection must turn entirely on numerical values, which can only be obtained by the consideration

[1] Abstract of a Paper read before the Royal Society on November 15 by Prof. G. H. Darwin, F.R.S.

[2] NATURE, November 17, 1887. The paper itself is in the Roy. Soc. Proc., November 15, 1887 (No. 259, p. 117).

[1] *Montpellier Acad. Sci. Mém.*

of some actual system. It is obvious that the solar system is the only one about which we have sufficient knowledge to afford a basis for discussion. The paper, of which this is an abstract, is accordingly devoted to a consideration of the mechanics of a swarm of meteorites, with special numerical application to the solar system.

When two meteoric stones meet with planetary velocity, the stress between them during impact must generally be such that the limits of true elasticity are exceeded, and it may be urged that a kinetic theory is inapplicable unless the colliding particles are highly elastic. It may, however, I think, be shown that the very greatness of the velocities will impart what virtually amounts to an elasticity of a high order of perfection.

It appears, *a priori*, probable that when two meteorites clash, a portion of the solid matter of each is volatilized, and Mr. Lockyer considers the spectroscopic evidence conclusive that it is so. There is no doubt enough energy liberated on impact to volatilize the whole of both bodies, but only a small portion of each stone will undergo this change. A numerical example is given in the paper to show the enormous amount of energy with which we are dealing. It must necessarily be obscure as to how a small mass of solid matter *can* take up a very large amount of energy in a small fraction of a second, but spectroscopic evidence seems to show that it does so; and if so, we have what is virtually a violent explosive introduced between the two stones.

In a direct collision each stone is probably shattered into fragments, like the splashes of lead when a bullet hits an iron target. But direct collision must be a comparatively rare event. In glancing collisions the velocity of neither body is wholly arrested, the concentration of energy is not so enormous (although probably still sufficient to effect volatilization), and since the stones rub past one another, more time is allowed for the matter round the point of contact to take up the energy; thus the whole process of collision is much more intelligible. The nearest terrestrial analogy is when a cannon-ball rebounds from the sea. In glancing collisions fracture will probably not be very frequent.

From these arguments it is probable that, when two meteorites meet, they attain an effective elasticity of a high order of perfection; but there is of course some loss of energy at each collision. It must, however, be admitted that on collision the deflection of path is rarely a very large angle. But a succession of glancing collisions would be capable of reversing the path, and thus the kinetic theory of meteorites may be taken as not differing materially from that of gases.

Perhaps the most serious difficulty in the whole theory arises from the fractures which must often occur. If they happen with great frequency, it would seem as if the whole swarm of meteorites would degrade into dust. We know, however, that meteorites of considerable size fall upon the earth, and, unless Mr. Lockyer has misinterpreted the spectroscopic evidence, the nebulæ do now consist of meteorites. Hence it would seem as if fracture was not of very frequent occurrence. It is easy to see that if two bodies meet with a given velocity the chance of fracture is much greater if they are large; and it is possible that the process of breaking up will go on only until a certain size, dependent on the velocity of agitation, is reached, and will then become comparatively unimportant.

When the volatilized gases cool they will condense into a metallic rain, and this may fuse with old meteorites whose surfaces are molten. A meteorite in that condition will certainly also pick up dust. Thus there are processes in action tending to counteract subdivision by fracture and volatilization. The mean size of meteorites probably depends on the balance between these opposite tendencies. If this is so, there will be some fractures, and some fusions, but the mean mass will change very slowly with the mean kinetic energy of agitation. This view is at any rate adopted in the paper as a working hypothesis. It was not, however, possible to take account of fracture and fusion in the mathematical investigation, but the meteorites are treated as being of invariable mass.

The velocity with which the meteorites move is derived from their fall from a great distance towards a centre of aggregation. In other words, the potential energy of their mutual attraction when widely dispersed becomes converted, at least partially, into kinetic energy. When the condensation of a swarm is just beginning, the mass of the aggregation towards which the meteorites fall is small, and thus the new bodies arrive at the aggregation with small velocity. Hence, initially, the kinetic energy is small, and the volume of the sphere within which hydrostatic ideas are (if anywhere) applicable is also small. As more and more meteorites fall in, that volume is enlarged, and the velocity with which they reach the aggregation is increased. Finally the supply of meteorites in that part of space begins to fail, and the imperfect elasticity of the colliding bodies brings about a gradual contraction of the swarm. I do not now attempt to trace the whole history of a swarm; but the object of the paper is to examine its mechanical condition at an epoch when the supply of meteorites from outside has ceased, and when the velocities of agitation and distribution of meteorites in space have arranged themselves into a sub-permanent condition, only affected by secular changes. This examination will enable us to understand, at least roughly, the secular change as the swarm contracts, and will throw light on other questions.

The foundation for the mathematical investigation in the paper is the hypothesis that a number of meteorites which were ultimately to coalesce, so as to form the sun and planets, have fallen together from a condition of wide dispersion, and form a swarm in which collisions are frequent.

For the sake of simplicity, the bodies are treated as spherical, and in the first instance as being of uniform size.

It is assumed provisionally that the kinetic theory of gases may be applied for the determination of the distribution of the meteorites in space. No account being taken of the rotation of the system, the meteorites will be arranged in concentric spherical layers of equal density of distribution, and the quasi-gas, whose molecules are meteorites, being compressible, the density will be greater towards the centre of the swarm. The elasticity of a gas depends on the kinetic energy of agitation of its molecules, and therefore in order to determine the law of density in the swarm we must know the distribution of kinetic energy of agitation.

It is assumed that when the system comes under our notice, uniformity of distribution of energy has been attained throughout a central sphere, which is surrounded by a layer of meteorites with that distribution of kinetic energy which, in a gas, corresponds to convective equilibrium, and with continuity of density and velocity of agitation at the sphere of separation. Since in a gas in convective equilibrium the law connecting pressure and density is that which holds when the gas is contained in a vessel impermeable to heat, such an arrangement of gas has been called by M. Ritter (*Annalen der Physik und Chemie*, vol. xvi., 1882, p. 166) an isothermal-adiabatic sphere, and the same term is adopted here as applicable to a swarm of meteorites. The justifiability of these assumptions will be considered later.

The first problem which presents itself, then, is the equilibrium of an isothermal sphere of gas under its own gravitation. The law of density is determined in the paper, but it will here suffice to remark that, if a given mass be inclosed in an envelope of given radius, there is a minimum temperature (or energy of agitation) at which isothermal equilibrium is possible. The minimum energy

of agitation is found to be such that the mean square of velocity of the meteorites is almost exactly $\frac{2}{3}$ of the square of the velocity of a satellite grazing the surface of the sphere in a circular orbit.

As indicated above, it is supposed that in the meteor-swarm the rigid envelope, bounding the isothermal sphere, is replaced by a layer or atmosphere in convective equilibrium. The law of density in the adiabatic layer is determined in the paper, and it appears that when the isothermal sphere has minimum temperature the mass of the adiabatic atmosphere is a minimum relatively to that of the isothermal sphere. Numerical calculation shows, in fact, that the isothermal sphere cannot amount in mass to more than 46 per cent. of the mass of the whole isothermal-adiabatic sphere, and that the limit of the adiabatic atmosphere is at a distance equal to 2·786 times the radius of the isothermal sphere.[1]

It is also proved that the total energy, existing in the form of energy of agitation, is exactly one-half of the potential energy lost in the concentration of the matter from a condition of infinite dispersion. This result is brought about by a continual transfer of energy from a molar to a molecular form, for a portion of the kinetic energy of a meteorite is constantly being transferred into the form of thermal energy in the volatilized gases generated on collision. The thermal energy is then lost by radiation.

It is impossible as yet to sum up all the considerations which go to justify the assumption of the isothermal-adiabatic arrangement, but it is clear that uniformity of kinetic energy must be principally brought about by a process of diffusion. It is therefore interesting to consider what amount of inequality in the kinetic energy would have to be smoothed away.

The arrangement of density in the isothermal-adiabatic sphere being given, it is easy to compute what the kinetic energy would be at any part of the swarm, if each meteorite fell from infinity to the neighbourhood where we find it, and there retained all the velocity due to such fall. The variation of the square of this velocity gives an indication of the amount of kinetic energy which has to be degraded by conversion into heat and distributed by diffusion, in the attainment of uniformity. This may be called "the theoretical value of the kinetic energy." It appears that in the swarm, this square of velocity rises from zero at the centre of the swarm to a maximum which is attained nearly half-way through the adiabatic layer, and then diminishes. It is found that the variations of this theoretical value are inconsiderable throughout the greater part of the range. From this it follows that there must be diffusion of kinetic energy from without inwards, and considerations of the same kind show that when a planet consolidates there must be a cooling of the middle strata both outwards and inwards.

We must now consider the nature of the criterion which determines whether the hydrostatic treatment of a meteor-swarm is permissible.

The hydrodynamical treatment of an ideal plenum of gas leads to the same result as the kinetic theory with regard to any phenomenon involving purely a mass, when that mass is a large multiple of the mass of a molecule; to any phenomenon involving purely a length, when the cube of that length contains a large number of molecules; and to any phenomenon involving purely a time, when that time is a large multiple of the mean interval between collisions. Again, any velocity to be justly deduced from hydrodynamical principles must be expressible as the edge of a cube containing many molecules passed over in a time containing many collisions of a single molecule; and a similar statement must hold of any other function of mass, length, and time.

Beyond these limits we must go back to the kinetic theory itself, and in using it care must be taken that enough molecules are considered at once to impart statistical constancy to their properties.

There are limits, then, to the hydrodynamical treatment of gases, and the like must hold of the parallel treatment of meteorites.

The principal question involved in the nebular hypothesis seems to be the stability of a rotating mass of gas; but unfortunately this has remained up to now an untouched field of mathematical research. We can only judge of probable results from the investigations which have been made concerning the stability of a rotating mass of liquid. Now it appears that the instability of a rotating mass of liquid first enters through the graver modes of gravitational oscillation. In the case of a rotating spheroid of revolution the gravest mode of oscillation is an elliptic deformation, and its period does not differ much from that of a satellite which revolves round the spheroid so as to graze its surface. Hence, assuming for the moment that a kinetic theory of liquids had been formulated, we should not be justified in applying the hydrodynamical method to this discussion of stability, unless the periodic time of such a satellite were a large multiple of the analogue of the mean free time of a molecule of liquid.

Carrying, then, this conclusion on to the kinetic theory of meteorites, it seems probable that hydrodynamical treatment must be inapplicable for the discussion of such a theory as the meteoric-nebular hypothesis, unless a similar relation holds good.

These considerations, although of a vague character, will afford a criterion of the applicability of hydrodynamics to the kind of problem suggested by the nebular hypothesis. And certain criteria suggested by this line of thought are found in the paper; they give a measure of the degree of curvature of the average path pursued by a meteorite between two collisions.

After these preliminary investigations, we have to consider what kind of meeting of two meteorites will amount to an "encounter" within the meaning of the kinetic theory.

Is it possible, in fact, that two meteorites can considerably bend their paths under the influence of gravitation, when they pass near one another? This question is considered in the paper, and it is shown that unless the bodies have the dimensions of small planets, the mutual gravitational influence is insensible. Hence, nothing short of absolute impact is to be considered an encounter in the kinetic theory, and what is called the radius of "the sphere of action" is simply the distance between the centres of a pair when they graze, and is therefore the sum of the radii of a pair, or, if of uniform size, the diameter of one of them.

(*To be continued.*)

SOME CURIOUS PROPERTIES OF METALS AND ALLOYS.[1]

THE lecture consisted mainly of experimental demonstrations of the changes induced in metals, either by slight variations in the treatment to which they are subjected or by rendering them impure by the addition of small quantities of metals or metalloids.

Prof. Austen began by pointing out that for centuries the early metallurgists investigated the action of exceedingly small quantities of matter upon masses of metal; and he said that, strange as it may seem, the promulgation, in 1803, of Dalton's atomic theory threw a flood of light upon chemical phenomena, but cast into the shade such investigations as those of Bergman, which dealt with influences

[1] This is one of the results established by M. Ritter in a series of papers in the *Annalen der Physik und Chemie* from 1878 onwards.

[1] Abstract of a Lecture delivered by Prof. W. Chandler Roberts-Austen, F.R.S., at the Royal Institution, on May 11, 1888.

of "traces" upon masses, and the authority of Berthollet was not sufficient to save them from neglect. In this eventful year for science, 1803, the latter published his essay on chemical statics, in which he stated, as a fundamental proposition, that in comparing the action of bodies on each other, which depends "upon their affinities and mutual proportions, the mass of each has to be considered" (English edition, by M Farrell, M.D., 1804, p. 5). His views were successfully contested by Proust, but, as Lothar Meyer says, the influence on chemistry of the rejection of Berthollet's views was remarkable:—"All phenomena which could not be attributed to fixed atomic proportions were set aside as not truly chemical, and were neglected. Thus chemists forsook the bridge by which Berthollet had sought to unite the sister sciences, physics and chemistry." Fortunately, however, in this country there was one chemist who had followed up the line of work indicated by the early metallurgists, for in 1803, the same year as that in which both Berthollet's essay and Dalton's atomic theory were published, Charles Hatchett (Phil. Trans., vol. xciii. p. 43, 1803) communicated to the Royal Society the results of a research which he had conducted, with the assistance of Cavendish, in order to ascertain "the chemical effects produced on gold by different metallic substances when employed in certain" (often very small) "proportions as alloys."

Allusion was then made to the evidence of the passage of metals into allotropic states, and it was shown that, although the importance of the isomeric and allotropic states was abundantly recognized in organic chemistry, it had been much neglected in the case of metals. Special attention was then devoted to the works of Joule and Lyon Playfair, who showed, in 1846, that metals in different allotropic states possessed different atomic volumes, and the lecturer then proceeded to the consideration of the work of Matthiessen, who, in 1860, was led to the view that in certain cases when metals were alloyed they passed into allotropic states, probably the most important generalization which has as yet been made in connection with the molecular constitution of alloys.

Instances of allotropy in pure metals were then shown to the audience, such, for example, as Bolley's lead, which oxidizes readily in air; Schützenberger's copper; Fritsche's tin, which fell to powder when exposed to an exceptionally cold winter; Gore's antimony; Graham's palladium; and allotropic nickel. It was further shown that metals could be obtained in chemically active states under the following conditions:—Joule proved that when iron is released from its amalgam by distilling away the mercury the metallic iron takes fire on exposure to air, and is therefore clearly different from ordinary iron, and is, in fact, an allotropic form of iron. Moissan (*Comptes rendus*, vol. lxxxviii. p. 180, 1879) has shown that similar effects are produced in the case of chromium and manganese, cobalt, and nickel, when released from their amalgams with mercury.

Evidence is not wanting of allotropy in metals released from solid alloys, as well as from fluid amalgams with mercury. Certain alloys may be viewed as solidified solutions, and when such bodies are treated with a suitable solvent, usually an acid, it often happens that one constituent metal is dissolved, and the other released in an insoluble form. Reference was then made to a new alloy of potassium and gold, containing about 10 per cent. of the precious metal. If a fragment of this alloy be thrown upon water, the potassium takes fire, decomposes the water, and the gold is released as a black powder: there is a form of this black or dark-brown gold which appears to be an allotropic modification of gold, as it combines with water to form auric hydride. If this dark gold be heated to dull redness, it readily assumes the ordinary golden colour. The Japanese use this gold, released from gold-copper alloys, in a remarkable way, for they produce, by the aid of certain pickling solutions, a beautiful patina on copper which contains only 2 per cent. of gold, while even a trace of the latter metal is sufficient to alter the tint of the patina.

With regard to theoretical views as to molecular change in metals, special care was given to a description of the work of Prof. W. Spring, of Liége, who had furnished much evidence in support of the view that polymerization of metals—that is, the rearrangement of atoms in their molecules—could take place even in *solid* alloys of lead and tin.

With reference to the passage of metals into allotropic states under slight external influences, it was stated that Debray (*Comptes rendus*, vol. xc. p. 1195, 1880) has given a case of an alloy in which a simple elevation of temperature induces allotropic change in the constituent metals. It is prepared as follows: 95 parts of zinc are alloyed by fusion with 5 parts of rhodium, and the alloy is treated with hydrochloric acid, which dissolves away the bulk of the zinc, leaving a rich rhodium-zinc alloy, containing about 80 per cent. of rhodium. When this alloy is heated *in vacuo* to a temperature of 400° C., a slight explosion takes place, but no gas is evolved, and the alloy is then insoluble in *aqua regia*, which dissolved it readily before the elevation of temperature caused it to change its state. We are thus presented (as the experiment shown to the audience proved) with another undoubted case of isomerism in alloys, the unstable, soluble modification of the alloy being capable of passing into the insoluble form by a comparatively slight elevation of temperature.

The industrial importance of the passage of metals and alloys into allotropic states, and the possibility of changing the mechanical properties of metals by apparently slight influences, were fully dealt with; and the lecture concluded with a detailed description of Prof. Austen's own experiments, which have since been printed in the Philosophical Transactions of the Royal Society, the results showing that very small amounts of metallic impurities exert an extraordinary effect on the tenacity and extensibility of gold, and that small as the amounts of these impurities are, their influence is rigidly controlled by the periodic law of Newlands and Mendelejeff, the deleterious action of a metallic impurity being in direct relation to its atomic volume. The audience was asked "to remember that the knowledge of the kind of facts which had been considered comes to us from very early times, for the influence produced on metals by small quantities of added matter had a remarkable effect on the development of chemistry, mainly by sustaining the belief of the early chemists in the possibility of ennobling a base metal so as to transmute it into gold. This was the object to which they devoted life and health, and laboured with fast and vigil. We inherit the results of their labours, and their prayers have been answered in a way they little anticipated, for, from an industrial point of view, if not from a scientific one, metals are 'transmuted' by traces of impurity. Possibly we are nearing an explanation of the causes which are at work, but the fact remains that iron may be changed from a plastic material, which in ornament can be fashioned into the most dainty lines of flow, into one of great endurance, to which, for the present at least, the defence of the country may be trusted, apparently because armour-plates and missiles owe their respective qualities to the fact that carbon, manganese, and chromium have small atomic volumes."

THE LEONID METEOR-SHOWER, 1888.

AT Bristol rain fell heavily between midday on November 12 and the same time on November 13, a 5-inch gauge registering an inch and eight-tenths, which is by far the greatest downpour of the year within twenty-four hours. In the afternoon of November 13 the clouds broke, and the weather showed a disposition to become more favourable. At night the sky was moderately clear

at times. I began to watch for the return of the Leonids at 15h. 30m., but there were many clouds until about 16h., when the firmament cleared and remained nearly cloudless until 18h. During the $2\frac{1}{2}$ hours I counted 29 shooting-stars, and of these 17 were Leonids; so that the shower was pretty conspicuous. The radiant-point was at 149° + 22°, which is almost identical with the place assigned to it in previous years.

The display was noteworthy for the brilliancy of the individual meteors. I observed a large Leonid fireball at 16h. 56m., and I also saw two Leonids equal to Jupiter and one first magnitude at other times. Their paths were as follow:—

	h. m.			°	°	°	°
Nov. 13 ...	16 41 ...	♃	...	166	+ 34	to 172	+ $37\frac{1}{4}$
	16 56 ...	Fireball	...	$125\frac{1}{2}$	− 22	to 121	− $30\frac{1}{2}$
	17 19 ...	♃	...	252	+ $33\frac{1}{2}$	to $257\frac{1}{2}$	+ 31
	17 46 ...	1	...	175	+ $66\frac{1}{2}$	to 210	+ $75\frac{1}{2}$

All these left bright streaks enduring for several seconds. The fireball at 16h. 56m. was situated very low in the southern sky, and I should not have seen it but for its brilliant flashes, which lit up the whole sky with unusual vividness. It must have been a splendid object in the English Channel and at stations in the north-west parts of France. The fine meteor at 17h. 19m. appears to have been seen also by Mr. Backhouse, at Sunderland. At Bristol it was observed near ϵ Herculis, and very close on the north-east horizon; at Sunderland it was projected near Arcturus, about 15° above the east-by-north horizon, and the most remarkable feature of its display at Sunderland was the durableness of the streak, which continued visible to the naked eye *nine minutes* after the flash which accompanied the outburst of the meteor. A comparison of the observations shows that this bolide at its disappearance was about 37 miles in vertical height above point in the North Sea near lat. $55\frac{1}{4}$° N., long. 3° E.

The recent shower appears to have been decidedly brighter than any other return of the same stream since 1879, when there occurred a very similar display to that which has just been witnessed. There was a large proportion of fine meteors from the Leonid radiant on November 13 of that year, and this seems a characteristic element of the shower.

Mr. Backhouse obtained observations at Sunderland which amply confirm those at Bristol during the apparition of 1888. He says the Leonids were "unusually numerous" on November 13. Between 16h. 48m. and 18h. 14m. he noticed about 14 of them, including one fireball, one equal to Jupiter, two equal to Sirius, and one equal to a first magnitude star. The streaks of the larger meteors belonging to this system are very pronounced in their aspect, and they sometimes brighten up considerably a few seconds after the nuclei have died away.

It is probable that this well-known group will become increasingly active every year until its maximum in 1899.

Bristol, November 17. W. F. Denning.

NOTES.

We understand that Prof. Ray Lankester is a candidate for the post of Deputy-Professor of Human and Comparative Anatomy in the University of Oxford, which has just been created in consequence of the continued illness of Prof. Moseley.

Prof. Giard's lectures at the Sorbonne begin to-day. He is to deal with the general proofs of the doctrine of evolution.

It is expected that the meeting of the International Congress of Anthropology and Prehistoric Archæology, at Paris, in 1889, will be a great success. The Congress was founded at Spezzia in 1865, and held its first session at Neuchâtel in 1866. Many members have already announced their intention of being present at the Paris meeting.

At the ordinary meeting of the Institution of Civil Engineers, on Tuesday, November 13, the President, Sir George B. Bruce, announced that Lord Armstrong, C.B., Past-President, in the belief that the Benevolent Fund attached to the Institution was not adequately supported by the general body of members, and that it needed support, had most generously offered to contribute any sum not exceeding £5000, provided the members among them would raise a similar amount.

At a recent meeting of the Edinburgh International Exhibition Association, the joint Committee appointed by the Association and the executive to consider the best way of disposing of the Exhibition surplus submitted its report. The Committee, among various other proposals, recommended that £1000 should be given to the Scottish Meteorological Society in aid of the Ben Nevis Observatory, and £100 to the Royal Scottish Geographical Society.

A resolute effort is about to be made to secure for North London (comprising the boroughs of Finsbury, Hackney, Islington, and St. Pancras) four great technical institutes. The Central Executive Committee has issued a circular, pointing out the need for these institutions, and appealing for funds. It is supposed that if £200,000 can be raised by private subscriptions a like sum will be contributed by the Charity Commissioners.

The next course of Christmas lectures adapted to a juvenile auditory will be given, at the Royal Institution, by Prof. Dewar, F.R.S., the subject being "Clouds and Cloudland." They will begin on December 27. During the recess the staircases leading from the gallery of the theatre have been considerably altered in order to facilitate more speedy egress.

We have already called attention to the fact that an effort is being made to secure for Bedford College (for ladies), York Place, Baker Street, London, new chemical and physical laboratories. The College has been among the most successful of the institutions which send up graduates to the science degrees at the University of London; but its students have hitherto been severely handicapped by the inadequacy of the laboratory accommodation. The opportunity has occurred of securing fresh building-ground adjoining the College, and plans have been prepared for the proposed additional laboratories. The offer must be definitely made before Christmas, and the estimated expenditure will be at least £3000. The Council do not feel warranted in going forward unless they have at least a considerable portion of this sum promised, for which they are entirely dependent on voluntary contributions. Subscribers' names will be received by the lady-resident at the College. An application for assistance is also being made to the City Companies. Bedford College is the only institution exclusively for ladies which provides first-class practical instruction of this kind.

The work of the United States Fish Commission during the past season seems to have been even more than usually successful. Stations capable of hatching four hundred million codfish-eggs have been established at Gloucester and Wood's Holl; and, according to *Science*, these stations will probably put at least one-fourth of that number of cod-fry into the Atlantic Ocean during the present season. It is expected, therefore, that codfish will soon be more plentiful on the coast of New England than they were years ago, and that a lost industry worth millions of dollars will be restored to that part of the United States.

The Committee on Science and the Arts, of the Franklin Institute, of Pennsylvania, wishes to make known the fact that it is empowered to award, or to recommend the award of, certain medals for meritorious discoveries and inventions which tend to the progress of the arts and manufactures. These medals are:—(1) The Elliott Cresson Medal (gold), founded by the legacy

of Elliott Cresson, of Philadelphia, and conveyed to the Trustees of the Franklin Institute. By the Act of the Institution, May 17, 1849, the Committee on Science and the Arts was designated and empowered to award this medal, and the Committee decided to grant it, after proper investigation and report by a sub-committee, either for some discovery in the arts and sciences, or for the invention or improvement of some useful machine, or for some new process, or combination of materials in manufactures, or for ingenuity, skill, or perfection in workmanship. (2) The John Scott Legacy Premium and Medal (twenty dollars and a medal of copper), founded in 1816, by John Scott, a merchant of Edinburgh, Scotland, who bequeathed to the city of Philadelphia a considerable sum of money, the interest of which should be devoted to rewarding ingenious men and women who make useful inventions. Upon request made to the Secretary of the Franklin Institute, full information will be sent respecting the manner of making application for the investigation of inventions and discoveries; furthermore, the Committee on Science and the Arts will receive and give respectful consideration to reports upon discoveries and inventions which may be sent to it with the view of receiving one or the other of the above-named awards. Full directions as to the manner and form in which such communications should be made, will be sent on application.

THE late Prof. Edward Tuckerman made a choice collection of books and papers relating to lichens, some four hundred numbers in all. These works have been presented by Mrs. Tuckerman, in accordance with her husband's wish, to Amherst College Library; and it is proposed to keep the collection by itself under the name of the "Tuckerman Memorial Library," and to render it worthy of the name by making it as complete as possible in its own department. Some persons interested in this specialty may like to assist in maintaining and completing the collection, on the understanding that it shall always be available to public use. Mr. Wm. I. Fletcher, the Librarian of Amherst College, has therefore issued a notice to the effect that he will be glad to receive contributions, either in money or in material (especially rare monographs that may have escaped Prof. Tuckerman's notice), to this memorial to an eminent scholar and man of science. Whatever money may be contributed will be kept as a fund of which only the income will be employed in making additions to the collection, or in repairs and rebinding. The sum of 1000 dollars would probably suffice as such a fund.

AT the meeting of the Scientific Committee of the Royal Horticultural Society on November 13, Mr. Henslow showed specimens of several species of plants which are propagated by cleistogamous flower-buds. By that means, while retaining a dwarf habit, they are able to multiply very rapidly, and to extend over considerable areas in a tennis-lawn. Although none of them are perennials, they remain so reduced in size that they are not exterminated by the mowing-machine periodically cutting them down. The result is that each species has more or less completely covered certain patches of ground, to the almost entire exclusion of everything else. The plants in question are *Cerastium glomeratum*, *Montia fontana*, *Trifolium procumbens*, *Sagina procumbens*, *Alchemilla arvensis*, *Veronica arvensis*, and *Poa annua*. Mr. Henslow added that he had observed, many years ago, *Trifolium subterranean* flourishing in the same way on the close-cut grass in Kew Gardens, on the site of the present rockery.

EARTHQUAKES occurred at Vyernyi on October 30, at 6 a.m.; at Digne, in the Department of the Lower Alps, on November 1, at 1.36 a.m.; in Sikkim on the 9th inst.; and in various places in California on the 19th inst.

ON the night of Saturday, November 3, about 8 o'clock, the tens of thousands of sheep folded in the large sheep-breeding districts north, east, and west of Reading were taken with a sudden panic, jumping their hurdles, escaping from the fields, and running hither and thither. Early on Sunday morning the shepherds found their animals under hedges and in the roads, panting as if they had been terror-stricken. Messrs. Oakshott and Millard, who write to us on the subject, suggest that a slight earthquake may have been the cause of this strange incident.

THE third general meeting of the Italian Meteorological Society was held at Venice, from September 14 to 21, in the Institute of Music of Benedetto Marcello. Reports of the various papers read are being published in the *Bollettino Mensuale* of the Society. There were discussions on various questions connected with general meteorology and climatology; meteorology applied to hygiene, and hydrology; and geodynamics.

A SUMMARY of the results of two interesting papers read by General Strachey at the recent meeting of the British Association on the meteorology of the Red Sea and Cape Guardafui, prepared from data collected by the Meteorological Office, is contained in the Proceedings of the Royal Geographical Society for November. In 1882 the Admiralty called attention to the precautions necessary in rounding Cape Guardafui from the southward, wrecks having frequently occurred in foggy weather and at night-time, owing to vessels being out of their reckoning from the great variation in the drift of the currents. The paper shows that, although not always infallible, the change of sea temperature in approaching the land is of great assistance in determining the position when near this dangerous coast. The late Captain Symington called attention to this important fact in 1880.

THE Meteorological Office of the Argentine Republic, which was established at Cordoba by Dr. Gould fourteen years ago, and to which we are chiefly indebted for our knowledge of the climate of this vast region, continues to follow the same useful lines under its present Director, Mr. G. G. Davis. Volume vi. of the *Anales*, recently published, contains exhaustive discussions on the climate of five places, from as long a series of observations as was available in each case. The observers are supplied with verified instruments, and the stations are regularly inspected. Several new stations have been established in the more remote districts.

MOST ornithologists have hitherto been of opinion that birds during their yearly migrations never cross the Caucasus chain of mountains, and that on their way towards the north, and on their return, they take the route *viâ* the shores of the Black Sea or of the Caspian. It appears, however, from recent observations made by M. Rossikoff, and communicated to the *Caucasus* newspaper, that such is not the case. On October 9, while on the banks of the Aragva, close by the Ananur station, he saw a number of large birds on the summit of the Tsikhes-dziri Mountain, and they proved to be a flock of some 100 to 150 white storks (*Ciconia alba*), which were crossing the Caucasus chain on their return journey. It appeared also from information gathered from the inhabitants that cranes, herns, and ducks have been often seen in that part of the main chain on their migrations.

TWO new crystalline compounds of arsenious oxide with sodium bromide and sodium iodide have been prepared by Prof. Rüdorff, of Charlottenburg. They form two additional members of an interesting isomorphous group discovered two years ago by Prof. Rüdorff, having the general formula $MX + 2As_2O_3$, where M may be potassium, sodium, or ammonium, and X represents chlorine, bromine, or iodine. In his former memoir, published in 1886, the following members of the series are described, and accurate analyses given:—$KI + 2As_2O_3$, $KBr + 2As_2O_3$, $KCl + 2As_2O_3$, $NH_4I + 2As_2O_3$, and $NH_4Br + 2As_2O_3$. These five salts are beautifully isomorphous,

forming hexagonal prisms with rhombohedral terminations, almost undistinguishable from each other on account of their extreme similarity. The two new compounds just announced in the current *Berichte* are the sodium compounds $NaBr + 2As_2O_3$ and $NaI + 2As_2O_3$. The former was obtained by dissolving 20 grammes arsenious oxide and 120 grammes sodium bromide in 350 c.c. boiling water, filtering the somewhat turbid liquid thus obtained, and allowing the clear solution to cool very slowly. On suspending in it a plate of cold glass, crystals of the compound were immediately deposited upon it, which proved on microscopic examination to consist of the hexagonal prisms common to the whole series. The crystals were then washed upon the large glass plate with cold water, dried superficially between filter-paper, afterwards at 130°, and submitted to analysis. The numbers obtained indicate the formula given above. The crystals, as in the case of many other double compounds, are decomposed into their constituents by contact with warm water. The second compound, $NaI + 2As_2O_3$, was similarly obtained, using 60 grammes sodium iodide, 22 grammes arsenious oxide, and dissolving them in half a litre of hot water. On allowing a glass plate to stand twenty-four hours in the solution, it was found to be covered with six-sided prisms, apparently identical in shape with those of the other compounds of the series previously obtained. It is interesting that both these compounds can be readily obtained by the addition of a sufficient quantity of bromide or iodide of sodium to a solution of sodium arsenite. The corresponding compound with chloride of sodium has not yet been prepared. Thus another series of analogous salts is added to the list of isomorphous groups, the number of which is steadily increasing. These groups are of especial interest to the crystallographer, inasmuch as they appear likely to be of invaluable assistance in our attempts to grasp the fundamental problem, "Why does a definite chemical compound crystallize in any particular system?"

THE County of Middlesex Natural History and Science Society has issued its Transactions during the session 1887–88. The volume includes, besides an abstract of an introductory address by Dr. Archibald Geikie, papers on subjects of scientific interest by Prof. Flower, Mr. W. Lant Carpenter, Dr. Sydney T. Klein, and others.

THE new number of the Journal of the Anthropological Institute (vol. xviii. No. 2) is one of great interest. Mr. Galton offers some suggestive remarks on replies by teachers to questions respecting mental fatigue, and Mr. J. Venn has a careful paper on "Cambridge Anthropometry." In a paper on the races of the Babylonian Empire, Mr. G. Bertin sets forth some conclusions which are so different from, and even opposed to, "accepted notions," that he hesitated a long time, he says, "before surrendering himself to the evidence."

DR. H. TEN KATE, in a letter to *Science*, expresses dissent from the opinions lately set forth by Dr. Daniel G. Brinton, as to "the alleged Mongolian affinities of the American race." Dr. Brinton holds that there are no such affinities, whereas to his opponent it seems obvious that the native race of America cannot be properly understood without reference to the Mongolians. Summing up his views on the subject, Dr. H. Ten Kate says:—"I wish to say that Dr. Brinton's argumentation against the affinity between Americans and Mongolians is based upon entirely wrong reasoning. If the reasons he gives were correct, then the classification of the other races of the human species would be equally wrong; for in each of them peoples are grouped together, which, although related by physical characteristics, are linguistically and ethnologically entirely different from each other, not to speak of the difference in their psychological and social evolution. When I admit that the native Americans are Mongoloids, I do not necessarily imply that America has been populated from Asia or elsewhere. However, if we accept the theory of evolution, this is the most probable explanation of the observed facts. But, leaving the doubtful origin of the Americans, and of their languages and arts, out of the question, I maintain that there is a physical similarity, racial affinity, and relationship between the indigenous Americans and the Mongolians in the widest sense. This is, in the present state of anthropological knowledge, an undeniable fact. He who denies it does not believe in physical anthropology; and not to recognize this branch of science is equal to denying natural history in general."

IN the last Administration Report of the Island of Dominica, which has just been laid before Parliament, the President estimates the original Carib population at about three hundred, or something under that figure. The children are healthy, and the numbers of Caribs, according to their own accounts, are not decreasing, but they are gradually becoming so intermixed with the Negroes that the pure Carib, "Franc Carib," will soon be non-existent. The Carib quarter, a reserve of large extent on the Windward side of Dominica, is wholly occupied by them. They are very peaceable and retiring; they live on vegetables and fruits, which they cultivate, and on fish. They keep their roads in good order. Their children appear to be very intelligent, but there is no school in the Carib quarter, though the people are very anxious to have one.

A SOCIETY has been formed in France, under the patronage of many men of high scientific and political standing, for the purpose of developing a proper system of physical education. The leader of the new movement is M. Philippe Daryl, *alias* Paschal Grousset, who, when a very young man, took an active part in the Paris insurrection of 1871. He afterwards lived in England, whence he sent interesting letters to the *Temps*.

THE *Madras Mail* says that Dr. Thurston, the Superintendent of the Madras Museum, has been requested by the Board of Revenue to visit various electrical establishments in Europe in order to select an electric globe light to shine in twenty fathoms of water. Such a light has long been wanted at the pearl fisheries, for, up to the present, the work of the fisheries has been confined to banks situated at a less depth than twenty fathoms.

THE next *conversazione* of the Royal Microscopical Society will be held on Wednesday, the 28th instant, at 8 o'clock.

THE additions to the Zoological Society's Gardens during the past week include a —— Monkey (*Cercopithecus* sp. inc.) from South Africa, presented by Mr. L. B. Lewis; an Indian Antelope (*Antilope cervicapra* ♂) from India, presented by Mr. J. W. Shand-Harvey; a Himalayan Bear (*Ursus tibetanus* ♀) from Northern India, presented by the Regiment of the 2nd Life Guards; an Axis Deer (*Cervus axis*) from India, presented by Lady Donaldson; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mrs. Wood; two American Bitterns (*Botaurus lentiginosus*) from North America, presented by Mr. J. B. Williams; a Peregrine Falcon (*Falco peregrinus*), captured at sea, presented by Mr. R. H. Armstrong; a Spotted Salamander (*Salamander maculosa*), European, presented by Mr. F. E. B. Roper, F.Z.S.; two Swans (*Cygnus olor* ♀ ♀), European, a Royal Python (*Python regius*) from West Africa, deposited; two Golden Plovers (*Charadrius pluviatilis*), British, purchased.

OUR ASTRONOMICAL COLUMN.

THE BRAZILIAN TRANSIT OF VENUS EXPEDITIONS, 1882.—We have received vol. iii. of the "Annaes do Imperial Observatorio do Rio de Janeiro," containing the reports of the

Brazilian expeditions to observe the transit of Venus of December 1882. The volume, which is a handsome quarto of some 750 pages, and is fully illustrated with photographs of the various stations and instruments, contains reports from three stations, the weather at the Imperial Observatory at Rio de Janeiro itself, which should have been a fourth station, having proved cloudy and wet. It had been at first intended to send an expedition to Cuba, but as the French astronomers were to occupy a position there, the little Island of St. Thomas, belonging to Denmark, was chosen instead. St. Thomas paired well with the southern station, Punta Arenas, in Patagonia, for the duration was much shortened at the former place and slightly lengthened at the latter, the sun being high at both stations, and ingress and egress at both taking place nearly symmetrically with regard to the meridian. The entire transit was also seen from the remaining station, Olinda, near Pernambuco, where ingress was somewhat retarded and egress much accelerated. The observations were all made by the method of projection, in order that the disturbing effects of irradiation might be got rid of as far as possible. The St. Thomas expedition which was under the command of Baron de Teffé, possessed three equatorials, and Dr. H. Draper had promised to supply a photo-heliograph, but his lamented death prevented the carrying out of his generous intention. The Olinda expedition, commanded by M. J. de Oliveira Lacaille, had two equatorials; whilst M. Cruls, the chief of the Punta Arenas party, had but one; the largest telescope in each case being 6¼ inches in aperture. M. Cruls selected a site for his party within a mile of that occupied by Dr. Auwers with the German expedition; for the Brazilian Parliament having delayed the necessary credit for the expedition to the last moment, the expedition did not arrive at the place until late, and it seemed better to take advantage of the German choice of position rather than lose time by surveying for a fresh site at a distance. The observations at each of these three stations were successful, the second internal contact being observed at all, but the first internal contact was lost at St. Thomas. The method of chords could not, therefore, be employed, but the combination of the second contacts of the two northern stations with both contacts of the southern gave 8″·808 as the resulting parallax. A large part of the volume is devoted to a report of the voyage of the corvette *Parnahyba*, by Captain L. Saldanha de Gama, the captain who conveyed the southern observing party to their station, and to a description of the natural history of Tierra del Fuego.

The Tail of Comet 1887 *a* (Thome).—Prof. Bredichin has discussed in the *Bulletin* of the Imperial Society of Naturalists of Moscow, 1888, Nos. 2 and 3, the observations of the direction of the tail of this comet. The comet was discovered by Mr. Thome, of Cordoba, on January 18, 1887, and it was remarkable for the smallness of its perihelion distance, the complete absence of any nuclear condensation in the head, and the length, straightness, and narrowness of the tail. Prof. Bredichin finds that the tail manifestly belongs to his third type, viz. those in which the repulsive force, $1-\mu$, does not exceed 0·1. He suggests, as the rate of outflow in comets of short perihelion distance is much more rapid at perihelion passage than later, and as the comet was not discovered until a week after perihelion, that the lighter materials may already have been driven off and reduced to such a degree of tenuity as to be invisible, leaving only substances of heavy atomic weight. As is well known, he associates his first type of tail, that in which the repulsive force is greatest, with hydrogen, the more ordinary second type with the hydrocarbons; and he suggests in the case of the present comet that elements with atomic weights like those of gold, mercury, and lead, would furnish a tail of the character observed. Some comets, however, which do not approach the sun closely, have tails only of the third type. If, then, Prof. Bredichin's explanation is to be received in its entirety, hydrogen and hydrocarbons are not always constituents of cometary tails.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 NOVEMBER 25—DECEMBER 1.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 25

Sun rises, 7h. 38m.; souths, 11h. 47m. 19·0s.; sets. 15h. 57m.: right asc. on meridian, 16h. 6·7m.; decl. 20° 54′ S. Sidereal Time at Sunset, 20h. 17m.

Moon (at Last Quarter November 26, 17h.) rises, 21h. 1m.*, souths 4h. 56m.; sets, 12h. 40m.: right asc. on meridian, 9h. 14·7m.; decl. 18° 7′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	5 53 ...	10 38 ...	15 23 ...	14 57·0 ...	14 59 S.
Venus.....	10 37 ...	14 18 ...	17 59 ...	18 37·4 ...	25 2 S.
Mars......	11 30 ...	15 29 ...	19 28 ...	19 49·1 ...	22 32 S.
Jupiter....	8 32 ...	12 33 ...	16 34 ...	16 52·1 ...	22 6 S.
Saturn....	21 48*...	5 14 ...	12 40 ...	9 32·2 ...	15 37 N.
Uranus ...	3 31 ...	8 57 ...	14 23 ...	13 16·0 ...	7 23 S.
Neptune..	15 50 ...	23 35 ...	7 20*...	3 56·2 ...	18 39 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
26 ...	37 Leonis	... 6 ...	7 25	near approach	178 —
28 ...	B.A.C. 3996	... 6 ...	1 44	near approach	301 —
30 ...	B.A.C. 4572	... 6 ...	5 59	near approach	124 —

Nov.	h.	
25 ...	13 ...	Saturn in conjunction with and 1° 31′ south of the Moon.
Dec.		
1 ...	11 ...	Saturn stationary.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′			h. m.
U Cephei	0 52·4 ...	81 16 N. ...	Nov.	25,	0 27 *m*
			,,	30,	0 6 *m*
S Arietis	2 9·8 ...	24 32 N. ...	Dec.	1,	*m*
λ Tauri...	3 54·5 ...	12 10 N. ...	Nov.	26,	20 44 *m*
			,,	30,	19 36 *m*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,,	29,	2 0 *m*
R Canis Majoris...	7 14·5 ...	16 12 N. ...	,,	25,	0 41 *m*
			,,	26,	3 57 *m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	,,	27,	*m*
U Hydræ	10 32·0 ...	12 48 S. ...	,,	26,	*m*
S Leonis	11 5·1 ...	6 4 N. ...	,,	30,	*M*
R Scuti	18 41·5 ...	5 50 S. ...	,,	29,	*M*
η Aquilæ	19 46·8 ...	0 43 N. ...	,,	29,	18 0 *m*
T Aquarii	20 44·0 ...	5 34 S. ...	,,	27,	*m*
T Vulpeculæ ...	20 46·7 ...	27 50 N. ...	,,	29,	21 0 *M*
			,,	30,	22 0 *m*
Y Cygni	20 47·6 ...	34 14 N. ...	,,	25,	2 18 *m*
			,,	28,	2 12 *m*
δ Cephei	22 25·0 ...	57 51 N. ...	,,	25,	4 0 *M*
			,,	28,	21 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

The most interesting periodical shower of the week is that of the *Andromedes*, the stream connected with Biela's comet, but no remarkable display can be expected from it this year: max. Nov. 27; radiant about R.A. 25°, Decl. 44° N. Other showers of the week are as follow:—

	R.A.	Decl.	
Near λ Persei	60° ...	50 N. ...	Very swift.
,, β Canum Venaticorum	190 ...	42 N. ...	Swift; streaks.

GEOGRAPHICAL NOTES.

Two letters relating to Dr. Nansen's expedition across Greenland have been published—one from Dr. Nansen himself to Mr. Augustin Gamel, Copenhagen, who is defraying the expenses of the expedition; the other from Mr. Sverdrup, one of Dr. Nansen's companions, to his father. The letters were sent forward from Godthaab by two *kajak*-men, who delivered them to the captain of the *Fox*, at Ivigtut. The following is a translation of Dr. Nansen's letter:—

Godthaab, *October* 4.

I have at last the great joy to report to you that Greenland has been successfully crossed from east to west. I regret that the very short time left to me before despatching my messengers will not permit any detailed account. I can just jot down a few words to be forwarded by the *kajak*-men. I am sending southwards in the hope of stopping the *Fox* at Ivigtut, and getting her to wait for us and take us home this autumn. But

in case the *kajak*-messenger should catch the steamer without inducing her to wait for us, I write these few lines just to inform you that we are all alive and well.

As you will know, we left the *Jason*, the Norwegian sealer, on July 17, and expected to reach the shore the next day. But in this we were sadly disappointed. Screwing ice, maëlstroms, impassable ice, where it was alike impossible to row or to drag the two boats, stopped us. One of the boats was stove in, but we got it repaired again. We drifted seawards at a speed of thirty sea miles in the twenty-four hours. Drifted in the ice for twelve days. Strove hard to get to the shore, were three times on the point of succeeding, but were as often carried out to sea again by a current stronger than our power of rowing. Were once, for a whole day and night, very near perishing in tremendous breakers of the sea against the ice-rim. After twelve days' drifting about, we managed at last to get ashore near Andretok, north of Cape Farewell, at 61° and some minutes of northern latitude. We rowed again northwards, reaching Uminik, from which point the crossing of the inland ice began on August 15.

We directed our course for Christianshaab on the western coast. Encountered severe snowstorms and had heavy ground. Estimating that it would be too late to reach Christianshaab in time for this autumn's vessel, we altered our course and steered for Godthaab, the ice-fields in that direction having, besides, been hitherto trodden by no one. After altering course, reached height of 10,000 feet, with temperature of 40° to 50° C. below zero. For several weeks we remained at an altitude of over 9000 feet. Tremendous storms, loose, new-fallen snow, enormously difficult passage. Towards end of September we reached at last the western side above Godthaab. Had a perilous descent, on ugly and very uneven ice, but got safely down to Ameralik Fjord. Managed to build a kind of boat from floor of tent, bags, bamboo reeds, and willow branches. In that frail craft Sverdrup and I rowed away, and arrived here on October 3. The four men are left at Ameralik, living there on short rations fare, but will be sent for as soon as possible.

There you have in short outline our Saga. We are all perfectly well, and everything has been in the best order. I hope that we may catch this steamer, and that instead of this letter you may see our sunburnt faces.

With many greetings, yours ever devotedly,

FRITHIOF NANSEN.

P.S.—Just now the *kajak*-men must leave, profiting by the favourable weather. They have 300 miles to make before getting to Ivigtut.

The following is a translation of Mr. Sverdrup's letter :—

GODTHAAB, *October* 4, 1888.

We arrived safely here yesterday after forty-six days' wandering from east to west. It did not prove so easy to get on shore from the *Jason* as we had expected. We got into formidable ice-screwings, and the current took us southwards and out from the shore, so that we had twelve days' very hard work before getting to land, and that 300 miles more to the south than we had intended. We began at once to work back along the coast, and this took us another twelve days, so that we did not begin our crossing of the ice before August 15. The ascent was very fortunate, as we chanced to find comparatively easy ice to climb up. We shaped our course for Christianshaab, but after getting up to 7500 feet we were attacked by a northern snowstorm. We resolved, therefore, to set our course for Godthaab, the distance being shorter, and there being a better chance of favourable winds. I may truly say that we had a hard time of it. The snow and ice were very heavy, and the weather was trying. For nearly three weeks we were up at nearly 10,000 feet, and had a temperature of −40° to −50° C. Only for four days were we snowbound. After all, we have to be thankful it was not worse. After getting down from the inland ice on the western coast, we had before us some ninety miles of barren country, of which the half lay along a fjord. We tried to cross here, but found it too hard work ; so we managed to construct a kind of boat from the bottom of the tent and some bags, and in that, after four days' rowing, Dr. Nansen and I reached here, where we had the most cordial reception from all the inhabitants of the colony. Two boats have now been sent to the bottom of the fjord to fetch our comrades. The regular vessel has long since left, but some 250 or 300 miles further south there is supposed to be, at Ivigtut, a steamer loading for Copenhagen, and we are now sending a *kajak*-message in order to stop that steamer if possible. We have but little hope of that, however, and are preparing to pass the winter here. That may be very comfortable after all, but of course we would prefer getting home. I must hurry up, as we are now going to dine with the parson, and, in fact, we have not had time for anything, as since arriving here we have gone from one social party to another. You may see from that how well we are off. I was the only one of our whole party who got over all the tremendous fatigues without the smallest ailment. I am, and have been all the time, as fresh and sound as a fish.

DR. GEORG SCHWEINFURTH has started upon an Oriental journey. He is going to Arabia first, to continue his studies of the coffee-plant.

THE FOUNDATION-STONES OF THE EARTH'S CRUST.[1]

DO we know anything about the earth in the beginning of its history—anything of those rock masses on which, as on foundation-stones, the great superstructure of the fossiliferous strata must rest? Palæontologists by their patient industry have deciphered many of the inscriptions, blurred and battered though they be, in which the story of life is engraved on the great stone book of Nature. Of its beginnings, indeed, we cannot yet speak. The first lines of the record are at present wanting—perhaps never will be recovered. But apart from this—before the grass, and herb, and tree, before the "moving creature in the water," before the "beast of the earth after his kind,"—there was a land and there was a sea. Do we know anything of that globe, as yet void of life? Will the rocks themselves give us any aid in interpreting the cryptogram which shrouds its history, or must we reply that there is neither voice nor language, and thus accept with blind submission, or spurn with no less blind incredulity, the conclusions of the physicist and the chemist?

The secret of the earth's hot youth has doubtless been well kept. So well that we have often been tempted to guess idly rather than to labour patiently. Nevertheless we are beginning, as I believe, to feel firm ground after long walking through a region of quicksands; we are laying hold of principles of interpretation, the relative value of which we cannot in all cases as yet fully apprehend—principles which occasionally even appear to be in conflict, but which will some day lead us to the truth.

I shall not attempt to give you an historical summary, but only to lay before you certain facts for which I can answer, and to indicate the inductions which these, as it seems to me, warrant. If I say little of the work of others, it is not from a desire of taking credit to myself, but because it is immaterial for my present purpose who first made a particular observation and how far his inductions therefrom were correct. The acknowledgment of good work would involve repudiation of bad, and for that, so far as persons are concerned, it seems hardly fair to use the present occasion. So, in the outset of this lecture, I will once for all make a statement which I have sometimes thought of invariably using, like a prefatory invocation, "You are free to suppose that everything herein has been said by somebody, somewhere," but I will add that, as far as possible, every assertion has been personally verified.

The name Cambrian has been given to the oldest rocks in which fossils have been found. This group forms the first chapter in the first volume, called Palæozoic, of the history of living creatures. Any older rocks are provisionally termed Archæan. These—I speak at present of those indubitably underlying the Cambrian—exhibit marked differences one from another. Some are certainly the detritus of other, and often of older, materials—slates and grits, volcanic dust and ashes, even lava-flows. Such rocks differ but little from the basement-beds of the Cambrian; probably they are not much older, comparatively speaking. But in some places we find, in a like position, rocks as to the origin of which it is more difficult to decide. Often in their general aspect they resemble sedimentary deposits, but they seldom retain any distinct indications of their original fragmental constituents. They have been metamor-

[1] An evening discourse, delivered at the Bath meeting of the British Association, by Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., &c

phosed, the old structures have been obliterated, new minerals have been developed, and these exhibit that peculiar orientation, that rudely parallel arrangement, which is called foliation. Except for this some masses are fairly homogeneous, while some exhibit a distinct mineral banding which is usually parallel with the other structure. These rocks are the gneisses and schists—the latter term, often vaguely used, I always restrict to rocks which exhibit a true foliation. In some schists the mineral constituents are comparatively minute, in others they are of considerable size. In the former case we may often venture to affirm that the rock is a metamorphosed sediment; in the latter its original condition is a matter of conjecture. Rocks of the former class often appear, to use no stronger word, to lie above, and so to be less ancient than those of the latter, and beneath that comes a coarser and more massive series, in which granitoid rocks are common. In these last foliation is often inconspicuous, and the rocks in consequence are not markedly fissile.

That these rocks are older than the Cambrian can often be demonstrated. Sometimes it can even be proved that their present distinctive character had been assumed before the overlying Cambrian rocks were deposited. Such rocks, then, we may confidently bring forward as types of the earth's foundation-stones. As the inscriptions buried in the Euphrates Valley tell us the tongue of Accad in the days prior to the coming of the Semite, so these declare what then constituted the earth's crust. If in such rocks we find any peculiarities of mineral composition or structure, these may legitimately be regarded as distinctive. We have only to beware of mistaking for original those which are secondary and subsequently impressed.

In other parts of the world we find rocks of like characters with those above named, the age of which cannot be so precisely fixed, though we can prove them to be totally disconnected from and much older than the earliest overlying stratum. To assert that these rocks are contemporary with the others is obviously an hypothesis which rests on the assumption that community of structure has some relation to similarity of origin. I am well aware that attempts have been made to discredit this. But if we eliminate difficulties which are merely sophistical—those, I mean, created by the use of ambiguous or misleading terms—if we acknowledge those due to our limited means of investigation, such as that of distinguishing a rock crushed *in situ* from one composed of transported fragments—in other words, of separating in every case a superinduced from a primary structure, and if we allow for others due to the limitation of our instrumental and visual powers, I do not hesitate, as the result of long and, I hope, careful work, to assert that certain structures are very closely related to the past history of a rock, and that in very many instances our diagnosis of the cause from its effect is not less worthy of confidence than that of an expert in pathology or physiology. Resemblances of structures, different in origin, do, no doubt, sometimes occur—resemblances not seldom due to partial correspondence in the environments; but in regard to these it is our duty to labour patiently till we succeed in distinguishing them. The difficulty of the task does not justify us, either in abandoning it in despair, or in sitting down, after a few hasty observations, to fashion hypotheses which have no better foundation than our own incompetence or idleness.

As it is impossible in the time at my disposal to demonstrate the proposition, I must assume what I believe few, if any, competent workers will deny, that certain structures are distinctive of rocks which have solidified from a state of fusion under this or that environment; others are distinctive of sedimentary rocks; others again, whatever may be their significance, belong to rocks of the so-called metamorphic group. I shall restrict myself to indicating, by comparison with rock structures of which the history is known, what inferences may be drawn as to the history of the last-named rocks, which, as I have already stated, are in some cases examples of the earth's foundation-stones, while in others, if they are not these, they are at any rate excellent imitations.

Let us proceed tentatively. I will put the problem before you, and we will try to feel our way towards a solution. Our initial difficulty is to find examples of the oldest rocks in which the original structures are still unmodified. Commonly they are like palimpsests, where the primitive character can only be discerned, at best faintly, under the more recent inscription. Here, then, is one of the best which I possess—a Laurentian gneiss from Canada. Its structure is characteristic of the whole group; the crystals of mica or hornblende are well defined, and commonly have a more or less parallel arrangement; here and there are bands in which these minerals are more abundant than elsewhere. The quartz and the felspar are granular in form; the boundaries of these minerals are not rectilinear, but curved, wavy, or lobate; small grains of the one sometimes appear to be inclosed in larger grains of the other. Though the structure of this rock has a superficial resemblance to that of a granite of similar coarseness, it differs from it in this respect, as we can see from the next instance, a true granite, where the rectilinear outline of the felspar is conspicuous. Here, then, is one of our problems. This difference of structure is too general to be without significance. What does it mean?

It is more difficult to obtain examples of schist of like geological age, wholly free from subsequent modification. Apparently the structure and composition of the rock have rendered it more liable to disturbance. But those exhibited, though by no means perfect examples, may serve to indicate the structure of an Archæan schist, consisting mainly of quartz and mica. We may take them as representative of a considerable series of rocks, which are often associated in such a manner as to suggest that, notwithstanding their present crystalline condition, they had a sedimentary origin. Can this inference be justified?

How shall we attack this problem? Clearly, the most hopeful way is by proceeding from the known to the unknown. Now, among the agents of change familiar to geologists, three are admittedly of great importance; these are water, heat, and pressure. As probably almost all changes in nature, with which we have to deal, have occurred in the presence of water, but those due to it alone are generally superficial, I shall assume its presence, and not attempt to isolate its effects. But we must endeavour to ascertain the results of pressure and heat, when acting singly and in combination, in modifying rocks of a known character; admitting, however, that probably while the one agent has been dominant, the other has not been wholly inoperative.[1]

The first effect of pressure due to great earth-movements is to flatten somewhat the larger fragments in rocks, and to produce in those of finer grain the structure called cleavage. This, however, is a modification mainly mechanical. It consists in a re-arrangement of the constituent particles, mineral changes, so far as they occur, being quite subordinate. But in certain extreme instances the latter are also conspicuous. From the fine mud, generally the result of the disintegration of felspar, a mica, usually colourless, has been produced, which occurs in tiny flakes, often less than one-hundredth of an inch long. In this process, a certain amount of silica has been liberated, which sometimes augments pre-existing granules of quartz, sometimes consolidates independently as microcrystalline quartz. Carbonaceous and ferruginous constituents are respectively converted into particles of graphite and of iron oxide. Here is an example of a Palæozoic rock, thus modified. It originally consisted of layers of black mud and gray silt. In the former, this filmy mica has been abundantly developed; it is present also, as we might expect, to some extent in the latter. Observe that the original banded structure, notwithstanding the pressure, has not been obliterated. Another point also demands notice. The black lines in the section indicate the direction of the cleavage of the rock, which is, roughly speaking, at right angles to the pressure which has most conspicuously affected the district, while the microfoliation, as we may call it, appears to be parallel to the original bedding, and is thus anterior to the dominant cleavage. The two may form parts of a connected series of movements, but, at any rate, they are so far separated that the pressure which produced the one, acted, roughly speaking, at right angles to that which gave rise to the other, and the folia were developed before they were bent and torn.

Let us now pass on to examine the effects of pressure when it acts upon a rock already crystalline. Here, obviously it is comparatively unimportant whether the original rock was a true granite or a granitoid gneiss; for at present we are only concerned with the effect of pressure on a fairly granular crystalline rock. But in the resultant structures there are, as it seems to me, differences which are dependent upon the mode in which pressure has acted. They are divisible into two groups: one indicating the result of simple direct crushing, the other of crushing accompanied by shearing. In the former case, the rock

[1] Heat will, of course, result from the crushing of rock. This some consider an important factor in metamorphism, but I have never been able to find good evidence in favour of it, and believe that as a rule the rocks yield too slowly to produce any great elevation of temperature.

mass has been so situated that any appreciable lateral movement has been impossible; it has yielded like a block in a crushing-machine. In the latter, a differential lateral movement of the particles has been possible, and it has prevailed when (as in the case of an overthrust fault) the whole mass has not only suffered compression, but also has travelled slowly forward. Obviously, the two cases cannot be sharply divided, for the crushing up of a non-homogeneous rock may render some local shearing possible. Still it is important to separate them in our minds, and we shall find that in many cases the structure, as a whole, like the cleavage of a slate, results from a direct crush; while in others the effects of shearing predominate. The latter accordingly exhibit phenomena resembling the effects of a tensile stress. Materials of a like character assume a more or less linear arrangement, the rock becomes slightly banded, and exhibits, as has been said, a kind of fluxion structure. This phrase, if we are careful to guard ourselves against misconception, is far from inappropriate. The mass gradually assumes a fragmental condition under the pressure, and its particles as they shear and slide under the effects of thrust, behave to some extent like those of a non-uniform mass of rock in a plastic condition, as, for example, a slaggy glass. But we must be on our guard, lest we press the analogy too far. The interesting experiments which have been made on the flow of solids, and on rolled-out plastic substances, while valuable as illustrations, represent, as it seems to me, a condition of things which must be of rare occurrence in a rock mass, pulverized by mechanical forces only. If I am to reason from them, I must regard the rock not as a fragmental solid—if the phrase be permissible—but as an imperfect fluid; that is to say, I must consider them as illustrative of structures in rocks which have yet to assume—not have already assumed—a crystalline condition.

Illustrations of the effects of direct crushing in a granitoid rock are common in the Alps. Those of a shearing crush are magnificently developed near the great overthrust faults in the north-west Highlands of Scotland.

In the former case, where a granitoid rock has been affected only to a moderate extent, and the resulting rock in a hand specimen would be called a gneiss without any very definite mineral banding, we find that under the microscope it exhibits a fragmental structure, the felspars are often somewhat rounded in outline, are frequently rather decomposed and speckled with minute flakes of white mica of secondary origin, and commonly seem to "tail off" into a sort of stream of microlithic mica, which has doubtless resulted from the destruction of felspar, the residual silica making its appearance as minutely crystalline quartz. The original quartz grains have been broken up, and are now represented by smaller grains, often in rudely lenticular aggregates, like little "inliers" of quartzite. The original flakes of black mica have been tattered and torn, and now appear as streaky clusters of flakelets, often less than one-sixth the original length. In extreme cases of crushing, the felspar has almost disappeared; the constituents are all reduced in size, and the rock at first sight would no longer be called a gneiss, but a fine-grained mica-schist. It has become extremely fissile, and the flat faces of the fragments exhibit a peculiar sheen, as if it had received a varnish of microlithic mica. In short, from a granitoid rock a microcrystalline mica-schist has been produced, which, however, differs markedly from the rock to which that name is ordinarily applied.

Let us now turn to a rock of similar nature, in which the effect of shearing is more conspicuous. I have selected a specimen, in which, as in the first example above, some of the felspar still remains in recognizable fragments. These, however, are commonly destitute of the "tail" of mica-microliths, and bear, at first sight, some resemblance to the broken porphyritic felspars which occur in a rhyolite. The mica, whether primary, but fragmental, or secondary, tends to get associated in undulating layers; the quartz also has a more uniform aspect and a more linear arrangement. In the most extreme cases the felspar all but disappears (though I fancy that it has here a better chance of surviving), the quartz and the mica are more and more aggregated in definite but thin bands, and the former, when viewed with crossing nicols, exhibits streaks, which, for a considerable distance, are almost uniform in tint, as if its molecules under a stress definite in direction had acquired a polarity, so that groups of these act upon light almost like a single crystal.

The effects of mechanical deformation, followed by mineral change, are also remarkably conspicuous in the case of pyroxenic rocks. Augite, it is well known, is by no means a stable mineral, and under certain circumstances is readily transformed into hornblende. This occurs in more than one way without mechanical action, but of these I do not now speak. Only of late years, however, has it been known that pressure can convert a dolerite into a hornblende-schist. Of this, through the kindness of Mr. Teall, who first proved the occurrence of this alteration in Great Britain, I can show you an example. The rock, as you see, has lost the structures of a dolerite, and has assumed those characteristic of many hornblende-schists. I say of many, because, though the rock is distinctly foliated, it does not exhibit a conspicuous mineral banding. My own observations confirm those of Mr. Teall, though I have never been so fortunate as to obtain, as he did, a complete demonstration of the passage from the one rock to the other.

It seems, then, to be demonstrated that, by mechanical deformation, accompanied or followed by molecular re-arrangement, foliated rocks, such as certain gneisses and certain schists, can be produced from rocks originally crystalline. But obviously there are limits to the amount of change. The old proverb, "You cannot make a silk purse of a sow's ear," holds good in this case also. To get certain results, you must have begun with rocks of a certain character. So that it is often possible, as I believe, to infer not only the nature of the change, but also that of the original rock. Hitherto we have been dealing with rocks which were approximately uniform in character, though composed of diverse materials—that is, with rocks more or less granular in aspect. Suppose, now, the original rock to have already acquired a definite structure—suppose it had assumed, never mind how, a distinct mineral banding, the layers varying in thickness from a small fraction of an inch upwards. Would this structure survive the mechanical deformation? I can give an answer which will at any rate carry us a certain way. I can prove that subsequent pressure has frequently failed to obliterate an earlier banded structure. In such a district as the Alps we commonly find banded gneisses and banded schists, which have been exposed to great pressure. Exactly as in the former case, the new divisional planes are indicated by a coating of films of mica, by which the fissility of the rock in this direction is increased. The mass has assumed a cleavage-foliation. I give it this name because it is due to the same cause as ordinary cleavage, but is accompanied by mineral change along the planes of division; while I term the older structure stratification-foliation, because so frequently, if it has not been determined by a stratification of the original constituents, it is at any rate a most extraordinary imitation of such an arrangement. In many cases the new structure is parallel with the old, but in others, as in the "strain-slip" cleavage of a phyllite, the newer can be seen distinctly cutting across the older mineral banding. As an example, take a rock mainly consisting of quartz and mica. Sometimes there has been a certain amount of crushing of the constituents, followed by a re-crystallization of the quartz and the formation of a pale-coloured mica. Sometimes, when the direction of the disturbance has been at right angles to the stratification-foliation, the latter is made wavy, and the mica-flakes are twisted round at right angles to their original position. Sometimes there has been a dragging or shearing of the mass, so that a considerable amount of mica has been re-crystallized along the new planes of division. To put it briefly, I assert, as the result of examining numbers of specimens, that though in certain cases the new structure is dominant, a practised eye seldom fails to detect traces of the older foliation, while in a large number of instances it is still as definite as the stripe in a slate.

We have got, then, thus far, that pressure acting on rocks previously crystallized can produce a foliation; but when it has acted in Palæozoic or later times, the resulting structures can be identified, and these, as a rule, are distinguishable from those of the most ancient foliated rocks, while at present we have found no proof that pressure alone can produce any conspicuous mineral banding. I am aware that this statement will be disputed, but I venture to plead, as one excuse for my temerity, that probably few persons in Great Britain have seen more of crystalline rocks, both in the field and with the microscope, than myself. So, while I do not deny the possibility of a well-banded rock being due to pressure alone, I unhesitatingly affirm that this at present is a mere hypothesis—an hypothesis, moreover, which is attended by serious difficulties. For, if we concede that, in the case of many rocks originally granular, dynamic metamorphism has produced a mineral banding, this is only on a very small scale: the layers are but a small fraction of an inch thick. No one

could for a moment confuse a sheared granite from the Highlands with a Laurentian gneiss from Canada or with an uninjured Hebridean gneiss. For the former to attain to the condition of the latter, the mass must have been brought to a condition which admitted of great freedom of motion amongst the particles, almost as much, in short, as among those of a molten rock. Clearly, the dynamic metamorphism of Palæozoic or later ages appears to require some supplementary agency. Can we obtain any clue to it?

An explanation of broadly-banded structures was long since suggested, and has recently been urged with additional force, which avoids some of our difficulties. We know that the process of consolidation in a coarsely crystalline rock has often been a slow one; the constituent minerals separate gradually from the magma, of which sometimes so little may remain, that a rock with a true glassy base has been mistaken for one holocrystalline. The residual and still unsolidified magma would admit of a slow flowing of the mass, but there would be so little of it that the crystals already individualized, though altered in position by differential movements, would be affected by strains, and liable to fracture. Such a rock, when finally consolidated, would exhibit many phenomena in common with a rock modified by dynamic metamorphism, but would differ in the greater coarseness of its structure. This may prove to be the correct explanation of the curious foliated and banded gabbros in the Lizard district. That some crystalline rocks must have passed through this stage I am now in a position to affirm, from evidence not yet published.

Let us, however, see whether another line of investigation may not throw some light on our difficulty. I have already mentioned the effect produced by the intrusion of large masses of igneous rocks upon other rocks. These may be either igneous rocks already solidified, or sedimentary rocks. The former may be passed over, as they will not materially help us. In regard to the latter, the results of contact-metamorphism, as it is called, are, as we might expect, very various. Speaking only of the more extreme, we find that sandstones are converted into quartzites; limestones become coarsely crystalline, all traces of organisms disappearing, and crystalline silicates being formed. In clayey rocks all signs of the original sediments disappear, crystalline silicates are formed, such as mica (especially brown), garnet, andalusite, and sometimes tourmaline; felspar, however, is very rare. Fair-sized grains of quartz appear, either by enlargement of original granules or by independent crystallization of the residual silica. It is further important to notice that, as we approach the surface of the intrusive mass—that is, as we enter upon the region where the highest temperature has been longest maintained—the secondary minerals attain a larger size and are more free from adventitious substances—that is, they have not been obliged as they formed to incorporate pre-existing constituents. The rock, indeed, has not been melted down, but it has attained a condition where a rather free molecular movement became possible, and a new mineral in crystallizing could, as it were, elbow out of the way the more refractory particles. I can, perhaps, best bring home to you the result of contact-metamorphism by showing you what its effects are on a rock similar to that which I exhibited in illustration of the effect of pressure-metamorphism on a distinctly stratified rock. These are, in brief, to consolidate the rock, and while causing some constituents to vanish, to increase greatly the size of all the others. It follows, then, that mineral segregation is promoted by the maintenance for some time of a high temperature, which is almost a truism. I may add to this that, though rocks modified by contact-metamorphism differ from the Archæan schists, we find in them the best imitations of stratification-foliation, and of other structures characteristic of the latter.

One other group of facts requires notice before we proceed to draw our inferences from the preceding. Very commonly, when a stratified mass rests upon considerably older rocks, the lower part of the former is full of fragments of the latter. Let us restrict ourselves to basement beds of the Cambrian and Ordovician—the first two chapters in the stone-book of life. What can we learn from the material of their pages? They tell us that granitoid rocks, crystalline schists of various kinds, as well as quartzites and phyllites, then abounded in the world. The Torridon sandstone of Scotland proves that much of the subjacent Hebridean had even then acquired its present characteristics. The Cambrian rocks of North and South Wales repeat the story, notably near Llynfaelog in Anglesey, where the adjacent gneissoid rocks from which the pebbles were derived, even if once true granites, had assumed their differences before the end of the Cambrian period. By the same time similar changes had affected the crystalline rocks of the Malverns and parts of Shropshire. It would be easy to quote other instances, but these may suffice. I will only add that the frequent abundance of slightly-altered rocks in these conglomerates and grits appears significant. Such rocks seem to have been more widely distributed—less local—than they have been in later periods. Another curious piece of evidence points the same way. In North America, as is well known, there is an ancient group of rocks to which Sir W. Logan gave the name Huronian, because it was most typically developed in the vicinity of Lake Huron. Gradually great confusion arose as to what this term really designated. But now, thanks to our fellow-workers on the other side of the Atlantic, the fogs, gendered in the laboratory, are being dispelled by the light of microscopic research and the fresh air of the field. We now know that the Huronian group in no case consists of very highly-altered rocks, though some of its members are rather more changed than is usual with the British Cambrians, than which they are supposed to be slightly older. Conglomerates are not rare in the Huronian. Some of these consist of granitoid fragments in a quartzose matrix. We cannot doubt that the rock was once a pebbly sandstone. Still the matrix, when examined with the microscope, differs from any Palæozoic sandstone or quartzite that I have yet seen. Among grains of quartz and felspar are scattered numerous flakes of mica, brown or white. The form of these is so regular that I conclude they have been developed, or at least completed, *in situ*. Moreover, the quartz and the felspar no longer retain the distinctly fragmental character usual in a Palæozoic grit, but appear to have received secondary enlargement. A rock of fragmental origin to some extent has simulated or reverted to a truly crystalline structure. In regard to the larger fragments we can affirm that they were once granitoid rock, but in them also we note incipient changes such as the development of quartz and mica from felspar (without any indication of pressure), and there is reason to think that these changes were anterior to the formation of the pebbles.

To sum up the evidence. In the oldest gneissoid rocks we find structures different from those of granite, but bearing some resemblance to, though on a larger scale than, the structures of vein-granites or the surfaces of larger masses when intrusive in sedimentary deposits. We find that pressure alone does not produce structures like these in crystalline rocks, and that when it gives rise to mineral banding this is only on a comparatively minute scale. We find that pressures acting upon ordinary sediments in Palæozoic or later times do not produce more than colourable imitations of crystalline schists. We find that when they act upon the latter the result differs, and is generally distinguishable from stratification-foliation. We see that elevation of temperature obviously facilitates changes, and promotes coarseness of structure. We see also that the rocks in a crystalline series which appear to occupy the highest position seem to be the least metamorphosed, and present the strongest resemblance to stratified rocks. Lastly, we see that mineral change appears to have taken place more readily in the later Archæan times than it ever did afterwards. It seems, then, a legitimate induction that in Archæan times conditions favourable to mineral change and molecular movement—in short, to metamorphism—were general, which in later ages have become rare and local, so that, as a rule, these gneisses and schists represent the foundation-stones of the earth's crust.

On the other side what evidence can be offered? In the first place, any number of vague or rash assertions. So many of these have already come to an untimely end, and I have spent so much time and money in attending their executions, that I do not mean to trouble about any more till its advocates express themselves willing to let the question stand or fall on that issue. Next, the statement of some of the ablest men among the founders of our science, that foliation is more nearly connected with cleavage than with structures suggestive of stratification. In regard to this I have already admitted, in the case of the more coarsely crystalline rocks, what is practically identical with their claim, for they also assert that when the banding was produced, very free movement of the constituents was possible; and in regard to the rest I must ask whether they were speaking of cleavage-foliation or stratification-foliation, which had not then been distinguished, and I know in some instances what the answer will be. The third objection is of a general nature. To

prevent the possibility of misstatement I will give it as a quotation:—"To a geologist (especially one belonging to the school of Lyell) it is equally difficult to conceive that there should be a broad distinction between the metamorphic rocks of Archæan and post-Archæan age respectively, as that the pre-Tertiary volcanic rocks should be altogether different in character from those of Tertiary and recent times." Of course in this statement much depends on the sense attached to the epithet "broad." As an abstract proposition I should admit, as a matter of course, that from similar causes similar consequences would always follow. But in the latter part of the quotation lurks a *petitio principii*. During the periods mentioned volcanic rocks appear, as we should expect, to have been ejected from beneath the earth's crust similar in composition and condition, and to have solidified with identical environment. Hence the results, allowing for secondary changes, should still be similar. But to assume that the environment of a rock in early Archæan times was identical with that of similar material at a much later period is to beg the whole question. My creed, also, is the uniformitarian; but this does not bind me to follow a formula into a position which is untenable. Other studies with which I have some familiarity have warned me that a blind orthodoxy is one of the best guides to heresy. "The weakness and the logical defect of uniformitarianism"—these are Prof. Huxley's words—"is a refusal, or at least a reluctance, to look beyond the 'present order of things,' and the being content for all time to regard the oldest fossiliferous rocks as the *Ultima Thule* of our science." Now, speaking for myself, I see no evidence since the time of these rocks, as at present known, of any very material difference in the condition of things on the earth's surface. The relations of sea and land, the climate of regions, have been altered; but because I decline to revel in extemporized catastrophes, and because I believe that in Nature order has prevailed and law has ruled, am I therefore to stop my inquiries where life is no longer found, and we seem approaching the firstfruits of the creative power? Because palæontology is, perforce, silent; because the geologist can only say, "I know no more," must I close my ear to those who would turn the light of other sciences upon the dark places of our own, and meet their reasoning with the exclamation, "This is not written in the book of uniformity"? To do this would be to imitate the silversmiths of old, and silence the teacher by the cry, "Great is Diana of the Ephesians."

What, then, does the physicist tell us was the initial condition of this globe? I will not go into the vexed question of geological time, though as a geologist I must say that we have reason to complain of Sir W. Thomson. Years ago he reduced our credit at the bank of time to a hundred millions of years. We grumbled, but submitted, and endeavoured to diminish our drafts. Now he has suddenly put up the shutters, and declared a dividend of less than four shillings in the pound. I trust some aggrieved shareholder will prosecute the manager. However, as a *cause célèbre* is too long a business for the end of an evening, I will merely say that, while personally I see little hope of arriving at a chronological scale for the age of this earth, I do not believe in its eternity. What, then, does the physicist tell us must have been in the beginning? I pass by those earliest ages, when, as "Ilion, like a mist, rose into towers," so from the glowing cloud the great globe was formed. I pass on to a condition more readily apprehended by our faculties—the time, the *consistentior status* of Leibnitz, when the molten globe had crusted over, and its present history began. Rigid uniformitarian though you may be, you cannot deny that when the very surface of the ground was at a temperature of at least 1000° F., there was no rain, save of glowing ashes—no river, save of molten fire. Now is ending a long history with which the uniformitarian must not reckon—of a time when many compounds now existing were not dissolved but dissociated, for combination under that environment was impossible. Yet there was still law and still order—nay, the present law and order may be said even then to have had a potential existence—nevertheless to the uniformitarian gnome, had such there been, every new combination of elements would have been a new shock to his faith, a new miracle in the earth's history. But at the times mentioned above, though oxygen and hydrogen could combine, water could not yet rest upon the ruddy crust of the globe. What does that mean? This, that assuming the water of the ocean equivalent to a spherical shell of the earth's radius and two miles thick, the very lava-stream would consolidate under a pressure of about 310 atmospheres, equivalent to nearly 4000 feet of average rock.[1] But on the practical bearing of this consideration I will not dwell. Let us pass on to a time which, according to Sir W. Thomson, would rather quickly arrive, when the surface of the crust had cooled by radiation to its present temperature. Let us, merely for illustration, take a surface temperature of 50° F. (nearly that of London), and assume that the present rise of crust temperature is 1° F. for every 50 feet of descent, which is rather too rapid. If so, 212° F. is reached at 8100 feet, and 250° F. at 10,000 feet. Though the latter temperature is far from high, yet we should expect that under such a pressure chemical changes would occur with much more facility than at the surface. But many Palæozoic or even later rock masses can now be examined which at a former period of their history have been buried beneath at least 10,000 feet of sediment; yet the alteration of their constituents has been small: only the more unstable minerals have been somewhat modified, the more refractory are unaffected. But for a limited period after the *consistentior status*, the increase of crust temperature in descending would be far more rapid; when one-twenty-fifth of the whole period from that epoch to the present had elapsed, and this is no inconsiderable fraction, the rate of increase would be 1° for every 10 feet of descent. Suppose, for the sake of comparison, the surface temperature as before, the boiling-point of water would be reached at 1620 feet, and at 10,000 feet, instead of a temperature of 250° F., we should have one of 1050° F. But at the latter temperature many rock masses would not be perfectly solid.[2] According to Sorby, the steam cavities in the Ponza trachyte must have formed, and thus the rock have been still plastic at so low a temperature as 680° F. At this period, then, the end of the fourth year of the geological century, whatever be its units, structural changes in igneous and chemical changes in sedimentary rocks must have occurred more readily than in any much later period in the world's history. A temperature of 2000° F., sufficient to melt silver—more than sufficient to melt many lavas—would have been reached at a depth of about 4 miles. It would now be necessary to descend for at least 20 miles in order to arrive at this zone. It, during the ninety-six years of the century, has been changing its position in the earth's crust, more slowly as time went on, from the one level to the other.

There is another consideration, too complicated for full discussion, too uncertain, perhaps, in its numerical results to be more than mentioned at present, which, however, seems to me important. It is this, that in very early times, as shown by Prof. Darwin and Mr. Davison, the zone in the earth's crust, at which lateral thrust ceases and tension begins, must have been situated much nearer to the surface than at present. If now, at the end of the century, it is at the depth of 5 miles, it was, at the end of the fourth year, at a depth of only 1 mile. Then, a mass of rock, 10,000 feet below the surface, would be nearly a mile deep in the zone of tension. Possibly this may explain the mineral banding of much of our older granitoid rock, already mentioned, and the coincidence of foliation with what appears to be stratification in the later Archæan schists, as well as the certainly common coincidence of microfoliation with bedding in the oldest indubitable sediments.

Pressure, no doubt, has always been a most important factor in the metamorphism of rocks; but there is, I think, at present some danger in over-estimating this, and representing a partial statement of truth as the whole truth. Geology, like many human beings, suffered from convulsions in its infancy; now, in its later years, I apprehend an attack of pressure on the brain.

The first deposits on the solidified crust of the earth would obviously be igneous. As water condensed, denudation would begin, and stratified deposits, mechanical and chemical, become possible, in addition to detrital volcanic material. But at that time the crust itself, and even stratified deposits, would often be kept for a considerable period at a temperature similar to that afterwards produced by the invasion of an intrusive mass. Thus not only rocks of igneous origin (including volcanic ashes) would predominate in the lowest foundation-stones, but also secondary changes would occur more readily, and even the sediments or precipitates should be greatly metamorphosed. Strains set up by a falling temperature would produce, in masses still plastic, banded structures, which, under the peculiar circumstances,

[1] If we take the specific gravity of water as unity, and that of mean rock as 2·7, the pressure would be = 3911·1 feet of rock.

[2] The lowest temperature, which, so far as I know, has been observed in lava (basic) while still plastic, is 1228° F.

might occur in rocks now coarsely crystalline. As time went on, true sediments would predominate over extravasated materials, and these would be less and less affected by chemical changes, and would more and more retain their original character. Thus we should expect that as we retraced the earth's course through "the corridor of time," we should arrive at rocks which, though crystalline in structure, were evidently in great part sedimentary in origin, and should beyond them find rocks of more coarsely-crystalline texture and more dubious character, which, however, probably were in part of a like origin; and should at last reach coarsely-crystalline rocks, in which, while occasional sediments would be possible, the majority were originally igneous, though modified at a very early period of their history. This corresponds with what we find in Nature, when we apply, cautiously and tentatively, the principles of interpretation which guide us in stratigraphical geology.

I have stated as briefly as possible what I believe to be facts. I have endeavoured to treat these in accordance with the principles of inductive reasoning. I have deliberately abstained from invoking the aid of "deluges of water, floods of fire, boiling oceans, caustic rains, or acid-laden atmospheres," not because I hold it impossible that these can have occurred, but because I think this epoch in the earth's history so remote and so unlike those which followed, that it is wiser to pass it by for the present. But unless we deny that any rocks formed anterior to or coæval with the first beginning of life on the globe can be preserved to the present time, or, at least, be capable of identification (an assumption which seems to me gratuitous and unphilosophical) then I do not see how we can avoid the conclusion to which we are led by a study of the foundation-stones of the earth's crust—namely, that these were formed under conditions and modified by environments which, during later geological epochs, must have been of very exceptional occurrence. If, then, this conclusion accords with the results at which students of chemistry and students of physics have independently arrived, I do not think that we are justified in refusing to accept them, because they lack the attractive brilliancy of this or that hypothesis, or do not accord with the words in which a principle, sound in its essence, has been formulated. It is true in science, as in a yet more sacred thing, that "the letter killeth, the spirit giveth life."

SYSTEMATIC RELATIONS OF PLATYPSYLLUS AS DETERMINED BY THE LARVA.

PROF. C. V. RILEY, in a paper read at a recent meeting of the National Academy of Science (U.S.A.), drew attention to the unique character of *Platypsyllus castoris*, a parasite of the beaver; and gave an epitome of the literature on the subject, showing how the insect had puzzled systematists, and had been placed by high authority among the Coleoptera and the Mallophaga, and made the type even of a new order. He showed the value, as at once settling the question of its true position, of a knowledge of the adolescent states. He had had since November 1886 some 14 specimens of the larva, obtained from a beaver near West Point, Nebraska, and had recently been led to study his material at the instance of Dr. Geo. H. Horn, of Philadelphia, who at a recent meeting of the Entomological Society of Washington announced the discovery of the larva by one of his correspondents the present spring, and will publish a full description of it. Prof. Riley indicated the undoubted Coleopterological characteristics of the insect in the imago state, laying stress on the large scutellum and five-jointed tarsi, which at once remove it from the Mallophaga, none of which possess these characters. He also showed that the larva fully corroborates its Coleopterological position, and that its general structure, and particularly the trophi, anal cerci, and pseudopod, confirm its Clavicorn affinities. He showed that the atrophied mandibles in the imago really existed as described by Le Conte, and that even in the larva they were feeble and of doubtful service in mastication. He mentioned, as confirmatory of these conclusions, the finding by one of his agents, Mr. A. Koebele, of Leptinillus (the Coleopterological nature of which no one has doubted, and the nearest ally to Platypsyllus), associated with Platypsyllus upon beaver-skins from Alaska; also the parasitism of Leptinus upon mice. He paid a high compliment to the judgment and deep knowledge of the late Dr. Le Conte, whose work on the imago deserves the highest praise, and whose conclusions were thus vindicated. "Platypsyllus therefore," he concluded, "is a good Coleopteron, and in all the characters in which it so strongly approaches the Mallophaga it offers merely an illustration of modification due to food, habit, and environment. In this particular it is, however, of very great interest as one of the most striking illustrations we have of variation in similar lines through the influence of purely external or dynamical conditions, and where genetic connection and heredity play no part whatever. It is at the same time interesting because of its synthetic characteristics, being evidently an ancient type, from which we get a very good idea of the connection in the past of some of the present well-defined orders of insects."

SCIENTIFIC SERIALS.

Atti della R. Accademia dei Lincei, July and August 1888.—In both of these numbers G. Vicentini and D. Omodei continue their important inquiries on the thermic expansion of certain binary alloys in the liquid state. So far they have arrived at the following general conclusions: (1) the variation of volume accompanying liquid metallic mixtures is extremely slight; (2) no relation can be established between the variations of volume that accompany the formation of alloys in the solid and liquid states; (3) the variation of density at the moment of solidification is in general less than would be the case were the constituent metals to preserve in the alloys the value that they possess in the isolated state; (4) the binary alloys of lead and tin, of tin and bismuth, and of tin and cadmium, possess in the state of perfect fusion an expansion equal to that resulting from the sum of the expansions of the associated metals; (5) the alloy of Bi_2Pb possesses a coefficient of expansion far greater than the sum of the expansions of the constituent metals. These experiments, which conclude for the present with a preliminary study of the antimony and zinc alloys, have been carried out at the physical laboratory of the University of Cagliari, Sardinia.

Rivista Scientifico-Industriale, October.—Experiments made with Crookes's radiometer, by Prof. Pietro Lancetta. The experiments here described have been undertaken chiefly for the purpose of making a synthesis of certain phenomena which are more easily produced by this apparatus than by any other means. It is also shown that the radiometer may in some cases be more advantageously employed than the ordinary thermometer, especially in testing certain laws regarding latent and luminous heat, Crookes's instrument being sensible both to the dark and luminous wave of the solar rays. The results of the experiments show generally that in a homogeneous medium the radiation of the thermo-luminous wave is propagated in a straight line; that the luminous wave is propagated *in vacuo*; that the intensity of the thermo-luminous wave is in inverse ratio to the square of the distance; that the evaporation of fluids as well as the rarefaction of gaseous bodies is accompanied by a lowering of the temperature, while the condensation of gas develops heat.

Journal of the Russian Chemical and Physical Society, vol. xx. fasc. 6.—On the speed and the products of decomposition of the chlorate and chlorite of lithium, by A. Potilitzin, being the second part of an inquiry into the properties of galloid compounds. The decomposition of the two above-mentioned salts, as well as of the bromate of barium, is best explained according to the law of unstable equilibrium indicated by the author in his former works, and which he sums up as follows: in each chemical reaction the equilibrium of the system depends upon the values of their atomic weights, their masses, and their stock of potential energy.—On the relation between the rotatory power and the refraction of organic compounds, by J. Kanonnikoff, first part.—On the action of organic iodides on natrium-nitro-ethane, by N. Sokoloff.—Obituary notice of Prof. Wroblewski, by S. Lamansky.—The total eclipse of the sun of August 19, 1887, by N. Egoroff; and on the results of meteorological observations during the same eclipse, by N. Hesehus.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 31.—"On the Effect of Occluded Gases on the Thermo-electric Properties of Bodies, and on their Resistances; also on the Thermo-electric and other Properties of Graphite and Carbon." By James Monckman, D.Sc. Communicated by Prof. J. J. Thomson, F.R.S.

A piece of platinum wire about 18 inches long was bent in the middle, and one-half protected by being covered with glass tube and made water-tight at the lower end. After annealing the free portion and testing until perfectly free from all strain effects, it was placed, up to about the middle, in acidulated water, and made the negative pole of a battery, and hydrogen liberated upon it for a few minutes. After being dried it was tested with a small flame at distances 1 cm. along its whole length. The result was a current from free wire towards that part on which hydrogen had been produced, greatest at the junction of the free wire and the saturated wire.

When wires of palladium were used, more powerful effects of the same kind were produced.

Carbon rods were next tried. It was found that when one of these rods was heated and placed against the other, the current was always from cold to hot below 200° C.

They were then used as the electrodes in decomposing dilute sulphuric acid, dried carefully until no current was produced on placing them in contact. On heating either rod and joining them as before, a current was produced from hydrogen to oxygen across the hot junction.

The same effect was obtained by decomposing hydrochloric acid solution.

Resistance.—To get rid of possible error from change of temperature, two wires of equal length and section were used and balanced against each other.

These were placed in water, and a current passed from the one to the other, allowed to remain in the acid a little to cool if necessary, and afterwards removed, dried, and placed in an empty glass vessel surrounded with a considerable quantity of water. There they rested until the temperature became the same as the water. When measured, the resistance of the wire containing the hydrogen was found to have increased about one-thousandth part.

Carbon.—Two thin rods about 2mm. diameter were electroplated at the ends and soldered to insulated copper wires.

When used as the poles of a battery the change of resistance was considerable, but greater on the rod that had been the positive pole. By using a platinum electrode, hydrogen or oxygen was produced at will upon the same rod, the other rod remaining unchanged. It then appeared that oxygen increased the resistance much more than hydrogen, rising in some cases as high as nine times; that when oxygen was liberated twice or thrice in succession the resistance increased each time. This continued increase was probably due to chemical changes produced by the active oxygen. Hydrogen gave an increase of resistance, not continuing beyond a certain point, and not becoming greater on repeated charging with the gas.

Generally also the effect of the hydrogen was temporary, disappearing, wholly in some cases, partially in others, when short circuited.

Superposition of Polarization.—Part of the change in the carbon is evidently produced by the mechanical action of the gases evolved, and by the chemical action of the oxygen; both of these will, however, produce permanent changes. That only part of the action is to be explained in this way is shown by the previous experiments. It is, however, further demonstrated by using two carbon rods in decomposing acidulated water; after passing the current for one minute, reverse it for one-tenth of a second and immediately join up to a galvanometer. A short but violent deflection appears for the latter contact, gradually falling to zero and passing to the other side, where it remains for a considerable time, though with much decreased quantity.

The same thing was obtained with platinum electrodes. The second contact must be very short, or the former polarization disappears.

Thermo-electric and other Properties of Graphite and Carbon.—In making the previous experiments, I had occasion to place the heated end of one carbon rod in contact with the cold end of another. The temperature of the hot end was varied from 30° C. to a red heat, whilst the cold end was kept at about 17° C.

Currents of electricity were of course produced. When the temperature of the hotter rod was raised but slightly, the current was from cold to hot through the point of contact, but when it was raised to a red heat the current passed from hot to cold.

I was led to expect that the line of carbon in a thermo-electric diagram, in which the area of the space between the lines is proportional to the electromotive force, would show a bend of some kind, and as no researches were known showing such a bend, it appeared desirable to test it carefully.

Near one end of a carbon rod a hole, about 5 mm. in diameter, was drilled, and into this the end of a platinum wire was inserted and fixed by being wedged with a piece of rod carbon. The whole was thoroughly covered with Indian ink, which, when dry, was again covered with clay. The carbon rod was insulated from the platinum wires, and they from each other by thin sheet asbestos and mica, by which means it was insulated from the vessel in which it was placed, and luted with clay to prevent access of air. From several series of experiments a thermo-electric line was calculated, and found to rise from 0° to 250°, beyond which it descended at the same rate.

Other Changes in the Properties of the Body at the same Temperature.

This change in the thermo-electric power of carbon is accompanied by other changes. The resistance, the expansion, and the specific heat all appear to undergo a corresponding alteration, as the following summary of results shows:—

	Below 250° C.	Above 250° C.
A. Effect of contact of hot and cold carbon.	Current from cold to hot.	Current from hot to cold.
B. Thermo-electric line	Rises.	Falls.
C. Rate of decrease of resistance per degree per ohm	Diminishes.	Increases.
D. The rate of increase of the coefficient of expansion	Increases.	Decreases.
E. Rate of increase of the specific heat	Fairly regular.	Falls to half.

Geological Society.—November 7.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—The Permian rocks of the Leicestershire coal-field, by Horace T. Brown. According to Mr. Brown, the Permian rocks of the Leicestershire coal-field belong to the same area of deposition as those of Warwickshire and South Staffordshire, all having formed part of the detrital deposits of the Permian lake which extended northwards from Warwickshire and Worcestershire, and which had the Pennine chain on its eastern margin. He pointed out the dissimilar nature of these deposits to those of the eastern side of the Pennine chain from Nottingham to the coast of Durham. There were proofs of the existence of a land barrier, owing to the uprising of the Carboniferous, between the district round Nottingham and the Leicestershire coal-field. The most northerly exposure of the Leicestershire Permians is thirteen miles south-west of those of South Notts. He indicated the probable course of the old coast-line of the western Permian lake. Denudation had bared some of the older Palæozoics of their overlying Coal-measures, and it is the rearranged talus from the harder portions of these older rocks which now forms the brecciated bands in the Leicestershire Permians. The reading of the paper was followed by a discussion, in which the President, Prof. Bonney, Mr. Whitaker, Prof. Blake, and Mr. Topley took part.—On the superficial geology of the central plateau of North-Western Canada, by J. B. Tyrrell, Field Geologist of the Geological and Natural History Survey of Canada. The drift-covered prairie extends from the west side of the Lake of the Woods to the region at the foot of the Rocky Mountains, rising from a height of 800 feet on the east to 4500 feet on the west, the gentle slope being broken by two sharp inclines known as the Pembina Escarpment and the Missouri Coteau, giving rise to the First, Second, and Third Praire Steppes. The author described the older rocks of this region, referring especially to his subdivision of the Laramie formation into an Edmonton series of Cretaceous age, and a Pascapoo series forming the base of the Eocene, and then discussed the superficial deposits in the following order: (1) *Preglacial gravels;* occurring along the foot of the Rocky Mountains, composed of waterworn quartzite pebbles, similar to those now forming, and, like them, produced by streams flowing from the mountains; (2) *Boulder-clay* or *Till;* (3) *Interglacial deposits* of stratified material; (4) *Moraines;* (5) The *Kames* or *Åsar*, generally occurring at the bottoms of wide valleys; (6) *Stratified deposits* and beach-ridges which have been formed at the bottoms and along the margins of fresh-water lakes lying along the foot of the ice-sheet; (7) *Old Drainage-channels.* Some remarks on this paper were made by the President, Dr. Hinde, Mr. Whitaker, Mr. Topley, and Mr. Marr.

PARIS.

Academy of Sciences, November 12.—M. Janssen in the chair.—On the cultivation of the square-eared variety of wheat in 1887 and 1888, by MM. E. Porion and P. P. Dehérain. The results of two years' experience in various parts of France show that this variety of red wheat yields under favourable conditions crops which till lately would have been thought fabulous. It succeeds best on well-drained heavy clay soils in the central and northern provinces, and if well manured and sown in regular furrows gives splendid returns; it also shows more power of resisting the destructive action of wind and rain than any other kind.—On the nature of milk, by M. A. Béchamp. This is a reply to the question, Does milk contain any anatomical elements of the system, and if so, do the lacteous globules represent any of these elements? The author's studies lead to the conclusion that milk is not an emulsion; that the lacteous globules are real adipose vesicles in a free state, and that cows' milk contains, besides caseine, other albuminoid substances, not in a free state, but combined in solution with alkalies.—Calculation of the tensions of sundry vapours, by M. Ch. Antoine. Here the general formula $\log p = A\left(D - \frac{1000}{\theta}\right)$ is applied to a number of vapours such as benzene, chloroform, alcohol, carbon chloride, ether, acetone, and carbonic acid, and the comparative results are given for the five temperatures that served as the base for the calculation of Regnault's new formula. The final formulas thus obtained, which had never been worked out by Regnault, must henceforth be adopted in studying the properties of the vapour of acetone.—A new method of improving the capacity of very long telegraphic lines, by M. Fernand Godfroy. This method consists in establishing at each extremity of the line a connection with earth, having a coefficient of self-induction powerful enough, if not to compensate the usual waste, at least greatly to diminish it by the inverse effects which the self-induction tends to produce. The method has already been tried with marked success, on several underground lines, amongst others that between the Paris central station and Angoulême, a distance of 300 miles. Here it was found possible to signal at the rate of twenty words per minute with an *ordinary* Morse apparatus, without any intermediate or local translator, and utilizing one direction only of the current.—On the silicated combinations of glucine, by MM. P. Hautefeuille and A. Perrey. The elements of a leucite or volcanic schorl with aluminia or glucine base ($4SiO_2,Al_2O_3,KO$, or $4SiO_2,Gl_2O_3,KO$), heated between 600° and 800° C. with an excess of neutral vanadate of potassa, are rapidly mineralized. But the nature of the resulting crystals varies in the course of the same operation according as the mineralizing agent yields to the product by which an increasing portion of its alkali is crystallized. Thus this product is homogeneous only under exceptional conditions, and as a rule is a mixture of several chemical species, whose separation is here studied.—Presence of glycolic acid and of normal propylenedicarbonic acid in the grease of sheep's wool, by MM. A. and F. Buisne. In the process of analyzing sheep-washings, the authors have succeeded in isolating these two acids, which are here described. The latter, with formula $COOH—CH_2—CH_2—CH_2—COOH$, is shown to be a higher homologue of succinic acid.—On the Hersiliidæ, a new family of parasitic Copepods, by M. Eugène Canu. Thanks to his discovery at Wimereux of two new genera closely allied to the Hersilia, and parasites of various invertebrates, the author has come to the conclusion that the Hersilia should constitute a new family as distinct from the Siphonostoms as they are from the Peltidians. A full anatomical description is given of this family of Hersiliidæ.—On a new geological map of France, by MM. Jacquot and Michel Lévy. This map, which is on the scale of 1 : 1,000,000, will be issued by the French Geological Service before the close of the year, and will embody the latest researches, including the unpublished reports for 1884–86.—M. du Chatellier has a note on the continued subsidence of the Finistère coast, Brittany; and M. V. Galtier describes some fresh experiments tending to show the efficacy of intra-venous injections of the virus of rabies as a prophylactic against the bite of mad dogs.

Astronomical Society, November 3.—M. C. Flammarion, President, in the chair.—M. Riccò sent a description of a sun-spot observed from November 12 to 25, 1882. This spot, the largest ever observed, had an area over fifty times that of a great circle of the earth. It exhibited rose-coloured veils in some parts, and the nuclei were crossed by large yellow arcs.—M. Gaudibert sent a drawing of Eratosthenes, showing twelve hills within the ring, and a multitude of small craters on the east.—M. Blot, of Clermont (Oise), sent an elementary demonstration of the formula $v^2 = \mu\left(\frac{2}{R} - \frac{1}{a}\right)$, which gives the velocity of a planet in terms of the major axis and of the radius-vector.—Observations of meteors, by M. de Alcantara Penya at Majorca, on June 23, at 7.20 p.m.; by Baroness Ottenfels in the Gulf of Juan, on July 17, at 9 p.m.; by M. Henrionnet at Troyes, on September 9, at 9.30 p.m.—Observations of sun-spots, by MM. Bruguière, Henrionnet, and Loiseau; and of Sawerthal's comet, by MM. Guillaume and Kropp.—M. Guiot, of Soissons, saw the companion to Sirius with a 3¾-inch refractor, and observed Vesta with the naked eye from September 5 to 20.—M. Ferret photographed the moon with a 3⅝-inch refractor.—M. Duprat sent an observation of the lunar eclipse of July 23 made at Constantine.—M. Lihou observed an occultation of a seventh magnitude star by Jupiter.—M. Foray observed Venus on the day of its conjunction, with a 4-inch refractor.—The Sociétés Scientifiques Flammarion, of Marseilles, Argentan, Bruxelles, Jaën, and Bogota, were elected Corresponding Societies.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Orchids of the Cape Peninsula: H. Bolus (Cape Town).—Monographs of the United States Geological Survey, vol. xii. (Washington).—Atlas to accompany a Monograph on the Geology and Mining Industry of Leadville, Colorado: S. F. Emmons (Washington).—Report of the Meteorological Service of the Dominion of Canada for the year ending December 31, 1885 (Ottawa).—The Kingdom of Georgia: O. Wardrop (Low).—New Zealand of To-day: J. Bradshaw (Low).—Physical Realism: T. Case (Longmans).—Cours de Thermodynamique: M. Lippmann (Paris, Georges Carre).—Truth for its own Sake—the Story of Charles Darwin: W. Mawer (Sonnenschein).—Travaux et Mémoires du Bureau International des Poids et Mesures, Tome vi. (Paris, Gauthier-Villars).—Birds in Nature: R. B. Sharpe (Low).—Orient Line Guide, 3rd edition: edited by W. J. Loftie (Low).—The Least of All Lands: W. Miller (Blackie).—Bulletin de la Société Astronomique de France, Première Année, 1887 (Paris).—Transactions and Proceedings of the Royal Society of Victoria, vol. xxiv. Part 2 (Williams and Norgate).—Bolletino della Società di Naturalisti in Napoli, Serie ii., vol. ii., Anno 2, Fasc. 2 (Napoli).

CONTENTS.

THURSDAY, NOVEMBER 29, 1888.

HAUPTMANN ON HARMONY AND METRE.

The Nature of Harmony and Metre. By Moritz Hauptmann. Translated and Edited by W. E. Heathcote, M.A. (London: Swan Sonnenschein and Co., 1888.)

THE author of this singular book was a noted man in the German musical world. He was not only an eminent violinist (pupil and intimate friend of Spohr), but a composer of merit, and, what is more to our purpose here, a great authority on the theory of music and the laws of composition. In 1842 he was appointed, on Mendelssohn's recommendation, Cantor and Musik-Director of the celebrated Thomas-Schule at Leipzig, and Professor of Counterpoint and Composition at the new Conservatorium in that city, filling these posts till his death in 1868. He was undoubtedly the foremost teacher in Europe, and his legion of pupils comprised many who afterwards became musicians of great eminence, including F. David, Curschmann, Burgmüller, the Baches, Joachim, von Bülow, Mr. Cowen, and Sir Arthur Sullivan. He was, moreover, a highly educated man and a Doctor of Philosophy. Hence any writings on music by him could not fail to command attention. His best-known work is a series of letters to his intimate friend, Hauser, the Director of the Conservatoire at Munich, full of spirit, admirable criticism, and deep musical knowledge.

Hauptmann's scientific turn of mind led him to study earnestly the problem which has occupied so many thinkers ever since music has had any consistent form—namely, to discover how the structure which, for some reason or other, seemed most appropriate for it, could be accounted for and explained on philosophical principles. The Greeks talked vaguely of the music of the spheres, and learned men of later days referred it to arithmetical proportions; both with as much reason as modern English musicians join in the *ignis-fatuus* hunt after "natural systems of harmony." Hauptmann sought the explanation in the depths of German metaphysics, and having satisfied his own mind on the matter, he brought out, in 1853, an elaborate volume which he called "Die Natur der Harmonik und der Metrik, zur Theorie der Musik," of which the book now before us is an English version. It appears by a part of the original preface (omitted by the present translator) that he had already introduced the system into his "musikalisch-theoretischen Cursus," and there is no doubt it attracted considerable attention in Germany. A second edition of the book was published in 1873.

But in the meantime the matter had assumed a new shape, through the labours of another German philosopher, Helmholtz. He had the advantage over Hauptmann of a large practice in experimental physics; hence it occurred to him to attack the problem on the physical side, and the result was the well-known epoch-making work, published in 1863, "Die Lehre von den Tonempfindungen als physiologische Grundlage für die Theorie der Musik." He investigated, far more thoroughly than anyone had done before, the physical properties of musical sounds, and their physiological effects on the ear. He showed, with the aid of the facts thus obtained, that the structure of music generally in use was due partly to the nature of the sounds of which it was composed, and partly to artificial considerations which had presented themselves to musical composers on æsthetic and artistic grounds. This swept away the necessity for any abstruse metaphysical speculations; and we strongly suspect that if Hauptmann had waited till Helmholtz's discoveries had been made known, the present treatise would never have appeared.

It is a question, under these circumstances, how far it was worth while to reproduce the book in English; however, here it is; and anyone interested in musical metaphysics, or metaphysical music, and who is unable to read the original, may puzzle over it to his heart's content. It is a handsome volume, and the translation is (so far as we have been able to compare it) carefully and intelligently done. Some of the language may seem uncouth, and much of it obscure, but this is inevitable from the nature of the original, the rendering of which must often have been a tough job. The translator has prefixed an introductory essay, and there is an appendix, entitled "A Short Analysis of Hauptmann's Treatise," which, as we do not find it in the original, we presume is the translator's also, though he does not say so.

The main feature of the work is the application, to musical structure, of the philosophy of Hegel. It would be useless here to attempt to discuss this elaborate subject, or even to describe it. All we can do is to insert a few extracts to give an idea of what sort of thing a person must expect who would take up the work.

As a general point of departure, the translator tells us that—

"The fundamental idea of the philosophy is that every notion—as key, scale, seventh chord, resolution, and so on—is made up after this fashion, *i.e.* that it possesses three elements, involving an antithesis and a reconcilement, and that one of the three elements is the root from which the other two, and so the whole construction, spring. This, Hauptmann regards as self-evident, and it is the basis of Hegelian metaphysics."

The author then says:—

"For the first step it will only be requisite to acquire an inward conception of the notion of the formative process in its wholeness, in the unity of its three elements with which we become acquainted in their first utterance, as the intervals of the octave, fifth, and third. This notion is and remains everywhere the same, in every formation and transformation. It is the notion that something, which at first subsists for intuition in immediate totality (octave), parts from itself into its own opposite (fifth), and that then this opposite is in its turn abolished to let the whole be produced again as one with its opposite (triad), as a whole correlated in itself. Going in the universal sense of this notion, we shall soon be obliged to grant that it no less than comprehends in itself the elements of all knowing, and that anything further for knowledge is not conceivable (*dass ein Weiteres für die Erkenntniss nicht mehr denkbar ist*)."

Sound is defined as

"the coming to be of the being which subsists absolutely during rest, and which is alternately abolished and restored. Not being in self, or dead persistence in rest, nor yet being out of self (*ausser-sich-seyn*) in the motion, is sounding, but coming to self (*zu-sich-kommen*)."

The octave is

"the expression for the notion of identity, unity, or equality with self."

The fifth

"contains acoustically the determination that something is divided within itself, and thereby the notion of duality and inner opposition (*sich selbst Entgegengesetztes*)."

The third contains

"the notion of identification of opposites; of duality as unity."

Of the scheme of a major key, as derived from triad construction, it is said :—

"The organic property of a membered whole can never be represented exhaustively, either by symbols and numbers or by words; it can only be spiritually indicated to intellectual feeling, *i.e.* reason, that meets it half-way. . . . . Now, because this notion has to unite both union and separation, it can only be fulfilled in endlessly continued passage into contrary and comprehension of all opposites. Thus it must be conceived as an infinite process, and consequently as the notion of eternal becoming, living or being real. This is Nature, who, produced as duality from the prime unity (*Ureinheit als Zweiheit hervorgegangen*), and busied continually in making her opposites be absorbed into one another, is live being itself and reality."

This is the sort of way in which harmony is treated; and the same system is adopted in the latter part of the book in regard to metre and rhythm. Most elaborate explanations, illustrated with tables and diagrams, are given of all varieties of time, rhythm, and accent, and they are all etherealized in the same transcendental style. For example, in regard to four-timed metre, we are told that—

"It is this inner reconcilement of separation in unity and unity in separation, the completed negation of every negativing excluding element, that speaks to us here in metrical determination as the essence of the triad, but in combinations of notes as the perfection of harmony; and generally in any guise of phenomenon as the perfected notion of determinate reality."

But we must, in justice to the book, explain that it is not entirely given up to Hegel. The translator properly remarks that, independently of the theory involved, it contains an account, written by a skilful musician and experienced teacher, of harmony and metre, in which the received rules are explained on general principles, while upon particular points new and unexpected light is frequently cast. This is true, for, though Hauptmann loved Hegel well, he loved music more, and nothing could repress his desire to amplify his explanations upon it. It is quite delightful to find him now and then throwing the metaphysics overboard, and breaking out into intelligible common-sense language on the facts and rules of harmony. One can only regret that this sort of thing is not general instead of exceptional.

We have good precedent for this regret. Hauptmann's book was well known to Helmholtz. He studied it carefully, made himself master of all its intricacies, and commented on many of his predecessor's statements and opinions with much respect. Yet what judgment did he pass upon Hauptmann's work as a whole? We will give the passage in the original, as it is really a most true and effective criticism. He says (original edition, p. 427) :—

"Ich kann mich nur dem Bedauern anschliessen, welches C. E. Naumann ausgedrückt hat, dass so viele feine musikalische Anschauungen, welche dieses Werk enthält, unnöthiger Weise hinter der abstrusen Terminologie der Hegel'schen Dialectik versteckt und deshalb einem grösseren Leserkreise ganz unzugänglich sind."

It is, indeed, a great pity that so much excellent musical matter, so many "refined musical views," as this work contains, should be hidden in such an atmosphere of impenetrable transcendental fog, and thus rendered inappreciable by any large circle of readers.

There is another reason which adds seriously to the difficulty of understanding these portions—that is, the use of a complicated system of alphabetical letters in varied types to express musical notes, chords, and passages, instead of the ordinary musical notation. The latter conveys its idea at a single glance; the former requires mental labour which is most wearisome. It is true the author explains that he wants in some cases greater accuracy than the ordinary notation will give; but this is obtained at a fearful sacrifice of clearness and intelligibility, and surely the object might have been accomplished in some less repulsive manner.

It seems odd that the translator, in his elaborate explanatory introduction, never mentions or notices Helmholtz in any way. One would hardly have expected that in the present day, when the science of music has come to be based chiefly on Helmholtz's investigations, a philosophical essay on the subject could have been penned without some allusion to them. There are many passages where the comparison seems invited; for example, Mr. Heathcote says: "The science of music is concerned more with the form than the materials." This is directly at variance with the work of Helmholtz, who bases all his science on the nature of the materials of music—namely, musical sounds—leaving the form to develop itself therefrom. Again, in explaining to us how we are to proceed to learn and follow out the Hegelian system, the translator says :—

"Therefore, when musical sound is said to be unity, we are not to think of it as existing as it does now, and capable of being distinguished into chords, notes, and scales. . . . . Musical pitch, tone, or quality of sound, which depend upon the triad, must not be thought of as existing before the triad exists, and still less as contributing to the formation of the triad."

How different this is from the philosophy of Helmholtz, who demonstrates to us that the triad is a product directly springing out of the physical nature of musical sound!

To sum up, the book is a very remarkable one, the product of a great mind, and of a great authority on the subject it treats of. But we should fear that its interest to English readers will be very limited. The Hegelian portion of it, although, as the translator remarks, it may serve the metaphysician as an application of this peculiar philosophy to a concrete subject, can be of little use to the musician as an explanation of the principles of his art. And as to the technical musical matter, which is really very valuable, the way to render its value appreciable by the public would be to translate it, not merely into English, but into a kind of language with which musicians are familiar, and which ordinary minds would be able to comprehend. W. POLE.

BOTANY OF SOCOTRA.

Botany of Socotra. By Isaac Bayley Balfour, M.D., F.R.S., &c., assisted by other Botanists. Forming Vol. XXXI. of the Transactions of the Royal Society of Edinburgh. (Edinburgh: Grant and Son. London: Williams and Norgate. 1888.)

UNDER the auspices of the Royal Society of London and the British Association for the Advancement of Science, Dr. Bayley Balfour proceeded to Socotra early in 1880, accompanied by Mr. A. Scott, a gardener, and joined at Aden by Lieut. Cockburn, for the purpose of investigating the natural history of the island. Originally it was intended that Colonel Godwin-Austen should lead the Expedition, but circumstances prevented him; and the Committee, through Sir Joseph Hooker, thereupon requested Dr. Balfour—who had recently completed an account of the results of his botanical exploration of the Island of Rodriguez—to undertake the task. He had just been appointed to the Botanical Chair in the University of Glasgow, and the duties connected therewith demanded his presence after the middle of April, therefore the duration of the expedition was necessarily very limited, and the time actually spent on the island was little more than six weeks, ending on March 30. Nevertheless, the botanical collections included specimens of between five and six hundred species of Phanerogams, besides Cryptogams; though, as might be expected from the hasty manner in which the work had to be performed, and the short season during which the plants were collected, many of the specimens were imperfect. The following spring a party of German naturalists visited the island, among them the distinguished African traveller and botanist, Dr. Schweinfurth, to whom Dr. Balfour sent a catalogue of the plants he himself collected, with the gratifying result presently to be explained.

Fortunately the German Expedition entered upon its labours a fortnight later than the time when the British Expedition finished its work. Vegetation was then at a later stage of development; and Dr. Schweinfurth was able to supplement largely Dr. Balfour's botanical collections, especially in the direction of much more complete material. The unselfish devotion to science displayed by Dr. Schweinfurth is eulogized by Dr. Balfour in the following words:—

"With a generosity which is as pleasing as it is rare, he subsequently sent his collections to me in England, in order that the whole flora might be worked out in one. I have already had opportunity to express publicly my lively appreciation of this act of friendship and self-abnegation, and I wish here to put the fact again on record, and to say how much Dr. Schweinfurth's specimens have contributed to the satisfactory working out of the details of the flora. The value of his collection must not be measured either by the number of the species, or by the species he found which we had not gathered. In the excellence of his specimens and their completeness, and the way in which they so frequently supplemented, in flower and fruit characters, deficiencies in ours—therein lay the value of Schweinfurth's plants, and I cannot appraise it too highly."

Dr. Balfour elaborated the joint collections at Kew, and it was announced in NATURE five years ago that he had completed the descriptive part; but delays in connection with the production of the plates, which need not be particularized, we are informed in the preface, prevented the issue of the volume until this year.

It is satisfactory, however, to be able to congratulate Dr. Balfour and the Royal Society of Edinburgh on the quality of the work now first presented to the public in a complete form. Although it may fall considerably short of being an account of all the plants that inhabit the island, it is sufficiently full to be of the utmost value to the student of systematical and geographical botany. Dr. Balfour's 521 pages of letterpress are illustrated by 100 plates, drawn by W. H. and J. N. Fitch, Mrs. Thiselton Dyer, and Miss M. Smith, though mainly by the latter lady. Apart from the descriptive matter, the geographical distribution of the individual species, and the affinities of the flora as a whole, are given in ample detail in the form of an introductory essay, which will rank high among recent contributions to phytogeography. There is also a map on a scale of about $1\frac{1}{4}$ inch to 5 miles, and a brief sketch of the position, physical features, and geology of the island.

Socotra is situated off the north-east corner of Africa, about 500 miles from the entrance to the Red Sea, between 12° and 13° N. lat., and 53° and 54° E. long. Its extreme length from east to west is about 72 miles, its breadth 20; and it is about 140 miles from the nearest point of the African coast, and a little more distant from the Arabian. The surface is mountainous, the interior averaging 1000 feet in altitude, with granite peaks exceeding 4000 feet. The main plateau is of limestone deeply cut into ravines and valleys. In some places the hills rise abruptly from the sea, in others there are intervening sand-plains several miles in width. The climate is cooler and more humid than in the adjacent parts of Africa and Arabia, and in some parts there are perennial streams; but the character of the vegetation generally is that of a dry sterile country.

Out of 575 Phanerogams, or flowering plants, collected, 10, including the date and palmyra palms, orange, cotton, tamarind, and castor-oil, were undoubtedly planted, and many others weeds of wide dispersion; whilst 206 are apparently endemic; and there are 20 endemic genera. Unfortunately, as I think, Dr. Balfour has not deducted the certainly and probably introduced species in his analysis of the composition of the flora, hence his figures representing the percentages of endemic genera and species do not reveal the true facts. Thus he calculates that 36·5 per cent. of the species, and 6·3 per cent. of the genera, are endemic; and these are high percentages, considering the short distance the island is separated from the mainland, and indicate a very ancient flora; but had he eliminated the species in question, these percentages would have stood even higher. On the other hand, it is a mistake to say (Introduction, p. xlix) that the percentage of endemic species is about the same as in Madagascar, where the proportion of endemic genera is double that in the Socotra. The percentage of endemic species in the Madagascar flora is probably at least double that in the Socotran flora, and Mr. Barron, in a paper he recently read before the Linnean Society, estimated the endemic element at 80 per cent. of the species. In British India it is as high as 68 per cent.; in Mexico and Central America combined it reaches 70, and in the whole of Australia 80 per cent.

A comparison of the flora of Socotra with that of the

Bermudas, which are more than three times the distance from the nearest mainland, will give some idea of the differences in the age of the two. The latter contains no endemic genus, and only about half a dozen species, and these are not of a highly differentiated character—one is a portion of a very old flora, and the other a flora of recent derivation. The small flora of Juan Fernandez (which lies about 400 miles from the coast of Chili) contains 21 per cent. of endemic genera and 78 per cent. of endemic species.

The endemic element in Socotra is distributed over fifty-four natural orders, and includes some highly curious types, such as *Cocculus Balfourii*, remarkable for its thick rigid cladodes and often leafless condition; *Thamnosma socotrana*, a member of a Mexican genus; *Dirachma*, a geraniaceous genus of American affinities; *Dendrosicyos*, unique in the Cucurbitaceæ for its arboreous character; *Trichocalyx*, a new genus of the Acanthaceæ; *Cockburnia*, a new genus of the almost exclusively African Selagineæ; and *Cœlocarpus*, a new genus of the Verbenaceæ, having strong American affinities. Of the 136 genera to which the endemic species belong, 98 are only known to be represented by endemic species; and of the 20 endemic genera, 18 are monotypic. The isolated types of American affinities are a repetition of what has also been observed in the fauna and flora of Madagascar.

In summing up, Dr. Balfour finds that the affinities of the flora are essentially tropical African and Asian, the former more pronounced. A former, though very ancient, land connection with Africa he regards as conclusively proved, and the evidence strongly favours the supposition that it was also united with Arabia. With regard to the element of strongest American affinity, its presence is still an unsolvable problem. W. B. H.

THE METALLURGY OF GOLD.

The Metallurgy of Gold; a Practical Treatise on the Metallurgical Treatment of Gold-bearing Ores. By M. Eissler. (London: Crosby Lockwood and Son, 1888.)

THE title suggests that this little volume is a more comprehensive treatise than the author has attempted to write, but it is nevertheless likely to be useful to a large class of readers.

There is a wide-spread belief that, as much of the gold in Nature is found in the "native" or metallic state, its metallurgy must be comparatively simple; and so it would be in nearly all cases if it were not for the fact that the precious metal often occurs in a very fine state of division, or in association with sulphides and tellurides of other metals. Ignorance as to the true nature of such ores has led to their being considered to be "base" or "rebellious," and has entailed much loss and disappointment.

It is asserted, on p. 5, that native gold is never quite pure, being almost invariably alloyed with silver; reference might, however, have been made to the interesting deposit of gold of exceptional purity recently discovered at Mount Morgan, in Australia.

The author points out that "the loss on working gold ores, even with our most modern appliances, is still enormous"; and he gives, among other statistical statements, the results of seven years' working in Colorado, where the average value of the precious metal in the ore, by assay, was £7 18*s.* per ton, while the amount actually extracted was only £3, showing a loss of over 60 per cent. A large section of the work is devoted to the consideration of methods of concentrating the free gold lost during crushing and amalgamating, and the processes for extracting the gold either by amalgamation or by chlorination. The author writes with a practical experience of the processes he describes, but his information is in many cases not up to date.

The illustrations are usually very clear, but in no instance is the scale on which they are drawn given, and there are too few references to the dimensions of the appliances described. The appearance of the illustrations often suggests that they have been borrowed from the trade circulars of the makers of mining machinery. It would surely have been possible to give a more intelligible section of a chlorination works than the one (p. 142) which was suitable enough for its purpose when it originally appeared as an incidental illustration to an Official Report to the United States Government published in 1873; and a far more useful drawing might have been found to accompany the description of the refining of gold by Miller's process than the diagrammatic one which was drawn eighteen years ago by the writer of the present review in order to make the nature of the process clear in an Official Memorandum.

More useful information than is to be found in the twenty-one pages devoted to assaying might easily have been condensed into them, and it is much to be regretted that no attempt has been made to deal with the treatment of tellurides, which are so troublesome to the smelter, and have occasioned so much loss. In a future edition it would also be well to give a description of the process of collecting the precious metals in lead, which plays so important a part in the smelting of complex auriferous ores of lead and copper. Hydraulic mining, also, should find a place. Certain defects of style will no doubt be corrected in a second edition; this will be welcome, for, although the work is hardly in sufficient detail to justify its being called a "practical treatise," it will be useful, especially to men who are engaged in smelting. The author suggests that it contains sufficiently full information for "investors and others interested in gold-mining operations who may wish to gain an intelligent insight into the *modus operandi* at the gold-mines." To them it may be warmly recommended, for, although the element of speculation can hardly be separated from genuine investment in gold-mines, the "adventurers," to use the old name, often deliberately neglect all investigation into the nature of the methods by which they hope to profit. W. C. ROBERTS-AUSTEN.

OUR BOOK SHELF.

Viaggio di L. Fea in Birmania e regioni vicini. II. "Primo saggio sui Ragni Birmani." Del Prof. T. Thorell. (Genova: Tipographia del R. Istituto Sordo-Muti, 1887.)

DR. THORELL deserves our best thanks for having begun a faunistic work on the spiders of Burmah; still greater would have been our gratitude had his minute and exhaustive descriptions been accompanied by figures of the numerous new species recorded. One of the greatest hindrances in the study of exotic araneology is the paucity of such works. The present, however, is not

the first contributed, by Dr. Thorell's labours, for the supply of works on the spider-fauna of exotic regions: witness his extensive work, "Studi sui Ragni Malesi e Papuani," in three volumes, 1877–81, describing and recording over five hundred species of the spiders of that richest of all known exotic regions, the Malay Archipelago. Up to the time of the publication of the present volume, but few Burman spiders were known, the earliest being an Epeirid, described by the late Dr. Stoliczka in 1869. This was followed, in 1878, by two others recorded by Mr. T. Workman, of Belfast; eight more were recorded in 1881 in Dr. Thorell's work above mentioned (on the spiders of the Malay Archipelago); and finally, twenty-two more by M. Eugène Simon in 1884. The total number of species of Burmese Araneidea now known is 163, by far the larger portion being new to science. Dr. Thorell records 145 species, of which 90 are new to science, and 130 new to Burmah. These figures doubtless give a very meagre idea of the spiders of such a rich zoological district as the Burmese Empire. Not to mention its situation in the tropics, Burmah has much in common with the productions of China, Siam, India, and the Malay Archipelago. We may therefore safely hazard a conjecture that the figures given by Dr. Thorell can scarcely represent a twentieth part of the spiders of Burmah. It is to be hoped that since the—still comparatively few—species as yet known have been collated in the present work, an impetus will be given to natural history collectors to add to our knowledge. For the want of figures in Dr. Thorell's work there is some compensation in the concise diagnoses which head each lengthened description. Excepting the introduction, which is in Italian, the work is written in Latin. It is exceedingly well got up, forming a handsome volume of over 400 pages, and is dedicated to the Rev. O. P. Cambridge, General A. W. M. Van Hasselt, and Dr. Ludwig Koch. The greater part of the Burmese spiders described by Dr. Thorell were collected by Signor Leonardo Fea, mostly in Upper (or North) Burmah; and of the rest some were collected in its southern and some in its central districts.

O. P. CAMBRIDGE.

An Introduction to Practical Inorganic Chemistry. By Wm. Jago, F.C.S., F.I.C. (London: Longmans, 1888.)

THIS volume of 72 pages is chiefly taken from the author's "Inorganic Chemistry, Theoretical and Practical," and has been separately issued by request. It is doubtless sufficiently exact for students who are preparing themselves for the "growing number of examinations in which the practical analysis of one or more simple salts is required," and therefore presumably it will serve the end for which it was designed; but it contains many statements that would tend to confuse the genuine student, and give his teacher unnecessary trouble. For instance, on p. 15 it is stated that a "brown ppt. of SnS" is soluble in "SAm_2, re-pptd. by HCl"; and a little lower down that "Arsenic, arsenious compounds . . . Heated in tube—all salts sublime." Both of these statements have so slender a foundation in fact that their direct tendency is to deceive the student. These are mere examples that might be multiplied considerably. Towards the end of the book, tables are given for the "Examination of Mixtures," and wherein they differ from the methods in common use they do not appear to be improvements upon them.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Engineers *versus* "Professors and College Men."

SOME of your readers may recollect that last year (NATURE, vol. xxxv. p. 462) I was led to put the following question to the *Engineer*:—"What is the result of dividing 10 eggs per minute by 2 eggs? Would it, or would it not, be 5 eggs per minute"? To this I have not yet received an answer! It must be very difficult!

I ventured to put the question because the *Engineer* had (in a leading article!) asserted that the result of dividing 3,942,400 foot-pounds per minute, by 33,000 foot-pounds, is 119·4 horse-power. It had actually accused me of ignorance for pointing out the error of such a statement!

But even this remarkable dictum is outstripped in absurdity by some of the *Engineer's* more recent assertions. I quote only a few, any one of which would be sufficient for my present purpose, leaving the vast storehouse to be fully ransacked by other inquirers more curious, and less busy, than myself.

On the 10th of August last the readers of the *Engineer* were treated to the following *ex cathedrâ* pronouncement:—

". . . it is almost impossible to point to anything of value in trade, or manufactures, or engineering which has emanated from the highly-trained Professor or College man."

No comment whatever need be made on this.

A fortnight later, in reviewing a book on the steam-engine, the *Engineer* remarks of Carnot's principle:—

". . . it will soon be understood that with steam this is not true. . . ."

There is nothing in the context to qualify this assertion, rather there is much to intensify and aggravate it. There is manifestly a confusion between temperature and pressure; but it is difficult to find its exact nature; and the only at all analogous case that I can remember is embodied in the indignant outburst of a Celtic student, hard pressed in an "oral," "D'ye mane to till me, Sor, that wather boils at a hundthred degrees in Oireland?"

It might well be thought that we had now gauged the maximum of possible absurdity. But, on September 28, the *Engineer* undertook to enlighten its readers on the subject of Energy; and the effort resulted in some astounding information.

Speaking of "the equation $E = \frac{Mv^2}{2g}$" (a form which will, perhaps, please my friend Prof. Greenhill, though to me it seems to denote merely the product of a mass by a length) our instructor says:—

"The received idea is that, so long as we get E equal to a given number of foot-pounds, it is of no consequence whether we vary v or vary M, . . . but if we introduce the element Time, *which ought not to be left out* [the italics are mine]. . . . The word Energy is unfortunately very vague. . . ." I shall presently recur to this.

October 5 is not without its little novelty. For, as we are gravely informed,

"That much despised faculty, common-sense, has always told engineers that when a given volume of air is passed through a channel or trunk, its pressure will fall as the trunk augments in dimensions."

Even the despised scientific man knows better than that!:—but, true to his convictions, the *Engineer* ignores the conclusions of such mere "Professors and College men" as Bernoulli, Willis, and Sir W. Thomson.

A week later, commenting on some sensible protests (which it prints, in order to refute (?) them) against its marvellous ideas about Energy, the *Engineer* gets back to something like last year's confusion, and mixes up work and horse-power in a manner truly amazing. This peculiar phase of his own mind is what the *Engineer* designates as the "vagueness" of the word Energy!

But the crowning feat is to come. In a very sensible and able introductory lecture to his engineering class at Leeds, Prof. Barr took occasion to speak of the advantages of preliminary scientific training for engineering students, and protested against the mischievous doctrines too often expressed by so-called "practical men" and their literary champions. He took the opportunity of giving a much-needed warning against the inaccuracies of the *Engineer*; referring (among others) to that already quoted about the failure of the Second Law of Thermodynamics.

He has had his reward! His difference of opinion from the *Engineer* is, of course, to be ascribed to youth, inexperience, want of breadth of knowledge, &c. The *Engineer* goes so far as to suggest such ugly ideas as "disregard for truth"; and, having done so, immediately proceeds to quote me by name as the author of a statement which, with the utmost possible distinctness, I had ascribed to its true author, the late Clerk-Maxwell.

Prof. Barr is quite able to hold his own against such ant-

agonists as the *Engineer*, were they to come, like sorrows, "in battalions":—but what of those many humble but ardent students who have had no scientific instruction, and who have been led by circumstances to rely upon such scientific information as they can pick up from the technical or professional journals, too often the only sources available to them?

That such a question can be asked, and with good reason, in Britain and in this nineteenth century, is a matter for profound humiliation.

How correct is the remark of Paulus Pleydell:—"There are folks before whom one should take care how they play the fool"! The gigantic joke perpetrated in the recent Presidential address to the British Association has been too often taken in earnest, and is already bearing fruit of a very different kind from any that could have been contemplated by the witty author. It is recorded that a practical joker once managed to block up the Strand with gaping idiots, simply by staring at the lion on Northumberland House, and muttering to himself, "It *did* wag." What a comment on this is furnished by the never-to-be-excelled discovery recently made by the *Engineer* that "the Engineer and the Engineer alone is the great civilizer," in whose train "the man of science follows"; with its correlative and complementary theorem "The world owes next to nothing to the man of pure science"!

The *Engineer* may read once more, and (I will hope) with profit, that memoir of Rankine which he has so strangely misquoted. I would commend to his special notice the following lines:—

"(Rankine) did not, indeed, himself design or construct gigantic structures, but he taught, or was the means of teaching, that invaluable class of men to whom the projectors of such works intrust the calculations on which their safety as well as their efficiency mainly depend. For behind the great architect or engineer, and concealed by his portentous form, there is the real worker, without whom failure would be certain. The public knows but little of such men. Not every Von Moltke has his services publicly acknowledged and rewarded by his Imperial employer! But he (*i.e.* the man of pure science) who makes possible the existence of such men confers lasting benefit on his country." P. G. TAIT.

The Great Modern Perversion of Education.

I THINK Mr. Victor Dickins will admit on second thoughts that he has hardly taken pains enough to slay the dragon that confronts him. In his letter to you he says, "I have shown above that competition does not produce any of the evil results complained of in the protest," but the special—if not the only—point to which he addressed himself was, I think, to show that the great prize-winners carried on their success into after-life. Now, the protest never asserted or implied that many prize-winners did not succeed fairly well in after-life. Could this be asserted, the charge against such examinations would be so overwhelming and so easy of proof, that the hours of their survival would be few to count. What the protest asserted was that from time to time—"fairly often," might perhaps stand as the translation of the words "again and again"—the great promise of the brilliant young man comes to nothing; that is to say, this happens sufficiently often to warn us, even if no other warnings existed, that our system may be injuring instead of benefiting, may be restricting and destroying mental powers instead of enlarging them.

The point, however, is not the most fruitful one to discuss. It occupied but an insignificant position in the protest—I think less than six full lines in a paper amounting to about three hundred—and, as far as I know, is not a point on which any one of the assailants of competitive examinations has laid much stress. And one reason is plain. We should all differ so much as to what is success. If you pointed me out either a lawyer who successfully stated his case, a public man who got up a question in a few days, and at the end of the time embodied his remedy in a popular Bill to be laid before the House, or a journalist who came down to his office and wrote a brilliant article upon both the evil and the remedy, whilst admitting the useful qualities that each possessed, I should not consider that such qualities —however vigorously and effectively displayed—necessarily afforded any justification of a particular course of educational training. The world has need of such qualities; it rewards them liberally; and whether competitive examinations exist or not, such qualities will abound quite sufficiently under our present conditions.

If, on the other hand, it could be shown that Mr. Robert Browning, Lord Tennyson, Mr. Herbert Spencer, Mr. Darwin, Mr. Huxley, Mr. J. S. Mill, Mr. Buckle, the two Stephensons, the Duke of Wellington, and many such another, had been the product of competitive examinations, I should at once admit that the defenders of the system had greatly strengthened their case by showing that such training could—if not produce—at least not destroy some of the higher excellencies of mind. But I am afraid that no list of fairly successful politicians or lawyers or doctors enjoying fair practice will affect the case in a vital way. Success of an ordinary kind indicates certain valuable qualities, but they are not the qualities, I submit, that should indicate what form the higher education should take.

If Mr. Dickins writes to arrest the movement that has begun, there are certain points to which I think he should address himself,—points, that if he can successfully deal with, he will deprive us remonstrants of much public sympathy. We charge competitive examinations with lowering the higher motives that belong to education, and exercising a bad intellectual and moral influence upon both pupil and teacher. Admitting a good side,—admitting that success in examination implies self-denial and perseverance, and probably such qualities as quick perception, readiness, and good memory,—we still say that in presence of these great examinations the student learns much of what he has to learn in the wrong way. He cultivates what has been called the portative memory. He learns so as to forget. He loads himself with an immense quantity of detailed knowledge that no man in practical life desires to possess. He learns so as to make a display of knowledge rather than to be the real master of it. He strains after effect. He gives himself up to calculations and dodges. He studies the question of marks. He is learned in summaries, footnotes, and manuals. He does not follow out for himself the points that arouse his intellectual interest, but he throws himself as much as possible upon skilled guidance. He works under pressure, assimilating but a small part of what he takes in, and looking intently forward to the day of relief. He is without the great ideals that belong to learning. He is not primarily influenced by the desire of cultivating his own faculties, of learning how to know, of understanding the world in which he has to live; but by the desire of obtaining a favourable verdict from the man who holds the scales by which his success or his failure is to be determined. It is a highly artificial system, and gives throughout a wrong twist to the student's mind, just like the older system of disputations, which is said to have lowered the sacredness of truth, and to have led men into every shift and wile to disallow their ignorance or the weakness of their cause.

Now, these are general statements, and therefore they apply more to certain kinds of examinations than to others; and to certain characters than to others. It is perfectly true that what Mr. Latham calls art-matter can be tested with less injury to the student than knowledge-matter. That is to say, that you can examine a student more profitably to himself in the arts of playing an instrument, performing a dissection, working a mathematical problem, or translating a language, than you can examine him as regards his knowledge of history, literature, philosophy, or natural science. But, in the first place, he would be a bold man who would propose to fashion education according to the necessities of examination, and only to teach those subjects which lent themselves to examination. In the second place, all arts and all knowledge are so intimately allied, that it is easy to see what a narrowing and stunting influence, as regards intellectual development, there would be in a system that demanded anatomy without physiology, a power of translating a language without other knowledge of the history or literature or genius of such language, that demanded even in music simply a power of execution, and in mathematics simply an unlimited ingenuity in working problems on paper. Let anyone think steadily of such treatment of any of these subjects, and he will, I suspect, escape with difficulty from a sense of nightmare; especially if he think of a whole generation of young minds so manipulated for the sake of the examiners. In the third place, of all the undesirable things to achieve, a generally recognized standard of how to do a thing is the most undesirable. Such a standard you must have, when the examined are brought together from all parts of the kingdom to compete in the same examination; and a better-laid plan for the gradual degradation of an art can scarcely be conceived. We ought by now to have learnt this great truth, that standards which make for uniformity are the greatest enemies of improvement.

As regards the teachers, the effect must be as disastrous as

regards the pupils. If the learner cannot learn in the right way, it follows that the teacher cannot teach in the right way. He necessarily becomes an accomplice in the pressure, the hurry, the preparation for a special moment, the skilful handling of a subject so that a sort of examiner's essence may be extracted from it, and nothing more. Just as the pupil must treat many great subjects in an unworthy manner, not giving himself up wholly and devotedly to them; not following out the many questionings which such subjects naturally arouse in minds which have not lost their freshness and originality, not seeking the higher ends,—love of knowledge and the power of understanding this life which we all have to live,—but engrossed in what, from the point of view of self-cultivation, are the lower ends,—the desire of an intellectual triumph, and perhaps of the position which may reward the triumph,—so must the teacher co-operate hand in hand and step by step in all these inferior motives and inferior uses of a great vocation. Indeed, he will be fortunate if after some years of such work he resist the cynical influence which belongs to the system, and do not begin to believe that both young men and their teachers were specially designed for contests in intellectual cockpits, and that in no other way could the young be induced to forego the pleasures and attractions of life at twenty. I have keen recollections of an old keeper, who used greatly to impress my schoolboy's mind by the intense conviction that he had, that a cock would not have known how to use its spurs, a pike would not have been blessed with its appetite for a silvered spoon, and a fox with its scent, if some ulterior intentions had not existed somewhere on behalf of English sportsmen. Had he only understood our educational system, perhaps he would as stoutly have maintained that our young men were born to be examined, and their seniors to examine them. AUBERON HERBERT.

Old House, Ringwood, November 18.

Mr. Dyer on Physiological Selection.

IF the strength of a theory may be measured by the weakness of criticism, I have good reason to be hopeful for the future of "Physiological Selection." On this account I am glad that Mr. Dyer has sought to justify his remarks by giving his reasons for them, although I regret what appears to me the needless asperity of his tone. However, disregarding the personalities in which he has clothed his reply, I will endeavour to show that the reply itself is about as unfortunate as a reply could well be.

Taking his points *seriatim*, I am in no way responsible for the notices of my paper which appeared in the *Times* or in any other periodical, except, of course, those which I have published with my own signature. But, although not responsible for what the newspapers said, I should have corrected any "absurd misrepresentation," had I met with such. The passage, however, which Mr. Dyer now quotes from the *Times* of more than two years ago (and which, I presume, is correctly quoted) does not appear to me a misrepresentation at all. On the contrary, I gather from it that the writer must have perfectly well understood my paper. What he states is that, in my view, natural selection is a theory of the origin of adaptations "*rather*" than a theory of the origin of species. Mr. Dyer appears to regard this as identical with his own statement—viz. that in my view natural selection is not in any sense a theory of species, but "*only* of adaptations." In other words, the former statement correctly imputes to me the opinion that Mr. Darwin's theory is *primarily* a theory of adaptations wherever these occur, and, consequently, *also* a theory of species in every case where species differ from one another in respect of adaptive characters; while the latter affirms unequivocally that in my opinion "specific differences are not adaptive," and, consequently, that Mr. Darwin's theory is a theory of adaptations *to the total exclusion of species*—for an explanation of the origin of which "it follows that we must look to Mr. Romanes himself." Now I must say that if Mr. Dyer cannot see the distinction between these two statements, I may well cease to regret on my own account the difficulty which he says he experiences in understanding my papers.

But although it does not appear that the *Times* misunderstood me in this matter, it is quite true that Mr. Wallace did; and soon after my paper was published, he misrepresented me in exactly the same way as Mr. Dyer misrepresents me now. But I immediately and most emphatically repudiated this astonishing interpretation at the time, in a general answer to criticisms which was published in the *Nineteenth Century* for January 1887. Therefore, whatever Mr. Dyer may think about the reiterated contradiction which I gave in these columns a week or two ago, he is plainly and entirely in the wrong where he refers to it as "a denial that comes rather late in the day." He appears to have adopted Mr. Wallace's interpretation without deeming it worth his while to glance at my reply, before republishing to his audience at Bath a misrepresentation which I had long ago repudiated with all the resources of the English language that I could command.

Why, indeed, any such "denial" on my part should ever have been required has always been to me unintelligible. The original paper itself over and over again insists that I do not at all doubt the important (though, as Darwin says, "not exclusive") part which natural selection has played in the origination of species. Some of these passages I republished in my last letter (October 25), and thus it is for your readers to judge whether the smallest degree of ambiguity attaches to them. But, again ignoring these passages, even as now re-quoted, italicized, and especially addressed to himself, Mr. Dyer seeks to justify what I have now so often had to designate as this "absurd" rendering of my views, by pointing out that in my paper one of the sections is headed "Natural Selection not a Theory of the Origin of Species." This is the only justification that he attempts. Let us see what it is worth.

I readily acknowledge that, to have been quite accurate, the heading of this section ought to have been "Natural Selection not strictly speaking a Theory of the Origin of Species." But I submit that the oversight of here leaving out the words "strictly speaking" (which are elsewhere supplied), was an oversight which could not possibly have misled any reader as to my meaning—unless, of course, he confined himself to reading only the headings of my sections. For it was in the short section thus headed that the very passages occur which I selected from my whole paper to quote in my last letter, as furnishing "direct contradiction" to Mr. Dyer's statement. In other words, following immediately and repeatedly upon the heading in question, there are passages which carefully and unequivocally guard against the very imputation which Mr. Dyer now seeks to force upon me. As a critic of my writings, therefore, he is here trebly in the wrong. First, because his statement of my views admits of being flatly falsified by my original paper itself;[1] secondly, because he ignores all that I have since written upon the same subject; and thirdly, because he now fails to withdraw what I have told him is a travesty of my meaning.

But even this is not all. For Mr. Dyer goes on to say:—"Everybody knows that the idea of evolution of organic Nature existed in men's minds long before Mr. Darwin. He did not originate it; what he did originate was the theory that 'natural selection' is the mechanical means by which that evolution has been brought about. Mr. Romanes says roundly that it is not, or words have ceased to have meaning." Now, if, *without divorcing them from their immediate context*, Mr. Dyer will be considerate enough to point to any words which I have ever writen or ever spoken from which such an interpretation as this can, by any amount of twisting, be extracted, I shall indeed begin to believe that words have so far ceased to convey a meaning of any kind as to be practically useless for purposes of expression. Because I have insisted that, in the great drama of "evolution," natural selection has been *everywhere* the one great agent in the causing of adaptations; because I have said that, on this account, we should take much too narrow a view of so vast an agency were we to regard it as concerned *only* in the origin of "species," or as having to do *only* with such adaptations as happen to be of but specific value; because I have advocated a larger and more correct view of the stupendous importance of this "mechanical means" by which *all* "evolution" in organic Nature has been brought about, with the exception only (as I say in my paper) "of mutual sterility and trivial details of structure, form, and colour," which alone I attribute to the "supplementary factor" of physiological selection,—because I have said all these things, Mr. Dyer now tells me that I have roundly denied the agency of natural selection altogether! After this, I can only feel that

[1] For instance:—"Let me not be misunderstood. In saying that the theory of natural selection is not, properly speaking, a theory of the origin of species, I do not mean to say that the theory has had no part at all in explaining such origin. Any such statement would be in the last degree absurd. What I mean to say is that the theory is one which explains the origin and conservation of adaptations, whether structural or instinctive, and whether these occur in species, genera, families, orders, or classes."

it is hopeless to continue discussion with so extraordinary a disputant. Indeed, as he had published this statement in bald and obvious contradiction to all my writings, nothing more remains to be said: I simply challenge justification as plainly impossible.[1]

The next point in Mr. Dyer's criticism is where he says that if a large proportional number of specific differences are, as I allege, useless, this "would be quite as effective as proving the proposition universally in inflicting a deadly blow on the Darwinian theory, the very essence of which is that specific differences must be advantageous." Now this I deny *in toto.* It is no part of the essence of Mr. Darwin's theory to assume that all specific characters must be advantageous, nor does it even belong to this theory to decide in what proportion as to number advantageous characters stand to indifferent ones. Without going over the ground already traversed with regard to this matter in my previous letter, perhaps it may produce some effect on Mr. Dyer's mind if I quote Mr. Darwin's own opinion upon the subject. After stating what *would* be "absolutely fatal" to his theory, he proceeds (italics here and elsewhere mine), "I fully admit that *many* structures are now of no use to their possessors, and may never have been of any use to their progenitors. . . . *It is scarcely possible to decide* how much allowance ought to be made for such causes of change as the definite action of external conditions, so-called spontaneous variations, and the complex laws of growth; but, *with these important exceptions*, we may conclude that the structure of every living creature either now is, or formerly was, of some direct or indirect use to its possessor" ("Origin of Species," sixth edition, p. 160). Mr. Huxley expresses himself to exactly the same effect in his recently-published obituary notice, where he says that, so far as the theory of natural selection is concerned, a species may present "*any number*" of characters "which are neither advantageous nor disadvantageous, but indifferent, or even slightly disadvantageous" (Proc. R.S., vol. xliv. No. 269, p. xviii.). After all the controversy which I have had upon this subject with Mr. Wallace, I am exceedingly glad to find Mr. Huxley speaking out so "roundly" on the Darwinian side of it. Mr. Dyer, indeed, still objects that he thinks "Mr. Huxley is disposed to make too great concessions." Of course Mr. Dyer is entitled to have his own opinion upon the matter; but I submit he is not entitled to set up this opinion as so authoritative that I am *ipso facto* bound to accept its statement as constituting the very essence of the Darwinian theory. No doubt "with regard to plants" he is "competent to speak"; but he must surely be aware that other botanists who have more thoroughly considered this question are dead against him in his general conclusion. In particular, the late Prof. Nägeli made this subject the matter of a careful inquiry "with regard to plants," the result of which was very materially to influence the judgment of Mr. Darwin.[2]

Next, Mr. Dyer says that because I am not what he calls a "practised naturalist," my "method is the very inverse of that of Mr. Darwin." Now, without at all recognizing Mr. Dyer's right to lecture in this way on the subject of scientific research, I may nevertheless refer him to the history of every other theory which has ever been published with reference to generation, from the "provisional hypothesis" of pangenesis by Darwin himself, through the plastidule of Haeckel and the idio-plasma of Nägeli, to the keim-plasma of Weismann. In all these cases the "method" has been the same as mine—viz. to collate the known facts bearing on the principle suggested, and to leave for future work such experimental verification as may be possible. Moreover, even the theory of natural selection (to which, I suppose, Mr. Dyer more especially alludes) was established by general reasonings from the bringing together of facts already known;[3] and when we remember the much greater importance of this theory, as well as the whole change of thought which it has produced with reference to the general doctrine of evolution, I cannot feel that, relatively speaking, I was over-precipitate in publishing my views on physiological selection.

Further on Mr. Dyer objects to my names for the principle in debate—*i.e.* "physiological selection" and "segregation of the fit"—and says he is "surprised that Mr. Romanes has taken so far no notice" of this objection as originally presented in the *Times.* But here again "the demon of inaccuracy" pursues him. In my general reply to criticisms, already referred to above, and prominently so in these columns at the time, I fully considered this objection; and therefore, if on my side there were still any room left for "surprise," I might have here expressed a certain degree of wonder that before writing the letter which he has now published he should not have taken the trouble to read the author whom he is somewhat intemperately attacking.[1]

In point of fact, however, his attack is everywhere delivered with so complete an absence of judgment, as well as of information on what he is writing about, that it amounts to a mere hitting at random. This, I think, I have now sufficiently proved. Nevertheless, although truly "rather late in the day," I commend to his consideration my article in the *Nineteenth Century* of nearly two years ago, as disposing still more effectually than space will here permit of every one of his general statements and detailed objections. I may add that the bias shown by thus repeating borrowed criticisms, without first consulting my answers, quite deprives his opinion, in my estimation, of the weight to which I might otherwise have felt that it was entitled.

In conclusion, it is useless to conjecture with Mr. Dyer what Mr. Darwin would have thought of physiological selection as a theory; and I have already given my reasons for holding it improbable that he ever considered it (NATURE, vol. xxxiv. p. 545). On the other hand, I should like to remark, that although what he complained of as "the great power of steady misrepresentation"[2] has seriously prejudiced the theory in this country, such has not been the case abroad, where in many quarters it has been received with unqualified favour. This remark—which applies to botanical as well as zoological authority—is added merely in order that the theory may have fair play.

GEORGE J. ROMANES.

Geanies, Ross-shire, N.B., November 4.

P.S.—Proofs of this letter have been accidentally delayed in transmission.—G. J. R., Edinburgh, November 22.

Cleistogamy.

I SHOULD like to add a few words to the extract from the minutes of the Scientific Committee of the Royal Horticultural Society, quoted in NATURE, November 22, p. 86. The causes of the cleistogamous condition of some of the plants mentioned I would attribute to their stunted habit induced by mowing, coupled with a relatively cold season. For, while some of them, *e.g. Cerastium*, *Montia*, and *Alchemilla*, rarely open their buds, the *Veronica*, *Sagina*, and *Trifolium*, are more inclined to do so, if allowed to grow more vigorously, and if the temperature be higher. Cleistogamy is of course only a relative quality. Thus chickweed and spurry will open their flowers widely in hot weather; but are cleistogamous and abundantly self-fertile all

[1] In another part of his letter Mr. Dyer says that my theory of physiological selection "shrivels up the part played by natural selection to very small dimensions." Here, again, I can only request that some explanation should be given of the process of reasoning whereby my critic has arrived at this most astonishing conclusion.

[2] "I now admit, after reading the essay by Nägeli on plants, and the remarks by various authors with respect to animals, more especially those recently made by Prof. Broca, that in earlier editions of my 'Origin of Species' I perhaps attributed too much to the action of natural selection. . . . I did not formerly consider sufficiently the existence of structures, which, as far as we can at present judge, are neither beneficial nor injurious; and this I believe to be one of the greatest oversights as yet detected in my work."—"Descent of Man," second edition, p. 61. See also, for emphatic passages to the same effect in the "Origin of Species," sixth edition, pp. 171, 176, 421.

[3] "Belief in natural selection must at present be grounded entirely on general considerations, ... and chiefly from this view connecting under an intelligible point of view a host of facts."—Letter of Darwin to Bentham, "Life," &c., iii. p. 25.

[1] The following is what I said with regard to this criticism in the *Nineteenth Century* for January 1887, and it leaves nothing further to be said now:—"This is a point of no real importance, and I readily concede that in some respects physiological isolation would be a better name than physiological selection. The reasons which inclined me to adopt the latter in preference to the former will be gathered from what has just been said. If the theory is sound at all, a process of true survival takes place, in some cases of the primary [*i.e.* sexual], in other cases of those secondary [*i.e.* morphological] specific characters which are capable of inducing the primary; and in either event it is only certain changes of character, or particular variations, which are *selected* to survive as new species. Moreover, the term physiological selection does not exclude the term physiological isolation, any more than the term natural selection excludes the term survival of the fittest. . . . The 'fitness' of the individuals affected is guaranteed by the fact of their having reached the breeding age. This latter point is important, because Mr. Wallace accuses me of having lost sight of the consideration that my physiological variations must conform to the law of natural selection. . . . If these physiological varieties ever occur at all, *ex hypothesi* they must have so far passed muster with respect to general fitness as to be allowed to propagate their kind. It was for the sake of emphasizing this feature of my theory that I gave the latter the alternative title of 'segregation of the fit.'"

[2] "Origin of Species," p. 421. The whole paragraph, read in connection with the present controversy, is curiously interesting, and to me very consoling. Moreover, the scientific creed which it rehearses is in every particular identical with my own, while differing considerably from that of my critic.

through autumn and winter, if it be mild. With regard to *Trifolium subterraneum*, as it was about thirty years ago when observed it, I cannot now be certain that it was actually cleistogamous; but it grew with just the same habit as the above, and was most probably self-fertile as they are.

GEORGE HENSLOW.

Nose-Blackening as Preventive of Snow-Blindness.

I BEG to send you an extract from a letter just received from my son, of the Indian Geological Survey Department, and who is at present engaged by the Maharajah of Kashmir in exploring and reporting on his sapphire mines. Since it refers to former communications in NATURE (vol. xxxviii. pp. 7 and 101), upon a subject of interest to travellers, it may be of use.

I may here mention that my son speaks of having found the Eocene Nummulitic limestone in Zanskar at a height of 18,500 feet above the sea. Sir J. D. Hooker tells me that he has previously observed the Nummulites in Tibet, at a height of 18,000 feet.

J. D. LA TOUCHE.

Stokesay, Craven Arms, November 20.

"Some time ago there was a letter in NATURE describing a method of protecting the eyes from sun-glare, when crossing snow, by blackening the nose and cheeks under the eyes. I tried the dodge the other day, when I was crossing the snow-fields and glaciers from Zanskar, and found it very successful. My shikari and some of the other natives were much amused when I produced a piece of charcoal, and proceeded to blacken my face; but they also tried it, and said that it relieved them very much. I do not know how the effect is produced, but it was much the same as when one went off the snow on to a patch of moraine or rocks clear of snow. The blackening seemed to stop the reflected rays in some way. The natives expressed the feeling by saying that it cooled their faces. I found it quite possible to walk over the snow for many miles without glasses, which are a nuisance, especially on rough ground; but without the blackening I had to put them on. The sun at these high altitudes has much greater effect than in England when the ground is covered with snow."

Amber.

IN NATURE (vol. xxxvi. p. 63), I find the following note:—"The largest piece of amber ever discovered was recently dug up near the Nobi's Gate, at Altona. It weighed 850 grammes." I beg to state that a piece of amber, weighing 5·6 kilogrammes, is in the possession of Messrs. Stantien and Becker, in Königsberg, and that pieces weighing 6·5 and 9·5 kilogrammes can be seen in the Berlin Mineralogical Museum, both discovered off the sea coast of North Germany. Even as far inland as Silesia, a piece of Baltic amber, weighing 3 kilogrammes, has been found in the bed of the River Oder, near Breslau. Baltic amber occurs in Silesia also as high as 1400 feet above the level of the sea.

A. B. MEYER.

Royal Museum, Dresden, November 19.

ON THE MECHANICAL CONDITIONS OF A SWARM OF METEORITES.[1]

II.

THE next point to consider is the mass and size which must be attributed to the meteorites.

The few samples which have been found on the earth prove that no great error can be committed if the average density of a meteorite be taken as a little less than that of iron, and I accordingly suppose their density to be six times that of water.

Undoubtedly in a meteor-swarm all sizes co-exist (a supposition considered hereafter); for even if originally of uniform size they would, by subsequent fracture, be rendered diverse. But in the first consideration of the problem they have been treated as of uniform size; and, as actual sizes are nearly unknown, results are given for meteorites weighing $3\frac{1}{8}$ grammes. From these, the values for other masses are easily derivable. It is known that meteorites are actually of irregular and angular shapes, but certainly no material error can be incurred when we treat them as being spheres.

The object of all these investigations is to apply the formulæ to a concrete example. The mass of the system is therefore taken as equal to that of the sun, and the limit of the swarm at any arbitrary distance from the present sun's centre. The theory is of course more severely tested the wider the dispersion of the swarm, and accordingly in a numerical example the outside limit of the solar swarm is taken at $44\frac{1}{2}$ times the earth's distance from the sun, or further beyond the planet Neptune than Saturn is from the sun. This assumption makes the limit of the isothermal sphere at a distance 16, about half-way between Saturn and Uranus.

In this case the mean velocity of the meteorites in the isothermal sphere is $5\frac{1}{3}$ kilometres per second, being $\sqrt{\frac{6}{5}}$ of the linear velocity of a planet revolving about a central body with a mass equal to 46 per cent. of that of the sun, at distance 16. In the adiabatic layer it diminishes to zero at distance $44\frac{1}{2}$. This velocity is independent of the size of the meteorites. The mean free path between collisions ranges from 42,000 kilometres at the centre, to 1,300,000 kilometres at radius 16, and to infinity at radius $44\frac{1}{2}$. The mean interval between collisions ranges from a tenth of a day at the centre, to three days at radius 16, and to infinity at radius $44\frac{1}{2}$. The criterion of applicability of hydrodynamics ranges from $\frac{1}{60000}$ at the distance of the asteroids, to $\frac{1}{3600}$ at radius 16, and to infinity at radius $44\frac{1}{2}$.

All these quantities are ten times as great for meteorites of $3\frac{1}{8}$ kilogrammes, and a hundred times as great for meteorites of $3\frac{1}{8}$ tonnes.

From a consideration of the tables in the paper it appears that, with meteorites of $3\frac{1}{8}$ kilogrammes, the collisions are sufficiently frequent even beyond the orbit of Neptune to allow the kinetic theory to be applicable in the sense explained. But if the meteorites weigh $3\frac{1}{8}$ tonnes, the criterion ceases to be very small at about distance 24; and if they weigh 3125 tonnes, it ceases to be very small at about the orbit of Jupiter. It may be concluded then that, as far as frequency of collision is concerned, the hydrodynamical treatment of a swarm of meteorites is justifiable.

Although the numerical results are necessarily affected by the conjectural values of the mass and density of the meteorites, yet it was impossible to arrive at any conclusion whatever as to the validity of the theory without numerical values, and such a discussion as the above was therefore necessary.

I now pass on to consider some results of this view of a swarm of meteorites, and to consider the justifiability of the assumption of an isothermal-adiabatic arrangement of density.

With regard to the uniformity of distribution of kinetic energy in the isothermal sphere, it is important to ask whether or not sufficient time can have elapsed in the history of the system to allow of the equalization by diffusion.

It is shown therefore in the paper that in the case of the numerical example primitive inequalities of kinetic energy would, in a few thousand years, be sensibly equalized over a distance some ten times as great as our distance from the sun. This result, then, goes to show that we are justified in assuming an isothermal sphere as the centre of the swarm. As, however, the swarm contracts, the rate of diffusion diminishes as the inverse $\frac{5}{2}$ power of its linear dimensions, whilst the rate of generation of inequalities of distribution of kinetic energy, through the imperfect elasticity of the meteorites, increases. Hence, in a late stage of the swarm, inequalities of kinetic energy would be set up, there would be a tendency to the production of convective currents, and

[1] Abstract of a Paper read before the Royal Society on November 15 by Prof. G. H. Darwin, F.R.S. Continued from p. 83.

thus the whole swarm would probably settle down to the condition of convective equilibrium throughout.

It may be conjectured, then, that the best hypothesis in the early stages of the swarm is the isothermal-adiabatic arrangement, and later an adiabatic sphere. It has not seemed worth while to discuss this latter hypothesis in detail at present.

The same investigation also gives the coefficient of viscosity of the quasi-gas, and shows that it is so great that the meteor-swarm must, if rotating, revolve nearly without relative motion of its parts, other than the motion of agitation. But as the viscosity diminishes when the swarm contracts, this would probably not be true in the later stages of the history, and the central portion would probably rotate more rapidly than the outside. It forms, however, no part of the scope of this paper to consider the rotation of the system.

The rate of loss of kinetic energy through imperfect elasticity is next considered, and it appears that the rate, estimated per unit time and volume, must vary directly as the square of the qaasi-pressure, and inversely as the mean velocity of agitation. Since the kinetic energy lost is taken up in volatilizing solid matter, it follows that the heat generated must follow the same law. The mean temperature of the gases generated in any part of the swarm depends on a great variety of circumstances, but it seems probable that its variation would be according to some law of the same kind. Thus, if the spectroscope enables us to form an idea of the temperature in various parts of a nebula, we shall at the same time obtain some idea of the distribution of density.

It has been assumed that the outer portion of the swarm is in convective equilibrium, and therefore there is a definite limit beyond which it cannot extend. Now a medium can only be said to be in convective equilibrium when it obeys the laws of gases, and the applicability of those laws depends on the frequency of collisions. But at the boundary of the adiabatic layer the velocity of agitation vanishes, and collisions become infinitely rare. These two propositions are mutually destructive of one another, and it is impossible to push the conception of convective equilibrium to its logical conclusion. There must, in fact, be some degree of rarity of density and of collisions at which the statistical treatment of the medium breaks down.

I have sought to obtain some representation of the state of things by supposing that collisions never occur beyond a certain distance from the centre of the swarm

Then from every point of the surface of the sphere, which limits the region of collisions, a fountain of meteorites is shot out, in all azimuths and at all inclinations to the vertical, and with velocities grouped about a mean according to the law of error. These meteorites ascend to various heights, without collision, and, in falling back on to the limiting sphere, cannonade its surface, so as to counterbalance the hydrostatic pressure at the limiting sphere.

The distribution in space of the meteorites thus shot out is investigated in the paper, and it is found that near the limiting sphere the decrease in density is somewhat more rapid than the decrease corresponding to convective equilibrium.

But at more remote distances the decrease is less rapid, and the density ultimately tends to vary inversely as the square of the distance from the centre.

It is clear that according to this hypothesis the mass of the system is infinite in a mathematical sense; for the existence of meteorites with nearly parabolic and hyperbolic orbits necessitates an infinite number, if the loss of the system shall be made good by the supply.[1]

But if we consider the subject from a physical point of view, this conclusion appears unobjectionable. The ejection of molecules with exceptionally high velocities from the surface of a liquid is called evaporation, and the absorption of others is called condensation. The general history of a swarm, as sketched at the beginning, may be put in different words, for we may say that at first a swarm gains by condensation, that condensation and evaporation balance, and finally that evaporation gains the day.

If the hypothesis of convective equilibrium be pushed to its logical conclusion, we reach a definite limit to the swarm, whereas if collisions be entirely annulled the density goes on decreasing inversely as the square of the distance. The truth must clearly lie between these two hypotheses. It is thus certain that even the small amount of evaporation shown by the formulæ derived from the hypothesis of no collision must be in excess of the truth; and it may be that there are enough waifs and strays in space ejected from other systems to make good the loss. Whether or not the compensation is perfect, a swarm of meteorites would pursue its evolution without being sensibly affected by a slow evaporation.

Up to this point the meteorites have been considered as of uniform size, but it will be well to examine the more truthful hypothesis that they are of all sizes, grouped about a mean according to a law of error.

It appears, from the investigation in the paper, that the larger stones move slower, the smaller ones faster, and the law is that the mean kinetic energy is the same for all sizes. It is proved that the mean path between collisions is shorter in the proportion of 7 to 11, and the mean frequency of collision greater in the proportion of 4 to 3, than if the meteorites were of uniform mass equal to the mean. Hence the numerical results found for meteorites of uniform size are applicable to non-uniform meteorites of a mean mass about a third greater than the uniform mass; for example, the results for uniform meteorites of $3\frac{1}{8}$ tonnes apply to non-uniform ones of mean mass a little over 4 tonnes.

The means here spoken of refer to all sizes grouped together, but there is a separate mean free path and mean frequency appropriate to each size. These are investigated in the paper, and their values illustrated in a figure. It appears that collisions become infinitely frequent for the infinitely small ones, because of their infinite velocity, and again infinitely frequent for the infinitely large ones, because of their infinite size. There is a minimum frequency of collision for a certain size, a little less in radius than the mean radius, and considerably less in mass than the mean mass.

For infinitely small meteorites the mean free path reaches a finite limit, equal to about four times the grand mean free path; and for infinitely large ones, the mean free path becomes infinitely short. It must be borne in mind that there are infinitely few of the infinitely large and infinitely small meteorites. Variety of size does not then, so far, materially affect the results.

But a difference arises when we come to consider the different parts of the swarm. The larger meteorites, moving with smaller velocities, form a quasi-gas of less elasticity than do the smaller ones. Hence the larger meteorites are more condensed towards the centre than are the smaller ones, or the large ones have a tendency to fall down, whilst the small ones have a tendency to rise. Accordingly, the various kinds are to some extent sorted according to size.

An investigation is made in the paper of the mean mass of meteorites at various distances from the centre, both inside and outside of the isothermal sphere, and a figure illustrates the law of diminution of mean mass.

It is also clear that the loss of the system through evaporation must fall more heavily on the small meteorites than on the large ones.

After the foregoing summary, it will be well to briefly

[1] It must also be borne in mind that the very high velocities which occur occasionally in a medium with perfectly elastic molecules, must happen with great rarity amongst meteorites. An impact of such violence that it *ought* to generate a hyperbolic velocity will probably merely cause fracture.

recapitulate the principal physical conclusions which seem to be legitimately deducible from the whole investigation; in this recapitulation qualifications must necessarily be omitted or stated with great brevity.

When two meteorites are in collision, they are virtually highly elastic, although ordinary elasticity must be nearly inoperative.

A swarm of meteorites is analogous with a gas, and the laws governing gases may be applied to the discussion of its mechanical properties. This is true of the swarm from which the sun was formed, when it extended beyond the orbit of the planet Neptune.

When the swarm was very widely dispersed, the arrangement of density and of velocity of agitation of the meteorites was that of an isothermal-adiabatic sphere. Later in its history, when the swarm had contracted, it was probably throughout in convective equilibrium.

The actual mean velocity of the meteorites is determinable in a swarm of given mass, when expanded to a given extent.

The total energy of agitation in an isothermal-adiabatic sphere is half the potential energy lost in the concentration from a condition of infinite dispersion.

The half of the potential energy lost, which does not reappear as kinetic energy of agitation, is expended in volatilizing solid matter, and heating the gases produced on the impact of meteorites. The heat so generated is gradually lost by radiation.

The amount of heat generated per unit time and volume varies as the square of the quasi-hydrostatic pressure, and inversely as the mean velocity of agitation. The temperature of the gases volatilized probably varies by some law of the same nature.

The path of a meteorite is approximately straight, except when abruptly deflected by a collision with another. This ceases to be true at the outskirts of the swarm, where the collisions have become rare. The meteorites here describe orbits under gravity which are approximately elliptic, parabolic, and hyperbolic.

In this fringe to the swarm the distribution of density ceases to be that of a gas under gravity; and as we recede from the centre the density at first decreases more rapidly, and afterwards less rapidly than if the medium were a gas.

Throughout all the stages of its history there is a sort of evaporation by which the swarm very slowly loses in mass, but this loss is more or less counterbalanced by condensation. In the early stages the gain by condensation outbalances the loss by evaporation; they then equilibrate, and finally the evaporation may be greater than condensation.

Throughout the swarm the various meteorites are to some extent sorted according to size; as we recede from the centre the number of small ones preponderates more and more, and thus the mean mass continually diminishes with increasing distance. The loss by evaporation falls principally on the small meteorites.

A meteor swarm is subject to gaseous viscosity, which is greater the more widely diffused is the swarm. In consequence of this a widely extended swarm, if in rotation, will revolve like a rigid body without relative motion (other than agitation) of its parts.

Later in the history the viscosity will probably not suffice to secure uniformity of rotation, and the central portion will revolve more rapidly than the outside.

The kinetic theory of meteorites may be held to present a fair approximation to the truth in the earlier stages of the evolution of the system. But later, the majority of the meteors will have been absorbed by the central sun and its attendant planets, and amongst the meteors which remain free the relative motion of agitation must have been largely diminished. These free meteorites—the dust and refuse of the system—probably move in clouds, but with so little remaining motion of agitation that (except perhaps near the perihelion of very eccentric orbits) it would scarcely be permissible to treat the cloud as in any respect possessing the mechanical properties of a gas.

The value of this whole investigation will appear very different to different minds. To some it will stand condemned as altogether too speculative; others may think that it is better to risk error in the chance of winning truth. To me, at least, it appears that this line of thought flows in a true channel; that it may help to give a meaning to the observations of the spectroscopist; and that many interesting problems, here barely alluded to, may perhaps be solved with sufficient completeness to throw light on the evolution of nebulæ and planetary systems.

EDISON'S PERFECTED PHONOGRAPH.

THE marvellous results attained by Mr. Edison's recent improvement on, or, more properly perhaps, resurrection of, the original phonograph of 1878 have induced us to present a view of the latest form of the instrument, together with a short description of its main features and most recent performances.

Mr. Edison is still occupied in perfecting the instrument, and scarcely a week passes without his sending over to his European colleague, Colonel Gouraud, substantial evidences of progress towards perfecting the arrangements either for the recording and reproducing of all kinds of sounds, or else in the construction and the postal conveyance of phonograms.

Although, therefore, the instrument can hardly at present be said to have reached its final stage of development, in its chief constructive points it may be regarded as practically perfected; while some recent trials of it show that it is capable not merely of recording, but of reproducing, every kind of sound with which we are acquainted, including articulate speech, with a fidelity little short of absolute perfection.

When Léon Scott invented his phonautograph, he unconsciously came near the phonograph, though he merely contented himself with reproducing the vibrations pictorially on a blackened surface. Prof. Helmholtz, on the other hand, by his profound studies in the analysis and synthesis of speech into fundamentals, accompanied by varying combinations of subsidiary harmonics, seems to have created quite a scare among the phonographists, by showing them what a terribly complicated affair articulate speech was. In the phonograph, however, we have a machine which not only differentiates all these complicated systems of vibrations, checks, and harmonics, but integrates them equally well. It is, moreover, capable of repeating the integrations practically as often as we please.

This perfected power of record, reproduction, and preservation of sound has been accomplished partly through the substitution of a specially prepared wax for the original tinfoil, but also a good deal through other improvements in the diaphragms, needles, &c. The vibrations of the recording diaphragm are transferred by means of a cutting-needle to the wax, which is thus carved and indented into a series of hills and valleys, which represent in *intaglio* the resultant form of the original sound-vibrations, including part, if not all, of the minor inflections due to the presence of the subsidiary harmonics or overtones.

The tinfoil used in the original machine of 1878 only very partially fulfilled the office of a recording surface, and since every indentation in it necessarily involved a corresponding rise of the material on either side, the vibrations of the recording style, and *a fortiori* of the air itself, were only very imperfectly reproduced on its surface. The hollow character of the undulations, moreover, caused them to be easily effaced after a few repetitions.

The records on the wax, on the other hand, have been recently reproduced over 3000 times.

As a good many of the specifications describing the new improvements are not yet made public, we are not at liberty to describe minutely the various parts of the instrument. We can, however, give the following outline of its leading features :—

(1) There is a brass cylinder on which the wax phonogram is placed.

(2) A rocking holding arm, which carries what is termed the "spectacle," containing the recording and reproducing diaphragms, and which by means of a traveller arm is made to engage two rotating screws—one called the feeding screw, very finely threaded, which causes it to travel slowly from left to right over the rapidly rotating phonogram, and thus trace out a long spiral on its surface; the other, a coarser reversely-threaded screw, which enables it to be moved back more rapidly opposite any required position on the phonogram. By means of a screw-head attached to a turning bar with an arrangement of cams, the rocking holding arm can be made to engage either the feeding or reversing screw, or else lift the diaphragm and its recording or reproducing needle entirely off the surface of the phonogram. These back and forward movements can be preferably made by a treadle attached to either the turning bar or rocking arm, leaving the hands free for other purposes.

(3) Another very practically useful adjunct consists of a projecting arm attached to the turning bar, by which the operator when desiring to stop and think what he is going to say next can completely disengage the diaphragm and and its accompanying recording needle from the surface

Showing reproducing diaphragm in position, the operator listening through the tubes, and standing behind the instrument so as to allow it to be seen

of the wax; and if the pause is a long one, and he does not desire to waste his phonogram, he can reverse the movement of the diaphragm until it is opposite the point on the phonogram where he left off. By combinations of arrangements of this sort, all the actions of an ordinary writer, stopping to think, looking over, and even correcting what he has written, can be imitated.

(4) The arrangement by which a phonogram can be removed and another replaced, simply consists in having one of the centres on which the brass cylinder turns, attached to a movable arm.

(5) Though, theoretically, one diaphragm could effect both record and reproduction, it is found that the same shape of needle or style is unsuited for both purposes. Consequently, there are two diaphragms, one a recorder and the other a reproducer.

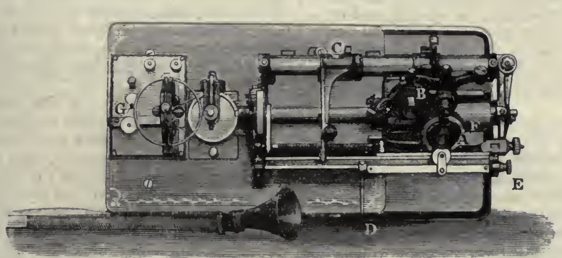
The style of the recording diaphragm is made of such a shape as to be more of the nature of a graving tool, while that attached to the reproducing diaphragm is inclined at an angle to the surface of the wax, and glides over its indentations without destroying them. In front of the recording style is placed the cutting-out tool, whose function is somewhat analogous to the dibble of the gardener. It not only prepares a furrow in the wax in which the record can be made, but it also destroys any previous record. So that the making of a fresh record and the effacing of a previous one can proceed simultaneously.

The use of tubes to the ear raises a point about which a good deal of misapprehension appears to exist. The machine at present is not intended for reproducing sounds with their original intensity, but rather for the perfect reproduction of articulate speech and music. Many persons,

therefore, on hearing the reproduction through the magnifying funnel, are disappointed to find the effect below their expectations. As soon, however, as they listen through the tubes, they are proportionately surprised at the loudness and the clearness of the sound and articulation. While for most practical purposes audition through the tubes is quite sufficient, Mr. Edison is, we understand, constructing a means by which the sounds can be greatly magnified. Even as it is, with the present funnel the reproduction can be heard very well throughout a large room. For example, at a lecture on November 10, before Harrow School, a perfect *mélange* of speaking, singing, and whistling, made by Colonel Gouraud on the spot, was plainly heard all over the lecture theatre, in which about 600 persons were present.

Other improvements comprise an electric motor and speed governor, by which the phonogram and the feeding screws can be rotated at a constant speed. As this electric motor is itself the subject of a separate patent, we are only at liberty to say that it is an electro-dynamic multipolar motor, in which a ring armature acts as a fly-wheel, and that it is adjustable to different speeds—a necessary point in order to preserve the same pitch where the rapidity of utterance is subject to variation. The circuiting of the motor and governor is ingeniously arranged so that the field of the armature can be opened without interfering with the field-magnet circuit, thus securing greater sensitiveness and an absence of sparking.

The phonograms themselves are divided into two sorts, office-grams and mailing-grams. The former are cylinders, capable at the present time of yielding from thirty to fifty surfaces for record, which number, as Mr. Edison says in a letter only received a few days ago, can now, by improved methods, be increased to two hundred. Obviously, however, such a cylinder would be an awkward affair to send by post. Mr. Edison has therefore



Instrument turned over so as to present a top view with recording diaphragm in position. A, recording diaphragm; B, reproducing diaphragm; C, rocking holding arm; D, bar for arresting record; E, turning bar; F, wax cylindrical phonogram; G, electric speed governor.

met this want by constructing the mailing-gram, and though this may seem a small matter from a scientific point of view, yet we venture to prophesy that among all his many achievements there will be none to which he will look back with greater pride, or which are destined to work a greater revolution in the history of the world than this apparently simple little mailing-gram. To say that it is capable of being posted, and reproduced at the other end without injury to the record, may perhaps give some idea of its practical value.

Regarding the way in which all this is accomplished, it is needless to say anything, except that the device bears the true stamp of genius, viz. simplicity. This portability of the phonograms is, in fact, one of the salient features by which the phonograph of 1888 stands out in marked contrast to the imperfect machine of 1878, and this improvement, in combination with the greater perfection and permanence of the record, at once raises it from the level of a pretty scientific toy or curiosity to one of immediate utility.

The practical working of the instrument, which has been greatly improved even upon what it was at the meeting of the British Association at Bath this year, may be gathered from the fact that Colonel Gouraud dictates all his correspondence through it, speaks to it in different languages, applies every conceivable test to try its powers, and with results which not only astonish him and everybody else, but even the inventor himself.

The purposes to which such an instrument can be applied—scientific, commercial, domestic, artistic, military—seem countless. The dreams which were indulged in when the phonograph of 1878 appeared, can now be realized; and we owe to Mr. Edison another substantial addition to the long list of direct results of scientific labour achieved during the present century.

FURTHER NOTES ON THE LATE ERUPTION AT VULCANO ISLAND.

MY friend Signor Gaetano Platania, who accompanied me on my trip through the Lipari Islands in June 1887, and stayed some days with me at Vulcano, has undertaken the task of describing that interesting event and the subsequent phenomena. He has very kindly forwarded me specimens of the ejectamenta, and to him I must express my thanks. He being already well acquainted with the products of that volcano, his observations will be of considerable value when published.

The first specimen submitted to me is that of the so-called bombs, common in other eruptions that have taken place from the present active crater of Vulcano. It is undoubtedly the *essential* ejectamenta, although included in the paste is much fragmentary *accessory* material. These so-called bombs are irregular polygonal masses of an obsidian-like material on the outside; the surfaces are traversed by a number of clefts or fissures V-shaped in section, which at their bottom and the deeper parts of their sides are seen to be composed of a spongy glass or even pumice. Their mode of formation is no doubt as follows:—The glassy magma from former loss of heat has become so viscous that the escape of vapours from the underlying magma is arrested until the tension rises, and the superincumbent pasty, almost solid, mass is broken up and ejected. This ejection has been preceded by some expansion and cracking, together with some cooling along the cracks, so that the blocks have partly consolidated as pumiceous obsidian, but when relieved from these conditions by ejection, the hotter material

within each block expands, due to the liberation, as vapour, of the dissolved H_2O, and the formation of a vesicular structure, which may progress to such a point as to constitute a true pumice. This is accompanied by fissuring of the external hardened surface, just as the expansion of dough splits open the crust, as the air-bubbles expand before and after the loaf is in the oven—in fact, we could not adopt a better term to define this structure than *bread-crust structure*. These fissures often divide crystals, pieces of included rock, &c., showing that little plasticity was left on the surface when this expansion took place. In fact, the conditions necessary are that the glass be sufficiently cold to break with a strain or blow applied sharply, but to bend when the force employed is gradual in its action, such as may be seen well in all vitreous substances. If we warm a stick of sealing-wax, hardened Canada balsam, &c., we may gently bend it to any form, but if our attempt is too quick, the stick breaks. In these bombs many of the surfaces appear to have first bulged and then broken. As these are ejected, and consist of a hard crust and soft interior, I suppose we must use that unfortunate term bomb, though they are rarely round and certainly do not strike one as resembling as much a bomb as do those masses found on the surface of lava-streams.

The colour is buffish-gray, the surface somewhat glistening and scattered over by the exposed broken surfaces (split by the division fractures between the contiguous blocks) of a dark green mineral, chiefly pyroxene, glassy crystals of feldspar, smaller black metallic lustred grains of magnetite, more rarely typical grains of olivine, quartz, and pyrites. There are also very many grains of different sizes of a darker-coloured fine-grained rock which incloses many of the augites, feldspars, olivines. Microscopically, the vesicular structure is seen to extend, though becoming less marked, to within (in the specimen examined) less than a millimetre of the surface, though in larger blocks from other eruptions preserved in my collection this may attain 2 centimetres or more. The crystals of pyroxene are usually well formed, though often broken. They include, wholly or partly, large, rather irregular magnetites, and in some cases are surrounded by wreaths of either sanidine or, more commonly, triclinic feldspar, probably labradorite. Where included in foreign rock-fragments, this latter is seen to be composed of a network of magnetite, augite, and feldspar microliths, and is often much altered. The feldspars are principally sanidines which may attain half a centimetre long; they are very dirty from inclusions, and somewhat rounded. There are also groups of labradorite crystals, and another triclinic feldspar in which the striations are remarkably close and fine. In some cases a triclinic feldspar seems to be intergrown with the sanidine. Of the latter mineral there are many microcrystals and microliths. Here and there are to be met with a few ill-formed crystals of dark-green amphibole.

What part of these minerals belong to the essential magma, and what are simply imperfectly fused out of the surrounding rock, it is extremely difficult to determine, and chemical analysis of the rock would be obviously useless, on account of the numerous inclusions of other rock-fragments. The association of such basic minerals with a distinctly acid rock would be very remarkable, were it not for the distinct origin of them by inclusion of accessory materials. The eruptive rocks of this island range from a very rich olivine basalt through a dolerite to the typical obsidians and spherulites. There is little doubt that these included minerals are the churned-up fragments in the crater apex which almost certainly cuts through those older rocks, and even part of the present active cone of Vulcano is composed of dolerite.

That these bombs are the primary ejectamenta in this eruption there is no doubt, on account of their freshness and the sharp uneroded angles and edges, as observed by Signor Platania, together with the absence of any solfatarizing. The specimen examined was ejected during the month of August, probably early in that month.

The next specimen is dated August 18, and consists of coarse sand or fine lapilli, about the size of a mustard-seed, with a little fine gray ash. This I made into an artificial breccia, and cut sections of it. It is composed of broken fragments of dolerite and glassy rocks, both often solfatarized, with chips of pyroxene, magnetite, &c., and, no doubt, is chiefly *accessory* ejectamenta derived from the crumbling sides of the crater being churned, ground up, and ejected.

Next is a fine ash of light gray colour, ejected on August 26, which is, in great part, also composed of similar materials to the last, with an abundance of very minute microliths, many of a dark-green colour, and therefore probably pyroxene or amphibole, though they remain dark between crossed nicols, from their great minuteness. I have observed no trace of tridymite found in such abundance in the ash of one of the recent eruptions of Vulcano. This we should expect to be formed at a later date, when the more tranquil vapours, escaping through the material at the crater bottom, would allow of their deposition.

From the description of the eruption by Mr. Narlian that was given in my paper on this subject at the British Association meeting this year, and reproduced in the *Times* and other newspapers, from the examination of the eruptive products, and from the state of the volcano previous to its last eruption (see my paper, "The Islands of Vulcano and Stromboli," NATURE, vol. xxxviii. p. 13) taken together with what we learn from the ejectamenta, we may obtain a fair idea of the eruptive process in this case. The chimney of the volcano was, no doubt, filled by an acid magma, which, perhaps, after the last eruption, was of much higher temperature, and in which fragments of other rocks from the crater and chimney sides had been churned up and partially fused. The temperature and liquidity seem to have been low, as the olivines and augites, although they have apparently been fused out of their original matrix, especially the latter, retain most perfectly their crystalline angles, and no chemical fluxing or reaction seems to have occurred between the basic minerals and the surrounding acid magma. Also, the occurrence of pyrites points in a similar manner to the same physical state. The choking of the crater after the former eruption, together with the gradual cooling of the upper part of the magma column during the intermediate solfataric stage of the volcano, would result in a gradually increasing obstacle to the boiling-off of the H_2O dissolved by the magma lower down. Two processes would therefore be going on, viz. increased superincumbent pressure, and augmenting tension of the part of the subjacent magma within reach of water-supply. The latter must obviously, after a certain time, increase in a greater ratio than the former, until the plug is blown asunder.

This plug in great part would consist of the magma with its inclusions reduced to that critical state between a liquid and solid, as seen in vitreous bodies. When this is broken up by the sudden impulse of the expansion of the subjacent aquiferous magma, it would split into fragments; and, these immediately cooling on their surface by the molecular formation and escape of vapour near that surface, cooling and solidification would result, but before this extended far in, the hotter interior would undergo frothing, and so bend, crack, and fissure the nearly hard coating, producing in this manner the bread-crust structure. These blocks seem from Mr. Narlian's account, to have fallen nearly red hot, as his children's feet were burnt, and part of the house where they fell was burnt. After the first explosion, a series of feebler explosions took place, and, I believe, are still continuing with diminished force, just as is seen

in boiling up an extremely thick syrup. Add to this the crumbling in of the crater-sides, their pulverization and ejection, and we have the picture of a typical paroxysmal eruption, tending towards an explosive one, of an obsidian volcano. H. J. JOHNSTON-LAVIS.

Naples, November 3.

Since writing the above, I have received the following letter from Mr. Narlian, which will form a fitting appendix to his former one read at the British Association, and published in the *Times* and elsewhere.

"*Lipari*, *Italy*, *November* 3, 1888.

"MY DEAR DR. JOHNSTON-LAVIS,—I have your kind note, for which I thank you. Our crater (*i.e.* Vulcano) is still in a very active condition. The eruptions succeed each other nearly every minute or two. Columns of thick black ashes are ejected to heights that cannot be less than 15,000 feet. The stones, red hot, are also thrown out in immense quantities and to great heights. Sometimes these eruptions are accompanied by loud detonations, which are indistinguishable from those of a gun, only they are so overpoweringly loud that at Lipari they are heard as if a piece of 100 tons had gone off near at hand. Till now there is no lava, and we hope there will not be any.

"I observe a difference in the ejected matter: in the beginning of the eruption they were stones, in time they began to show a burned calcined appearance, became quite black and friable by the action of the fire, and now they are nearly pumice of a dark and rough kind.

"I shall be glad to send you some few specimens by the first boat for Naples.

"I am, dear Sir,

"Yours faithfully,

"A. E. NARLIAN."

This prolonged activity is a most interesting phenomenon, and two explanations are open to us—viz. either the supply of igneous magma has increased, and the volcano is passing from the solfataric stage to a strombolian or Vesuvian phase, or the supply of dissolved H_2O in the magma extends to great depths, or is derived from a very large mass of magma. The change in the ejectamenta would rather point to the latter; as if, the first boiled paste being ejected, the more aquiferous paste from greater depths was undergoing discharge of its vapour. This may possibly be followed by the eventual outpour of lava, indicating the arrival at the surface of still deeper magma, comparatively poor in dissolved H_2O, so that the view of an obsidian stream may be in store for us before long—an event of considerable importance to vulcanological science. H. J. JOHNSTON-LAVIS.

NOTES.

WE lately noted that Mr. J. F. Duthie, Director of the Botanical Department, Northern India, had accompanied the recent military expedition to the Black Mountain country. The Black Mountain forms the northern boundary of the district of Hazara, which forms a long narrow valley, bounded on the west by Cashmir. Extending far into the heart of the outer Himalayan range, it is shut in on either side by mountains, rising to 17,000 feet. The flora is almost wholly unknown. But the time of year was unfavourable for botanical collecting, and Mr. Duthie writes to Kew: "I did not manage to find much of botanical interest on the Black Mountain; excepting the fine bits of forest, composed of *Abies Webbiana* and *Pinus excelsa* on the crests of the mountain, the country is barren in the extreme."

THE Kew Museum has lately received a choice collection of interesting objects from Corea, collected and brought home by Mr. T. Watters, who was Acting Consul in that country from January 1887 to June last. The specimens in question, which consist of hand-screens, fans, &c., made of paper from the paper mulberry (*Broussonetia papyrifera*, Vent.), together with samples of the paper itself, sun-blinds made of split bamboo, &c., illustrate in a remarkable degree the extreme neatness and accuracy of the Coreans in their handicrafts. The following are some of the specimens received and now exhibited in the Kew Museum. A series of different qualities of paper, all made from the bark of the paper mulberry. These comprise plain white or cream-coloured papers of various degrees of finish, used for drawing, writing, packing, &c.; also coloured papers such as are used by the people for writing birthday missives upon. It would seem that the Coreans, like the Japanese, use paper very extensively for a great variety of purposes. Thus, for fans, the handles of which are delicately ornamented, as well as for hand-screens, tobacco-pouches, coverings for hats in wet weather, paper is equally applicable; for the latter purposes, however, it is steeped in oil, which makes it thoroughly waterproof. The hand-screens are made by first forming a foundation of thin strips of split bamboo radiating from the handle, which is afterwards covered so completely on both sides with a thin paper film and varnished that a strong and durable article is the result. Some of the hand-screens presented by Mr. Watters to the Kew Museum were given to him by the King, and are of much finer workmanship than those that are purchasable. The oil-steeped paper tobacco-pouches and hat-coverings are a close imitation of oilskin; the latter, which when opened is cone- or tent-shaped, is used by all classes except the peasantry, even including the soldiers. The Corean boy's kite, which is also made of *Broussonetia* paper, consists of a piece of paper about a foot square with a circular hole in the middle, kept in form by thin strips of bamboo; a thin string is attached to each corner and brought together and connected to a single string, which is wound upon a wooden windlass. The perfection of splitting bamboo into thread-like strips seems to be divided equally between the Chinese and the Coreans, judging from a remarkably fine example of a blind which forms one of the exhibits. These very fine blinds are said to be used only by high mandarins, and the coarser kinds by the lower classes. Another illustration of very fine work is in the utilization of split rattans in the manufacture of articles of clothing, an undershirt and cuffs of very open ornamental workmanship being made entirely from this material, which is both soft and pliable. These shirts are said to be used next the skin in hot weather to prevent the oute shirt adhering to the body.

MR. J. S. JAMESON, of the Emin Pasha Relief Expedition, who died of fever at Bangala Station, on August 17 last, had accumulated a number of carefully-selected trophies and objects of natural history. These objects have been brought together at 166 Piccadilly, and arranged by Mr. Rowland Ward, so that they may be accessible to naturalists, and to his friends, who have been invited to view them to-day.

THE Russian Geographical Society has just published an "Instruction for Observations upon Shifting Sand Regions." The paper was carefully prepared by a Committee of persons thoroughly acquainted with the subject, and might with advantage be translated and communicated to other Geographical Societies.

AN important addition to school laboratories has just been completed at Eastbourne College, where the science teaching is undergoing great development. The laboratory, which has just been built there, affords working accommodation for twenty-four students, and has been thoroughly well fitted. The working

benches are ranged down each side of the length of the room, and are all fitted with gas and water; and ample storage room is provided by four capital cupboard-cases. An excellent lecture-table occupies the usual position; the sand-bath and still are of copper, and heated by Fletcher's burners. There is also a convenient master's room, which, when finished, will be very complete.

PROF. H. G. SEELEY, F.R.S., is about to deliver a course of lectures on the practical study of the geology of the country round London. This course is given at the request of students of the London Geological Field Class; and information concerning the lectures may be obtained from Mr. William Dunn, 21 King William Street, Strand, W.C.

THE Penny Science Lectures at the Royal Victoria Hall for the month of December will be as follows:—December 4, on "Nature's Hot Springs," by Dr. S. Rideal; December 11, on "Limestone Rocks and their History," by Mr. E. Wethered. The series will begin again on January 22, 1889.

AT a *conversazione*, given by the Oxford University Junior Scientific Club, on Friday, November 23, in the University Museum, Prof. Milnes Marshall, of the Owens College, delivered a lecture on "Animal Pedigrees," which was highly appreciated by a numerous company of members of the University and their friends. Later in the evening Colonel Gouraud introduced and explained the new Edison phonograph. The members of the Club further entertained their guests by varied scientific exhibits and demonstrations, while the band of the 60th Royal Rifles enlivened the proceedings with music.

THE Duchess of Albany has consented to become Patroness of the Sanitary Institute.

A GIGANTIC stalactite cave has been discovered near Rübeland, in the Harz Mountains, surpassing the neighbouring Bauman's Cave in size and beauty. Some excellent photographs of different parts of the cave were taken by Dr. Max Müller, of the Brunswick Technical High School, by means of the electric light. These photographs are shortly to be published, accompanied by explanatory notes by Prof. Kloss, of Brunswick. The cave is to be lighted by electricity, and opened to the general public next year, after precautions have been taken to keep it in its present perfect state.

DURING the last summer Dr. Otto Zacharias examined carefully the crater "Maare" (lakes) of the volcanic Eifel. They are inhabited by numerous species of Copepoda, Daphnidæ, Radiolaria, Rotifers, water mites, and insect larvæ. The largest of the "Maare," the Laacher See, which measures about seven miles in circumference, contains a special fauna. Besides this lake, Dr. Zacharias examined four others: the Pulvermaar, Holzmaar, Gemündener Maar, and Schalkenmehren Maar.

AT the meeting of the Royal Meteorological Society on November 21, Dr. A. Riggenbach, of Basle, read an interesting paper on a method of photographing cirrus clouds. Great difficulty is experienced in obtaining photographs of cirrus clouds, the reason being that the blue light of the sky acts with nearly the same actinic energy as the white light of the clouds on the sensitive silver salts of the plate. What is wanted is that this blue light of the sky should be dulled, the light of the clouds being left unaffected, and this can be done by means of the analyzer of a polarizing apparatus. The light from the blue sky is partly polarized, and to the largest extent at the points which are situated 90° from the sun; the plane of polarization passing through the points looked at, the sun, and the eye of the observer. On the other hand, the light coming from a cloud is only polarized to a slight extent. Having spoken of what can be done by the use of a Nicol's prism, Dr. Riggenbach went on to say that we might substitute for a Nicol's prism a dark mirror, a painter's mirror, or, best of all, a plate of obsidian. If such a plate be held so that the plane which passes through the cloud, its reflected image, and the eye, is normal to the line from the observer to the sun, the mirror extinguishes the polarized light from the sky almost completely, and the reflected image of the cloud comes out sharp on a dark background. If such an obsidian plate be fixed before the lens of a photographic camera, so that its plane is inclined at an angle of 33° to the optical axis of the lens, and the camera be placed so that the sun's rays shine perpendicularly on one of its sides, we then turn the whole apparatus round, in the direction in which the sun lies, as an axis, until a cirrus cloud is visible in the camera. If a sensitized plate be inserted, a picture of the cloud can be produced under the most favourable conditions possible. A still simpler mode of obtaining such cloud pictures is to use the surface of a lake as a polarizing mirror. The best clouds for such a purpose are those at sunrise or sunset, at an altitude of about 37°, and in an azimuth either greater or less than that of the sun by 90°.

THE North Atlantic Pilot Chart for November states that the most noteworthy disturbance during October was a West Indian hurricane which developed near Yucatan on the 9th, and reached the south coast of Long Island on the 12th. The tracks of all the depressions moving eastward from the American coast during the first half of the month lay well to the northward of the normal path until reaching the 55th parallel. This is very interesting in connection with the persistence with which an energetic area of high barometer lingered over the middle of the Atlantic, and affords a good illustration of the tendency of areas of low pressure to avoid those of high. Only one iceberg, near Belle Isle, was reported.

WE have received the Report of the Meteorological Service of the Dominion of Canada for the year 1885, which shows continual progress and improvement in the various departments. In addition to the Annual Reports, containing the results for numerous stations, a Monthly Review is published giving a general *résumé* of the weather throughout the Dominion, and an analysis of the daily forecasts and storm warnings. The weather signals carried on the railway cars are much appreciated, both by the farming community and the general public. New stations are established in the more remote districts as opportunities offer, and many valuable observations are also obtained along the line of the Canadian Pacific Railway, in regions where it is difficult to get observers other than the station officials.

Two fine series of salts of two new platinum bases containing sulphur and organic radicles have been prepared by the Swedish Professor Blomstrand and his assistants, of Lund. They will form a striking addition to the now large number of these remarkable platinum compounds, which have been obtained since the preparation of the first of their class, the well-known green salt of Magnus, in 1828. When a solution of potassium platinous chloride, K_2PtCl_4, is shaken with two molecular equivalents of ethyl sulphide $(C_2H_5)_2S$, a quantitative precipitation of a yellow chloride, of the composition $Pt^{S(C_2H_5)_2\,.\,S(C_2H_5)_2Cl}_{Cl}$, occurs. On treating this somewhat complex substance suspended in water with another two equivalents of ethyl sulphide, the whole eventually dissolves, with the exception of a small quantity of an oily substance, which appears to be formed as a secondary product. On allowing the separated clear liquid to stand, it gradually deposits crystal crusts of greenish-yellow monoclinic tables of the chloride of the first new base, $Pt^{S(C_2H_5)_2Cl}_{S(C_2H_5)_2Cl}$. In a similar manner, Prof. Blomstrand has obtained the bromide, $Pt[S(C_2H_5)_2Br]_2$, which crystallizes in reddish-yellow monoclinic prisms; and the iodide, which resembles the bromide very closely, but forms beautiful dark-red monoclinic crystals of considerable size. In addition to these well-defined halogen salts, the nitrite

was obtained in large colourless rhombic crystals; the sulphate, $Pt[S(C_2H_5)_2]_2SO_4 + 7H_2O$, in exceptionally large crystals exhibiting a great number of faces; and also the nitrate in very soluble crystals. The halogen salts of the group are readily transformed into the more stable platinic compounds, which are found to be much more difficultly soluble. Thus the chloride, $Cl_2Pt[S(C_2H_5)_2Cl]_2$, was obtained in the form of yellow tables and prisms belonging to the triclinic system. The bromide forms red monoclinic prisms, and the iodide, perhaps the prettiest salt of the whole series, crystallizes from chloroform in dichroic prisms, which appear dark red by transmitted, and dark blue by reflected, light. The second series of salts are precisely analogous, but contain the radicle methyl instead of ethyl. It is interesting to note that Prof. Blomstrand was successful in isolating the base of the second series itself, $Pt[S(CH_3)_2]_2(OH)_2$, as a yellow, strongly alkaline liquid. Between the two series, an interesting mixed sulphine-chloride was obtained, containing both ethyl and methyl, $Pt\genfrac{}{}{0pt}{}{S(C_2H_5)_2Cl}{S(CH_3)_2Cl}$, by the addition of two equivalents of methyl sulphide to the yellow chloride first mentioned above. It will be readily seen that the work thus briefly reviewed forms a most valuable contribution to our knowledge of the platinum bases.

A LIST of the writings of Dr. Asa Gray, chronologically arranged, with an index, has been printed as an appendix to vol. xxxvi. of the *American Journal of Science*, and is also published separately. The compiler has done his work with great care.

IN an interesting paper on "Musical Sands," Mr. C. Carus-Wilson has discussed the cause of the remarkable sonorous properties exhibited by the sands of various localities, a subject which was referred to in this journal on August 30 and September 27 of the present year. Mr. Carus-Wilson gives the details of numerous experiments. Some of them are of a very ingenious character, which lead him to the conclusion that the vibration of the individual sand-grains is brought about by friction, and that it is the cumulative effect of numerous vibrating particles of the same size that becomes audible. This conclusion differs in some respects from the theories which have been put forward by some other investigators of these very curious phenomena.

THE first part of a useful "Introduction to Entomology," by J. H. Comstock, has been published at Ithaca, U.S.A. The groups of insects have been fully characterized, so that their relative affinities may be learned; and much space has been given to accounts of the habits and transformations of the forms described. The work contains many original illustrations.

A LITTLE book, by Mr. W. Mawer, containing a brief account of the life and discoveries of Darwin, has just been issued by Messrs. Swan Sonnenschein and Co. The volume is likely to be of interest to young people, for whom it has been specially written.

IN the Administration Report of the Madras Government Central Museum for the year 1887-88, Mr. Thurston, the Superintendent, speaks of two tours which he made during the year on behalf of the Museum, viz. to Tuticorin and the Nílgiris. At the former place great assistance was rendered by Captain Phipps, Port Officer, in placing boats and divers at his disposal. Large collections, illustrative of the marine fauna, were made, mainly through the medium of native divers, who displayed no little skill, and some of the specimens, *e.g.* the entire collection of Sponges and Echinoderms, have been sent to the British Museum (Natural History), for investigation and report. The results of this tour will be published after Mr. Thurston has made a further tour of the Gulf of Manaar. During his tour on the Nílgiris a large area of both the plateau and slopes was explored, and large collections of birds, reptiles, butterflies, &c., were made, but as he only returned to Madras a few days before the termination of the official year, he reserves a list for his next annual report.

THE current number of the *Asiatic Quarterly Review* contains an article by Captain A. C. Yate on the Shan States, in which reference is made to the ethnology of the obscure region bounded on the west by Burmah, on the north and north-east by China, on the east by Tonquin, and on the south by Siam and the Karennee. Of the Shans he has a very low opinion: he describes them as sordid and lazy, they live in extreme poverty, they are not brave, and their foot is yet barely on the lowest rung of the ladder of civilization. Still, they are somewhat ahead of the tribes around them. They have a literature: Captain Yate says it lies about dirty and uncared for in pagodas, priests' houses, and travellers' rest-houses. Amongst the other tribes inhabiting the same region are the Palaungs, the Red and White Karens, Kakyens or Kachins, Dunoos, Laos, Was, Kaws, Chins, Yíns or Yeíns, Yindalaings, Padaungs, Taungthus, Müsüs, and Kakins. All these are believed to speak their own dialects, but none of them have a written language. The Palaungs, Kachins, and Laos are the more important of these. The Kachins are extending all over the region, and are hated and feared by the Shans. They worship or propitiate certain evil forces in Nature which they call "nats." Captain Yate gives some curious details in regard to this and other tribes, but his aim is rather to describe what they are as he saw them, than to discuss the ethnological problems connected with this region and its races and fragments of all but extinct peoples.

IN a paper in the current number of the Journal of the Anthropological Institute, Mr. J. Allen Brown states, on the authority of Mr. Carlyle, late of the Archæological Survey of India, that some few of the rudest aboriginal tribes of the wildest central parts of India still practise a modified and partial sort of tattooing, but only with deep blue or other dark or grey colour, never with red. Hindus use red and white, and sometimes yellow colours superficially, without incisions, which will wash off, as religious sectarial caste marks on their foreheads. These are the only instances, so far as Mr. Brown can learn, where such pigments are now used either for embellishment or religous symbols in India.

WE have received the second part of vol. iii. of the Proceedings of the Linnean Society of New South Wales. The following are among the contents: on additional evidence of the genus *Ichthyosaurus* in the Mesozoic rocks ("Rolling Downs Formation") of North-Eastern Australia, by R. Etheridge, Jun. (Plate vii.); on additional evidence of the occurrence of *Plesiosaurus* in the Mesozoic rocks of Queensland, by R. Etheridge, Jun. (Plate viii.); description of a new *Tripterygium* from Port Jackson, by E. P. Ramsay and J. D. Ogilby; notes on the Mueller Glacier, New Zealand, by Captain F. W. Hutton (Plates ix. and x.); the insects of King's Sound and its vicinity (Part 1), by William Macleay; Australian indigenous plants providing human foods and food-adjuncts, by J. H. Maiden; geographical notes in Malaysia and Asia, by the Rev. J. E. Tenison-Woods; Diptera of Australia (Part 2, the Sciaridæ), by F. A. A. Skuse (Plate xi.); note on sympathy and foster-parentage among birds, by E. G. W. Palmer; on some new and rare Hydroida in the Australian Museum Collection, by W. M. Bale (Plates xii.–xxi.); notes on Australian Coleoptera, with descriptions of new species, by the Rev. T. Blackburn; the development and structure of the pineal eye in *Hinulia* and *Grammatophora*, by W. J. McKay (Plates xxii.–xxiv.).

IN his note on sympathy and foster-parentage among birds, Mr. Palmer tells a curious story of a wood-duck and a hen.

"Some time ago," he says, "a boy brought in an egg found near a waterhole, which was placed with other eggs under a sitting hen, and in due course hatched out a wood-duck. The wood-duck was reared among a clutch of chickens, was as well tended as her other chicks by the mother hen, and reached adult age. On one occasion a hen brought out a brood of chickens, and the wood-duck kept in close companionship with the hen and chicks for several days, until the hen took umbrage at the duck's constant attendance, and several fights between the hen and duck ensued. Eventually the duck drove away the hen, and took sole charge of the chickens throughout the day, the hen following round disconsolately till night-fall each day, when the duck surrendered her charge, allowing the mother to brood over them at night, but again taking charge of them in the morning. This continued till the chickens were able to take care of themselves."

THE writer of the article on "The Opening of the Pasteur Institute" (NATURE, November 22), informs us that by an oversight a misprint occurs on p. 74, in the first line of the second paragraph. Class *C.* should read Class *A*.

THE additions to the Zoological Society's Gardens during the past week include a Black-eared Marmoset (*Hapale penicillata*) from South-East Brazil, presented by Miss B. Pollock; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mr. David Baumann; a —— Toad (*Bufo* ——) from California, presented by Mr. D. E. Cardinall; a North American Turkey (*Meleagris gallo-pavo*) from North America, presented by Mr. F. J. Coleridge Boles; a Tawny Owl (*Syrnium aluco*), European, deposited.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *e* (BARNARD, SEPTEMBER 2).—Dr. L. Becker, who has recently computed elements for this comet by the method of variation of the distances, finds (*Dun Echt Circular*, No. 164) that the most probable orbit is hyperbolic in character, the residuals for the intermediate observations being much larger for the most suitable parabola than for the hyperbolic orbit.

The elements are as follow :—

$$T = 1889 \text{ January } 29\cdot90453 \text{ G.M.T.}$$

$$\left.\begin{aligned} \pi - \Omega &= 339^\circ\ 54'\ 32''\cdot1 \\ \Omega &= 357\ \ 15\ \ 59\cdot1 \\ \iota &= 166\ \ 22\ \ 24\cdot3 \end{aligned}\right\} \text{Mean Eq. } 1888\cdot0.$$

$$\log q = 0\cdot2595204$$
$$\log e = 0\cdot0135800$$

Ephemeris for Greenwich Midnight.

1888.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
Nov. 29	2 28 2	5 57·3 S.	0·0501	0·2971	12·1
Dec. 1	2 15 18	6 21·0			
3	2 3 9	6 41·4	0·0669	0·2928	11·4
5	1 51 40	6 58·4			
7	1 40 50	7 12·2	0·0887	0·2887	10·5
9	1 30 42	7 23·2			
11	1 21 15	7 31·5	0·1131	0·2847	9·6
13	1 12 29	7 37·5			
15	1 4 21	7 41·2 S.	0·1390	0·2810	8·7

The brightness on September 2 has been taken as unity.

Dr. Berberich has, however, computed parabolic elements using an observation made at Karlsruhe on October 28, the ephemeris from which satisfies well an observation obtained at Rome as recently as November 7.

According to Dr. Becker's ephemeris, the comet will pass within 1½′ of a bright nebula, 100 ♅ I Ceti, a little before midnight on December 10, and as the head of the comet is of very considerable size, the nebula will be completely involved in it. The moon will be near setting, so that, though the comet will be low, about 75° Z. D., spectroscopic observations might be obtained if the night were clear. Dr. Berberich's elements would place the transit some three hours later, when the comet would have set to English observers, but makes the transit nearly a central one.

COMETS FAYE AND BARNARD, OCTOBER 30.—The following ephemerides for these objects for Berlin midnight are by Drs. Lamp and Spitaler respectively (*Astr. Nach.*, No. 2867) :—

	Comet 1888 *d* (Faye).		Comet 1888 *f* (Barnard, Oct. 30).	
1888.	R.A. h. m. s	Decl. ° ′	R.A. h. m. s.	Decl. ° ′
Dec. 1	8 14 34	2 29·2 N.	10 19 27	8 50·8 S.
3	8 14 36	2 12·8	10 20 47	8 16·9
5	8 14 30	1 57·2	10 21 59	7 41·5
7	8 14 15	1 42·6	10 23 4	7 4·4
9	8 13 52	1 28·9	10 24 0	6 25·8
11	8 13 21	1 16·2	10 24 50	5 45·8
13	8 12 42	1 4·5	10 25 32	5 4·2
15	8 11 55	0 53·9	10 26 4	4 20·7
17	8 11 1	0 44·4	10 26 28	3 35·5
19	8 10 1	0 36·1 N.	10 26 45	2 48·8 S.

The brightness on December 19 of Faye's comet is 1·8 that of the brightness at discovery; of Barnard's, 0·9: Both comets change but slowly in brightness, as the distance from the earth is diminishing, whilst the distance from the sun is increasing.

THE SATELLITE OF NEPTUNE.—Mr. A. Marth pointed out in the *Monthly Notices* (vol. xlvi. p. 506), some two years ago, that the values for the inclination of the orbit of the satellite of Neptune and its ascending node, as deduced from the observations obtained in Malta in the years 1852 and 1864 by Lassell and Marth, and again at the Washington Naval Observatory from 1874 to 1884, show a well-marked, progressive, and regular change. Referring these values to the plane of the orbit of the planet, they are as follows :—

Date.	*n.* °	*i* (motion considered direct). °
1852	176·20	148·33
1864	180·41	146·19
1874	182·59	144·04
1883	184·31	142·38

Mr. Marth offered no theory by which to account for this change, but begged for continued observations. Prof. Asaph Hall also has more recently urged the necessity for further observation, and by fresh observers, avowing at the same time his suspicion that these changes are due to systematic errors in the observations. But M. Tisserand, in a late communication to the Academy of Sciences of Paris, shows that the changes can easily be explained by the theory of a slight flattening of the surface of Neptune. In this case the angle between the plane of the planet's equator and that of the orbit of the satellite will be constant, and the pole of the orbit will revolve in a small circle round the pole of the planet, a complete revolution taking more than 500 years. The inclination of the orbit of the satellite will be considerable, probably greater than 20°; but the flattening of the surface of Neptune can only be slight, too small to be detected by direct measurement. Further observations may enable the amount of the inclination to be more exactly determined, and, at the same time, will show whether the changes in question are due or not to this one cause alone.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *DECEMBER* 2–8.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 2

Sun rises, 7h. 48m.; souths, 11h. 49m. 49·0s.; sets, 15h. 51m.: right asc. on meridian, 16h. 36·8m.; decl. 22° 5′ S. Sidereal Time at Sunset, 20h. 39m.

Moon (New on December 3, 10h.) rises, 6h. 3m.; souths, 10h. 56m.; sets, 15h. 39m.: right asc. on meridian, 15h. 42·7m.; decl. 15° 23′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	6 26	10 51	15 16	15 37·8	18 26 S.
Venus ...	10 41	14 27	18 13	19 14·9	24 21 S.
Mars ...	11 18	15 24	19 30	20 11·8	21 25 S.
Jupiter ...	8 12	12 12	16 12	16 58·9	22 17 S.
Saturn ...	21 20*	4 46	12 12	9 32·3	15 38 N.
Uranus ...	3 6	8 31	13 56	13 17·3	7 30 S.
Neptune..	15 22	23 6	6 50*	3 55·4	18 36 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Dec	h.	
2 ...	9 ...	Mercury in conjunction with and 3° 23′ south of the Moon.
3 ...	17 ...	Jupiter in conjunction with and 2° 38′ south of the Moon.
3 ...	18 ...	Mars at least distance from the Sun.
6 ..	2 ...	Venus in conjunction with and 2° 4′ south of the Moon.
6 ...	22 ...	Mars in conjunction with and 0° 15′ south of the Moon.
8 ...	23 ...	Jupiter in conjunction with the Sun.

Saturn, December 2.—Outer major axis of outer ring = 42″·8 : outer minor axis of outer ring = 10″·0 : southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·4 ...	81 16 N. ...	Dec. 4,	23 46 *m*
Algol	3 0·9 ...	40 31 N. ...	,, 4,	6 15 *m*
			,, 7,	3 4 *m*
λ Tauri...	3 54·5 ...	12 10 N. ...	,, 4,	18 29 *m*
R Canis Majoris ...	7 14·5 ...	16 12 N. ...	,, 2,	23 33 *m*
			,, 4,	2 49 *m*
S Cancri	8 37·5 ..	19 26 N. ...	,, 6,	22 13 *m*
W Virginis	13 20·3 ...	2 48 S. ...	,, 2,	3 0 *m*
U Herculis	16 20·9 ...	19 9 N. ...	,, 8,	*M*
W Herculis	16 31·3 ...	37 34 N. ...	,, 4,	*M*
R Lyræ	18 51·9 ...	43 48 N. ...	,, 3,	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 4,	2 0 *m*
			,, 7,	1 54 *m*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 4,	6 0 *m*
			,, 5,	21 0 *M*

M signifies maximum ; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near η Persei	42 ...	56 N. ...	Very slow ; faint.
,, ζ Tauri...	80 ...	23 N. ...	Taurids II. Max. Dec. 6. Slow, brilliant.
,, δ Geminorum ...	110 ...	24 N. ...	Rather swift.
,, β Ursæ Majoris ...	163 ...	58 N. ...	Very swift ; streaks.

GEOGRAPHICAL NOTES.

At the usual meeting of the Royal Geographical Society on Monday, Mr. J. Thomson read before a large audience a paper on "A Journey to the Atlas Mountains." He gave a most interesting account of the scenes visited, in the course of his trip, by himself and his companion, Mr. Harold Crichton-Browne. Describing some of the practical results, he said they had ascended and crossed the Atlas Chain in no fewer than six different places besides making various subsidiary trips into the lower ranges. A large series of barometric and boiling-point observations have been taken, which would assist in forming a more accurate idea of the general elevation of the range. Several glens had been explored, and the head-waters of some important streams had been mapped out. New and important light had been thrown upon the geological structure of the mountains. A small collection of plants from the higher altitudes had been made, and finally a series of photographs (which were exhibited) of the mountains, the inhabitants, and their houses have been obtained. He had reached an altitude in the mountains 1300 or 1500 feet higher than any other traveller.

The December meeting of the Royal Geographical Society will be held on Monday, the 17th, at the University of London, instead of Monday, the 10th, as announced in the sessional programme. Colonel R. G. Woodthorpe will read a paper on explorations on the Chindwin River, Upper Burmah.

The Russian Expedition for the exploration of Tibet, organized by the late General Prjevalsky, is, notwithstanding the death of the famous explorer, to be despatched on its mission. This announcement was made by M. Semenow, the Vice-President of the Russian Geographical Society, at a meeting held by the Society in honour of General Prjevalsky.

THE RENAISSANCE OF BRITISH MINERALOGY.[1]

An ideal Presidential address should treat, not of a special point in the science of interest to a section only of the Society, but of the science in its broader aspects ; and the simplest permanent arrangement of this kind is that which makes it deal with the progress of the Society or of the science during the interval which has elapsed since the delivery of the next preceding address.

But in the case of our own Society we labour under special disadvantages, whether the address is to be on a specific subject or on the progress of the science. Not only is the Society small, but the number of its members able to devote any large part of their time to pure mineralogy is far smaller still. Hence if a set address were expected from the President we should be unnecessarily limited in our selection for that office. Many of our ablest members, men who would make the best of Presidents, men of wide culture and extensive general knowledge, men endowed with ideas and the power of expressing them, men who would bring to us a large experience obtained on the executive of other and larger Societies, though willing, nay anxious, to help us in the management of the affairs of the Society, would be prevented from giving us their services in the chair, owing to the sheer impossibility of devoting the requisite time and thought to the preparation of a purely mineralogical address such as they would consider worthy at once of the Society and of their own reputation.

Again, the number of our London meetings has been up to the present only three a year, and, as far as we can see, it is not yet desirable to increase their frequency. To set aside annually one-third, or even one-fourth, of the time of the Society for consideration of the views of the President, or of the progress and past work of the Society, would seem to be wanting in regard for proportion. A few minutes spent by the members in turning over the pages of the *Magazine* will give a better idea of the work of the Society, and be at the same time more exhilarating, than any summary a President can make. A report by the Council on the finance and general business, as brief as it can be made, seems to me sufficient for all reasonable purposes, and least wasteful of the time of the members and officers.

I have referred to the *Magazine*, and have said that a good idea of the work of the Society may be obtained in a few minutes by turning over the pages of one of its volumes. It may be cast in our teeth that the volume is small, but we can proudly and truly retort that few volumes of the same size furnish so vast an amount of heavy reading. The density, indeed, is prodigious—not that of lead, but of gold, refined gold. The volume is intended for transmission to all posterity, and not as a mere addition to the ephemeral literature and scientific gossip of a too prolific century. The present generation, by its careful use of the volume, will doubtless help it to reach its destination.

Bulky publications are, indeed, matters for shame rather than pride : they are the immediate ruin of a small Society, a perennial burden to librarians, and, as their contents are never completely indexed, a terror to subsequent investigators. The Ancients not unwisely refrained from the invention of printing ; they recognized that their duty to themselves was to read only what was worth the vast labour of transcription, and that their duty to posterity was to transmit to it only their masterpieces ; when even these became burdensome, an incendiary, doubtless a librarian, quickly reduced their volume. But for us Moderns the cost of multiplication of copies has become so small that everything, good or bad, is printed and preserved ; and it becomes necessary to spend the greater part of one's life in the preparation and study of indexes rather than of the literature itself. It would be an immense boon to mankind if some impartial and perfect tribunal could be empowered to do on the large scale what the curate and barber did so satisfactorily with Don Quixote's books on the small scale—distinguish the worthy from the worthless, and relentlessly annihilate that which ought not to cumber our shelves or demand even a passing glance.

I have said that the number of our members is small. I am not sure that it would be politic as yet to increase it. Members who have a living interest in mineralogy are most valuable, and of such we cannot have too many ; but mere subscribers of paltry gold would eventually be a source of weakness. After encouraging the Society to extravagant expenditure, they would fall away and leave it in the lurch.

[1] Extracted from an address by Mr. Fletcher, the retiring President, at the annual meeting of the Mineralogical Society, October 30, 1888.

But is it necessary that the number of people in this country with a lively interest in mineralogy should remain so small? We have only to look abroad to see at once that the cause of this smallness of number is not inherent in the subject itself. In Germany, for instance, everyone—of course, with here and there a conspicuous exception—seems to be either a Professor of Mineralogy or a student of it. Periodical publications, at once voluminous and teeming with valuable results of scientific work, are there maintained. Somewhat more than a century ago, the study of minerals was everywhere popular, and received its share of attention from the cultured classes. Students thronged to Freiberg from all parts of the world to hang on Werner's lips.

There is one reason for this decline of general interest in mineralogy which I may mention in passing. In the good old days minerals were named and classified by help of simple external characters, and the facts of the science could be easily grasped without much preliminary training. Since then, the invention of crystallography and of the atomic theory, and the discoveries made in physical optics, have brought about a vast change in the treatment of the subject, and the mineralogy of to-day is even in its elements beyond the range of ordinary mortals. The pages of its text-books are sprinkled with wonderful formulæ designed by perverse chemists, and with unpronounceable hieroglyphics maliciously invented by cruel crystallographers.

But the chief reason for the decline of mineralogical study in our own country is that mineralogy has been almost completely excluded from our educational system. In the older Universities, it is true, mineralogy has been long represented by Professors, but, until lately at least, it has been allowed scarcely any weight in the examinations for a degree. The study of other branches of science has been encouraged, while that of mineralogy has been neglected and forgotten. One of the evils of the examination system is that all the available energy of our youth is concentrated wholly on subjects upon which stress is laid by a not omniscient Examining Board; most students, too, must almost necessarily take up subjects in which there is an opportunity of showing their comparative ability, and by a knowledge of which there is reasonable prospect of being able to gain a future livelihood.

Taught only as a subordinate and unimportant subject at the Universities, and not taught at all outside, pure mineralogy has been in great danger of becoming extinct in this country: a few years ago the capsizing of a coach or the bursting of a balloon might have been the end, and the British Professor or student of pure mineralogy have become a mere tradition.

The discovery of the transparency of thin rock-sections, and the important conclusions which may be arrived at from their microscopical examination, have now turned attention to mineralogy once more, and it seems likely that the knowledge of mineral characters requisite for petrographical work may at last lead to our mineralogical renaissance. Owing chiefly to the patience and perseverance of Prof. Lewis, there is now a certain amount of encouragement to mineralogical study at Cambridge; and if the renaissance is to be brought about, the example of Cambridge must be followed by the other Universities, and mineralogy be assigned a higher place in the examinational system.

Minerals are omnipresent. Is it unreasonable to ask that everyone should be taught their simple characters, and be shown how to recognize such minerals as are met with at every turn? The teaching would improve the capacity for observation, and give fresh interest to many a pleasant ramble—through the workings of a mine. And is not Prof. Ruskin in the right when he claims that a knowledge of the minerals, conveniently grouped as precious and ornamental, should form a part of every gentle education? More especially ought we not to insist upon such elementary teaching for the numerous officials sent out by the nation to the less explored regions of the world?

In the higher teaching of mineralogy, difficulties present themselves, but they might easily be lessened by division of labour; a preliminary training in the elements of mathematics, physics, chemistry, and crystallography being absolutely necessary to the manufacture of a mineralogist. The teaching of crystallography is generally relegated to the Professor of Mineralogy, and the subject regarded as a mineralogical difficulty; but this ought not to be the case. It is true that a mineralogist was the first to discover a relationship between the various crystallized forms of the same substance, and thus to institute a crystallographic science, which has since been found indispensable in mineralogical study. It is true that the mineralogist has been the originator of every advance in crystallographic knowledge. It is true that the mineralogist has in minerals ready-made crystallizations, which in their excellence and variety of form can rarely be imitated in the laboratory. But it is no more the province of a mineralogist to teach crystallography than it is to teach chemistry or the use of a delicate balance. He does teach it indeed, but that is merely because his pupils reach him imperfectly trained for the pursuance of his own subject.

Crystallography should be taught as a special subject; and a knowledge of it should be required not only of the mineralogist but of the chemist, and even of the physicist. Hitherto, at least, the chemists of this country have been too content either to leave the crystalline forms of their artificial products undetermined, or to impose the task of their determination on the already sufficiently occupied mineralogist. It seems obvious that in a satisfactory system of education every chemist should be taught how to measure and describe the crystalline characters of the products which it is his fate to call into existence. On various occasions expression has been given to this view, but the only chemist who has yet seen his way to act upon it is Prof. Henry Armstrong, who, I am happy to say, has introduced the subject into the educational course of the City and Guilds Technical Institute. I trust that before another generation passes away his excellent example will be followed throughout the country. A knowledge of the elements of crystallography, including the mechanics of crystal measurement, ought to be made a *sine quâ non* for a degree in chemistry at every University.

The measurement of the angles of a crystal, the determination of its symmetry, and the calculation of its form, are infinitely less difficult than is generally imagined: given a knowledge of elementary mathematics and the careful use of measuring instruments, the processes are in general extremely simple. The complexity of crystallographic calculation is only apparent, and is due to the existence of text-books: they are generally worse than useless. A voluminous work on crystal-calculation is usually an attempt to provide formulæ which shall enable a student to solve every possible problem by rule of thumb, without his needing to have the faintest idea of what he is really doing. Practically, anyone familiar with the processes of trigonometry can deduce from first principles the formula required for each special case as it occurs in less time than he can discover the rule in the ponderous tome invented for his mystification.

I am, of course, far from asserting that the teaching of crystallography presents no difficulties at all: what I do wish to insist upon is that the kind of crystallographic knowledge requisite for the chemist in his own work is such that it may fairly be demanded of every one of them: the higher flights may be abandoned to the specialist.

At the present time, when Professors of Crystallography are not yet called into existence, there is one step which ought to be at once taken, and which would make mineralogy more possible eventually to a large number of our students. Every student of practical physics is taught how to measure with a reflecting goniometer the angles of an artificial prism: he should further be taught the measurement of the angles of a simple crystal, and the deduction of its symmetry. In his optical studies especially, such a practical knowledge of crystalline symmetry would be a great help to him. The reflecting goniometer in one of the forms used for crystallographic work might well be an instrument in use in every physical laboratory, and would subserve many a useful purpose. As soon as every physicist is taught how to determine the angles and symmetry of a simple crystal, and every chemist is further enabled to define the crystalline forms of his artificial products, we shall have a large army of students for whom the transition to mineralogical work will be easy; then, and not till then, can we hope for any useful increase in the number of the members of this Society; then, and not till then, can we hope that our country will in its study of mineralogy take its proper place among the nations of the earth.

One more point I may mention. Until a few years ago there were two distinct Societies, a Mineralogical and Crystallological: they had objects far from identical, and in a more perfect world might have flourished side by side. The fusion of the two Societies without any extension of the title of the Mineralogical has had for its effect that we cannot satisfactorily demand what the Crystallological could—namely, the support of the organic chemist; and although it was understood at the time of the fusion that papers on the crystallization of artificial products,

organic or inorganic, should be within the scope of the joint Society, their inclusion in a *Mineralogical Magazine* would suggest a misnomer. The change or extension of our already lengthy title has its evident inconveniences, but it may be worthy of careful deliberation by the Society at some early date as to whether such an extension is not really necessary for the clearer definition of our objects if we are to enlist the sympathy of many who, though they may feel to be beyond the pale of a Mineralogical Society, yet by their investigation of the crystalline forms of the products of the laboratory may in the future, as in the past, throw light on the crystallography of the Mineral Kingdom.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following gentlemen have been appointed examiners:—

Mathematical Tripos, Part II.: Dr. Routh (Chairman), Messrs. W. Burnside, J. Larmor, and A. R. Forsyth.

Natural Sciences Tripos and 2nd M.B.: Dr. Hill (Master of Downing), Prof. Cleland (Human Anatomy), Prof. Stirling, Dr. Gaskell (Physiology).

Natural Sciences Tripos: Prof. W. G. Adams, Mr. W. N. Shaw (Physics), Prof. W. A. Tilden, Mr. H. J. H. Fenton (Chemistry), Prof. Lewis, Prof. Story-Maskelyne (Mineralogy), Mr. F. Darwin, Prof. H. Marshall Ward (Botany), Mr. W. F. R. Weldon, Mr. F. S. Harmer (Zoology).

Natural Science Tripos and Special B.A.: Mr. W. W. Watts, Mr. A. Harker (Geology).

M.B. and Special B.A.: Prof. W. G. Adams, Mr. S. L. Hart (Elementary Physics), Mr. H. F. Neville and Mr. H. J. H. Fenton (Elementary Chemistry), Mr. F. Darwin and Mr. S. F. Harmer (Elementary Biology).

2nd M.B. only: Mr. Pattison Muir and Mr. H. Robinson (Pharmaceutical Chemistry).

Messrs. W. Carruthers, F.R.S., and J. E. Marr are appointed examiners for the Sedgwick Prize to be adjudged in 1892.

The University Lectureships in Botany and Advanced Human Anatomy, tenable for five years, are vacant. Candidates must send their names to the Vice-Chancellor on or before November 30.

Open Scholarships and Exhibitions will be competed for at the following Colleges, beginning on the undermentioned dates: Mathematics, Pembroke, December 11; Trinity Hall, December 11; Queens', December 12; Mathematics and Natural Science, Gonville and Caius, December 7; King's, December 10; Jesus, Christ's, and Emmanuel combined, December 11; St. John's, December 11; Trinity, December 11; Sidney Sussex, January 1. The tutors will give full information.

The Sheepshanks' Astronomical Exhibition will be competed for on December 10 and 11 at Trinity College.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 15.—"Combustion in Dried Oxygen." By H. Brereton Baker, Dulwich College, late Scholar of Balliol College, Oxford. Communicated by Prof. H. B. Dixon, F.R.S.

In 1884, some preliminary experiments, published in the Journal of the Chemical Society, convinced me that moisture exerted an important influence on the combustion of carbon. Since that time experiments have been made, not only with that element but with several others, and the same influence seems to be exerted on the combustion of some, while no such influence could be detected in the case of other elements. It was discovered very early in the investigation that hydrogen, both free and combined, aided the union of carbon with dried oxygen, and therefore, for the new experiments on this and other elements, special attention was devoted to their purification from hydrogen. It was found that two of these elements, amorphous phosphorus and boron, had, like carbon, a very great power of occluding hydrogen. To eliminate it some of the elements were heated in a current of pure chlorine, while others were heated in sealed tubes with the chlorides of the elements, special precautions being taken to free the purified elements from all traces of the agents used in their purification. In this way the elements—carbon, sulphur, boron, and phosphorus, the latter in both red and yellow modifications—were found to have their combustion influenced by the dryness of the oxygen. Some chemical union was found to take place, the extent of which varied with the dryness of the substances. In no case, however, did it manifest itself by flame. Ordinary phosphorus was obtained so pure as not to glow in the oxygen dried by phosphorus pentoxide, though the pressure was increased and diminished in every possible way. If water was added, rapid combustion at once set in.

The elements—selenium, tellurium, arsenic, and antimony—were purified with as much care as was expended on the elements mentioned above. Their combustion was, however, not found to be affected in any way by the dryness of the gas.

In the course of the investigation two facts were discovered about the combustion (1) of amorphous phosphorus, and (2) of carbon in oxygen. Amorphous phosphorus is generally regarded as being incapable of true combustion. It is asserted that before amorphous phosphorus can be heated to its kindling point, it changes into ordinary phosphorus, which then burns. This has been proved not to be the case. Amorphous phosphorus was heated in a current of nitrogen, free from traces of oxygen, to 260°, 278°, and 300° in three experiments, without undergoing any change to the ordinary modification. If moist oxygen was substituted for the nitrogen, combustion took place at 260°. It seems, therefore, probable that amorphous phosphorus undergoes a true combustion in oxygen without previous change to the ordinary modification.

With regard to the combustion of carbon, it has always been a doubtful question which of the two oxides is first formed. Is carbon monoxide the first product, undergoing further oxidation to the dioxide, or is carbon dioxide the first and only substance formed? The problem seems incapable of direct solution. It is, however, open to indirect attack. When carbon is heated in a current of partially dried oxygen, a slow combustion goes on, and, though the oxygen is in excess, both oxides are produced. The amount of monoxide, however, is twenty times the amount of the dioxide. Experiments also show that this occurs at temperatures at which dry carbon dioxide is not reduced by carbon. The carbon monoxide must, therefore, be produced by the direct union of its elements, its further oxidation being prevented by the dryness of the gases. Confirmatory experiments were performed in which carbon monoxide was found to be produced by the slow combustion of carbon in air at 440°, a temperature too low for the reduction of the dioxide by carbon. It is probable that the ordinary combustion of carbon goes on in two stages, that carbon monoxide is first produced, and, if circumstances are favourable, this is further oxidized to carbon dioxide.

"On the Secretion of Saliva, chiefly on the Secretion of Salts in it." By J. N. Langley, M.A., F.R.S., Fellow of Trinity College, and H. M. Fletcher, B.A., Trinity College, Cambridge.

Heidenhain has shown that when saliva is obtained by stimulating the chorda tympani, the percentage of salts in the saliva depends upon the rate of secretion, so that the faster the secretion the higher the percentage of salts is up to a limit of about 0·6 per cent.

The authors do not find any rate of secretion beyond which an increase in rate fails to increase the percentage of salts in the saliva. The increment in the percentage of salts decreases, however, with each equal successive increment in the rate of secretion.

As a rule, in saliva obtained by injecting pilocarpine, the percentage of salts follows Heidenhain's law; the exceptions are probably due to the action of pilocarpine upon the circulation.

The percentage of salts in saliva obtained by stimulating the sympathetic is higher than corresponds to its rate of secretion, the saliva obtained by stimulating the chorda being taken as a basis of comparison; this sympathetic saliva may be secreted at $\frac{1}{100}$th of the rate of chorda saliva, and yet contain very nearly as high a percentage of salts.

Dyspnœa decreases the rate of secretion of saliva with a given stimulus, and if not too prolonged, increases the percentage of salts, and tends to increase the percentage of organic substance in the saliva.

Clamping the carotid during secretion has the same general effect as dyspnœa, but it causes a still more marked increase in the percentage of salts. Its after-effect is also much greater, and lasts longer.

Bleeding has a similar effect to dyspnœa and to clamping the carotid, but its most marked effect is an increase in the percentage of organic substance.

Injection of dilute salt solution, NaCl, 0·2 to 0·6 per cent., in sufficient quantity, considerably increases the rate of secretion of saliva; the percentage of salts in the saliva decreases, although the rate of secretion of salts usually increases; the percentage of organic substance decreases; that is, increasing the volume of the blood with dilute salt solution chiefly increases the rate of secretion of water.

Injection of strong salt solution increases the percentage of salts in saliva. This is in accordance with the recent observations of Novi, that the chlorine in the salts of saliva is increased for a given rate of secretion by increasing the percentage of sodium chloride in the blood.

Saliva produced by stimulating the chorda tympani, or by injecting pilocarpine, after a small dose of atropine has been given, contains a low percentage of organic substance and of salts.

The authors, like Werther, find that sub-lingual saliva has a considerably higher percentage of salts than sub-maxillary saliva.

If lithium citrate, potassium iodide, potassium ferrocyanide, and pilocarpine are injected into the blood, lithium can be detected in the first drops of saliva secreted, potassium iodide after the first six drops; potassium ferrocyanide cannot be detected at any stage of secretion.

The general result of these experiments is to show that the secretion of water, of salts, and of organic substance are differently affected by different conditions, and that the percentage composition of saliva is determined by the strength of the stimulus, by the character of the blood, and by the amount of blood supplied to the gland.

All, or nearly all, the arguments which have been adduced to prove that the secretion of organic substance is governed by special nerve-fibres, have their counterparts with regard to the secretion of salts, so that we might imagine at least three kinds of secretory fibres to be present. The experiments, on the whole, indicate that this complicated arrangement does not exist, but that the stimulation of a single kind of nerve-fibre produces varying effects according to the varying conditions of the gland cells.

Linnean Society, November 15.—Mr. W. Carruthers, F.R.S., President, in the chair.—On behalf of Mr. H. Bolus, Mr. J. G. Baker exhibited a specimen of *Eriospermum folioliferum*, a plant showing a very remarkable type of leaf-structure. It was figured by Andrews in his "Botanist's Repository" in 1807, and lost sight of until recently refound by Mr. Bolus in Namaqualand.—Prof. Stewart exhibited a substance which had been picked up on the sea-shore, the nature of which it had puzzled many to determine, its structure being regarded by some as animal, by others as vegetable. He proposed to submit it to careful microscopical examination.—Mr. J. E. Harting exhibited a South American bat from Trinidad (*Noctilio leporinus*), alleged to be of piscivorous habits, and remarked upon a similar habit which had been observed in a species of *Pteropus* in India.—A paper was read by Mr. B. D. Jackson, on behalf of Mr. H. Chichester Hart, on the mountain range of plants in Ireland, and was criticized by Mr. J. G. Baker, who gave an interesting sketch of the characteristics of the Irish flora.—Two papers were then read, by Mr. Sladen, on the mammals and birds collected by Mr. H. N. Ridley in Fernando Noronha, in the determination of which the author had been assisted by Mr. O. Thomas and Mr. R. B. Sharpe.

Physical Society, November 10.—Prof. Reinold, President, in the chair.—The following communications were read:—On the calculation of the coefficient of mutual induction of a helix and coaxal circle, by Prof. J. V. Jones. In arranging some experiments for determining resistance absolutely by Lorenz's method, the author had occasion to consider what form of standard coil was most suitable for accurate calculation, and chose a helix of large diameter with a single layer of wire. To obtain a sufficient number of turns requires considerable axial length, and Lord Rayleigh's approximate method of calculating the coefficient was found to be insufficient where an accuracy of 1/100 per cent. is required. A method of calculation is given considering the wire as a helix whose equations are $y' = A \cos \theta'$, $z' = A \sin \theta'$, and $x' = k\theta$, those of the circle being $y = a \cos \theta$ and $z = a \sin \theta$. Applying the formula obtained to a circle of 10 inches diameter placed concentric with a helix of 20 inches diameter and 4 inches long, the value obtained is $M = n \,.\, 53{\cdot}259$, whereas Lord Rayleigh's formula gives $n \,.\, 53{\cdot}317$. Dr. Fleming described a wooden anchor ring wound like a gramme armature, and having a secondary coil added, which he had devised as a standard of mutual induction, and used for determining capacity absolutely. In reply, the author said he had not considered the wire to have thickness, as he felt sure this would not affect the result for his coil by one part in 100,000. With respect to Dr. Fleming's anchor ring, he considered the difficulty of winding it sufficiently uniformly to be a great drawback to its general adoption.—On the upper limit of refraction in selenium and bromine, by Rev. T. Pelham Dale, read by Mr. Baily. In a former paper (read February 11, 1888) the author showed that an upper limit of refraction for selenium should theoretically exist about the middle of the visible spectrum, and the present communication describes some experiments which tend to confirm the prediction. On placing a thin film of selenium under a spectro-microscope, it was found to be opaque to rays above the green, and previous calculation had given 5295·7 as the limiting wave-length transmissible. Sulphur at ordinary temperatures should have its upper limit beyond the visible spectrum, but theory indicates that increased temperature will lower the limit. It is well known that sulphur darkens when heated, and when a film of boiling sulphur was examined under the spectro-microscope, all but the red end of the spectrum was absorbed. On cooling, the region of absorption gradually retreated towards the violet end. Selenium is also found to become more transparent as it is cooled, and its refractive equivalent is equal to that of sulphur multiplied by the ratio of its chemical equivalent to its density. Important optical as well as chemical relations thus exist between the two elements. The results obtained by bromine films were remarkably similar to those of selenium, the violet rays being entirely cut off. A method of solving the equation $a \sin \theta = \sin m\theta$ (on the limiting solution of which the upper limits of refraction depends), by a table of Eulerian integrals, is given in the paper, and an analogy between total reflection and the upper limit of refraction is traced.—Experiments on glass in polarized light, by Prof. S. P Thompson.—On a new form of standard resistance coil, by Dr J. A. Fleming.

Chemical Society, November 1.—Mr. W. Crookes, F.R.S., President, in the chair.—The following papers were read:—The constitution of the terpenes and of benzene, by Prof. W. A. Tilden, F.R.S. When oxidized under similar conditions with dilute nitric acid, the natural terpenes—australene, terebenthene, and hesperidene—yield less than 2 per cent., dipentene (terpilene) yields 27·6 per cent., and cymene and paraxylene yield 73 to 80 per cent. of paratoluic acid. Each of the four terpenes combines with two molecular proportions of bromine, and no more. Camphene, however, does not combine with bromine, and hence must be regarded as saturated in the usual sense. The author concludes that since the terpenes contain at most four units of available combining capacity, the nucleus of six carbon atoms which they all undoubtedly contain must be united into a closed chain containing at the most two double "bonds." Kekulé's benzene formula is a well-known representation of a ring of six carbon atoms, but must be abandoned, since the author considers that the terpenes are certainly not benzene derivatives. Kekulé's formula is open to the objection that it represents benzene as containing "ethylenic" carbon, for which there is no evidence at all. Moreover, a body of this formula, when treated with nitric acid, ought to yield abundance of oxalic acid. This the terpenes do, but the benzenoid hydrocarbons do not. Referring to this last statement, Dr. Japp, F.R.S., said that phenol on oxidation with alkaline permanganate gave a considerable quantity of oxalic acid; Mr. Groves, F.R.S., added that oxalic acid is obtained in quantity on oxidation of chloranilic acid; and Dr. Perkin, F.R.S., remarked that he had obtained a quantity of oxalic acid in preparing picric acid from phenol. Dr. Perkin also said that the low magnetic rotatory power of American turpentine was a probable indication of the non-existence of a nucleus of six carbon atoms. Mr. Wynne remarked that the production of α-naphthol by the distillation of phenylisocrotonic acid, a compound in which "ethylenic" carbon was undoubtedly present, might be quoted in support of Kekulé's benzene formula; and Prof. Armstrong, F.R.S., expressed the opinion that the evidence at disposal was entirely insufficient to enable us to determine the constitution of the terpenes with any degree of probability.—Some new compounds of magnesia with the halogens, a contribution to the

study of the electrolysis of magnesium chloride solution, by Messrs. Cross and Bevan. The authors find that the white substance separated at the cathode under the condition that the solution is not kept in circulation is a chloroxygen compound of magnesium, probably a magnesium hypochlorite. Similar compounds are formed on electrolyzing solutions of magnesium bromide and iodide.—The heat of dissolution of various substances in different liquids, by Mr. S. U. Pickering. The heats of dissolution of the nitrates and chlorides of calcium and lithium in water and in alcohol, and of bromine and iodine in various liquids, have been determined; and the results are held to support the author's view that the affinity of the radicles composing a salt molecule is not entirely saturated by their combination, and that the "residual affinity" of one of these radicles becomes entirely saturated by the solvent; the heat of combination of the atoms and the heat of dissolution of the molecule which they form being thus parts of the same chemical operation.—The criteria of plane and axial symmetry, by Prof. Armstrong, F.R.S. Wislicenus, in his now widely-known paper on the space arrangement of the atoms in carbon compounds, has termed compounds of the form $\begin{array}{c}\mathrm{a.C.b}\\ \Vert\\ \mathrm{b.C.a.}\end{array}$ *axially symmetric*, and those of the form $\begin{array}{c}\mathrm{a.C.b.}\\ \Vert\\ \mathrm{a.C.b.}\end{array}$ *plane symmetric*. In allocating these formulæ, he has argued, in a case such as is afforded by the two tolane dichlorides, for example, that the compound of higher melting-point (143°) is necessarily the plane symmetric modification, as it is produced on chlorinating tolane $\begin{array}{c}C_6H_5.C\\ \vdots\\ C_6H_5.C\end{array} + Cl_2 = \begin{array}{c}C_6H_5.CCl\\ \vdots\\ C_6H_5.CCl\end{array}$, and from the three possible configurations of tolane tetrachloride has arrived at the conclusion that on reduction it should yield the axially symmetric tolane dichloride in the larger quantity. Blank's experiments (*Annalen*, ccxlviii. 1) show that the two isomeric dichlorides are produced nearly in the proportion of two parts of that of lower melting-point (63°) to one of that of higher melting-point, and the axially symmetric structure is therefore, it would seem, as a matter of course, assigned by him to the dichloride melting at 63°. The author regards these views as being based on pure assumptions, and shows that all the facts at present known tend to prove that symmetric compounds—such as the para- or symmetric di-derivatives, or the symmetric tri- and tetra-derivatives of benzene, or the axially symmetric $\alpha\alpha$ or $\beta\beta$ modifications in the case of isomeric di-derivatives of naphthalene—have always the highest melting-point. The tolane dichloride of higher melting-point is therefore probably the axially symmetric modification. The majority of "alloisomerides" are compounds containing unsaturated carbon usually in association with one of the halogens or oxygen, and the author points out that it does not appear that this circumstance has yet been taken into account, or that the extent to which the peculiarities manifested by the negative elements are concerned in and may condition the isomerism has been in the least considered. The symbolic system introduced by Van 't Hoff, and adopted by Wislicenus, tends, moreover, to withdraw attention from the consideration of the possible effect of the peculiarities referred to, inasmuch as a "double bond" is represented as the precise equivalent of two, and a treble bond as that of three single bonds; which all observations show is a misrepresentation of the facts.—Derivatives of methylindole, by Dr. H. G. Colman.—Acetamide and phenanthraquinone, by Dr. A. T. Mason.—The action of ethylenediamine on succinic acid, by the same.

PARIS.

Academy of Sciences, November 19.—M. Janssen in the chair.—On the "Collection of the old Greek Alchemists," by M. Berthelot. The parts now presented to the Academy complete the publication of the Greek text and French translation of this great work. Part v. contains technical treatises of special interest on the goldsmith's art, the tempering and colouring of bronze and iron, bronze casting, iron-gilding, the preparation of gold-leaf, the colouring of artificial gems, the treatment of pearls, the preparation of lye from ashes, beer, soap, &c. Most of these treatises appear to have formed part of a great work on practical chemistry composed in the eighth and ninth centuries, and several are written in the Byzantine dialect. But some are of great antiquity, amongst others one dealing with the phosphorescence of precious stones.—On the satellite of Neptune, by M. F. Tisserand.—On the latitude of the Gambey mural circle at the Paris Observatory, by M. H. Faye. A method is proposed by which the exact latitude of the instrument itself may be directly determined, and the results controlled which have been obtained with other processes by M. Périgaud in the present year, and by others at earlier dates. M. Périgaud determines the latitude of the Observatory as 48° 51′ 10″·9.—Note on the stability of the French seaboard, by M. Bouquet de la Grye. The results of the comparative observations taken for some years past at the ports of Brest, Cherbourg, and Havre, tend mainly to confirm the general researches of Colonel Goulier. From the comparative tables it appears that the mean level diminishes in the direction from Brest to Havre; at Havre the annual subsidence seems to be about 2 mm., at Cherbourg 1 mm., at Brest nearly absolute stability.—A study of submarine navigation, by M. A. Ledieu. A survey is given of the futile efforts made in this direction previous to the invention of Whitehead's regulating pendulum in 1872; which, combined with M. Joseph Farcot's servo-motor, gave the true solution of the problem. From that time dates all real progress in submarine navigation.—On various methods of treating rabies, by M. Odo Bujwid. Since his visit to M. Pasteur's establishment in 1886, M. Bujwid has been treating persons bitten by dogs, either mad or suspected of being mad, in his laboratory at Warsaw. At first he followed the simple processes of inoculation of M. Pasteur, and of M. Frisch, of Vienna, with some failures in both cases. But during the last sixteen months he has adhered exclusively to the intensive or severe treatment, which has been applied to 370 patients without a single fatality.—Observations of Palisa's new planet (281), and of Barnard's comet (October 30, 1888), made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Rambaud, Sy, and Renaux. The observations of the planet are for the period from November 3 to November 8; those of the comet from the 5th to the 8th of the same month.—On the subsidence of the land in France, by Colonel Goulier. In reply to General Alexis de Tillo's remarks on his previous note, the author points out that his conclusions on this subject are not put forward as final, but only as a probable hypothesis to be accepted until proved untenable.—On mountain-ranges and their relations to the laws of deformation of the terrestrial spheroid, by M. A. de Grossouvre. On purely theoretical considerations the author arrives at the conclusion that the zones of foldings are progressively receding southwards. This conclusion is entirely in accordance with the observed facts connected with the dislocation of the earth's crust, and the regularity of the phenomenon is regarded as a confirmation of the theory of the primitive fluidity of the globe.—On equalities of two degrees, by M. Michel Frolov. These researches have reference to the properties of groups of *n* numbers, whose first and second powers give respectively equal sums, properties which have not yet been studied by any other arithmetician.—Maximum spectrum of Mira Ceti, by Mr. J. Norman Lockyer.—On the mutual relations of meteorites and shooting-stars, by M. Stanislas Meunier.—Note on the tensions of various vapours, by M. Ch. Antoine.—On the decomposition of the haloid salts of silver under the influence of light, by M. F. Griveaux.—Hydrochlorates of benzidine: their dissociation by water, by M. P. Petit.

BERLIN.

Physical Society, November 2.—Prof. von Helmholtz, President, in the chair.—Dr. Brodhun gave an account of experiments which he had made, in conjunction with Dr. König, for the purpose of testing the fundamental law of psychophysics in connection with the sense of sight. These have already been described by Dr. König in a communication to the Physiological Society, previously reported in NATURE (vol. xxxviii. p. 464). In the discussion which ensued, the President spoke on the sensation of light derived from the resting retina (*Eigenlicht*). In connection with the preparation of the new edition of his "Physiological Optics," he had made experiments and calculations which showed that the *Eigenlicht* is much greater than has hitherto been supposed. Taking as a standard the unit used by König and Brodhun in their researches, the *Eigenlicht* has a magnitude of forty to sixty such units. It is characterized by the irregular brightness of the field of vision, giving rise to the sensation of a more or less coarsely punctated image with lighter and darker patches; it further leads to the result that with equally small intensities of illumination large objects are seen more easily than

small, so that the liminal intensity (*Schwellenwerth*) for small surfaces is considerably greater than for large.—Prof. Kundt exhibited a large number of photographs of spectra which had been prepared in his laboratory with a view to testing the action of light-absorbing substances on dry-plates. Spectra photographed on dry-plates which had been coloured with chlorophyll were especially interesting. They consisted of a bright strip ending near F, followed by a dark portion intercepted by an extremely bright line at the spot where the absorption-band of chlorophyll is present in the red. Plates stained with eosin similarly showed a bright strip corresponding to the absorption-band of this substance in the yellow, whose brightness was much greater than that of the rest of the spectrum. These experiments showed that the rays of light which are absorbed by the above colouring-matters exert an extremely active chemical action on the plates. Experiments made with a view to determining whether absorption of light has a similar effect on fluorescence yielded negative results. It still remains to investigate whether the maximum brightness of the spectrum photographed on a plate stained with chlorophyll corresponds exactly with the absorption-band of this substance, taking into account at the same time the influence of the solvent used for the solution of the chlorophyll on the position of its absorption-band. Prof. Kundt then exhibited a simple form of bolometer which, based on the principle described by Dr. R. von Helmholtz at a recent meeting of the Society, may be easily and cheaply constructed out of thin tinfoil, and is specially adapted for lecture experiments. The same speaker finally exhibited two selenium cells constructed by Uljanin, of Strasburg, in July 1887, and still extremely sensitive to the action of light.

Meteorological Society, November 6.—Dr. Vettin, President, in the chair.—Dr. Kremser gave an account of meteorological observations made during a balloon voyage on June 23 of this year. For the purpose of carrying out scientific observations on the lines of Welsh and Glaisher in England, and of Tissandier in France, von Siegsfeld had constructed a balloon fourteen metres in diameter.—Dr. Assmann described some ice-filaments which he had observed in a valley of the Harz Mountains during a three days' frost in October.

Physiological Society, November 9.—Prof. du Bois-Reymond, President, in the chair.—Dr. Virchow described, as based on his own original researches, the development of the cylindrical epithelium of the yolk-sac, thus amplifying the communication made at the previous meeting of the Society on the development of the blood, blood-vessels, and yolk-sac of the chick. He distinguished seven stages in the development of the epithelium, each of which he separately described and explained by means of drawings, diagrams, and preparations, which he exhibited.—Dr. Martius made a communication on new experiments which he had carried out with a view to refuting certain attacks which had been made on his investigation of cardiograms as laid before the Society in the previous year. It may be remembered that the speaker had connected the curves traced by the impulse of the apex of the heart on a rotating cylinder with the various phases of a cardiac cycle, by auscultating the heart, and recording each sound he heard by a mark on the same rotating drum. Since he had assured himself that he could record the above without any irregularity due to his own reaction-time, he considered that the point on the cardiogram which corresponded with the mark for the first sound of the heart was synchronous with the closure of the auriculo-ventricular valves: the point corresponding to the mark indicating the second sound of the heart he regarded as coincident with the closure of the semilunar valves. Objections were made to the above method, and the possibility of the observer's reaction-time not being zero was left out of account. In answer to these, Dr. Martius has now made some new experiments with a pendulum beating seconds which closed an electric circuit as it passed through the vertical: the current thus produced attracted an armature, made a clapper-like noise, and recorded the instant of its production on a rotating drum. A second circuit was arranged for momentary closure by the observer, yielding a recording mark on the same drum: this circuit was closed regularly each time the clapper was heard, and the two marks made in the way indicated above coincided to within differences of time of at most 0·03 of a second, an exactitude which amply sufficed for the investigation of the cardiograms. It may be remarked here, as a matter of theoretical interest, that the above is not a case really involving reaction-time, but only the recording of known signals which are repeated rhythmically at regularly recurring intervals. With these rhythmic stimulations the duration of the interval is usually *estimated*, and in such cases the variations in the time-value of the estimate are mostly greater than those just quoted. When, however, after some time the rhythm of the stimulations has been fully grasped by the observer, then the interval is no longer estimated, but the record is made with an exactly similar rhythm.

STOCKHOLM.

Royal Academy of Sciences, November 14.—Musci Asiæ borealis, first part, Livermosses, by Prof. G. O. Lindberg and Dr. H. W. Arnell.—The disguise of the *Decapoda oxyrrhina* through special adaptations of the structure of their body, by Dr. Carl Aurivillius.—On the bladders of the Fucaceæ, by Dr. N. Wille.—Mineralogical notes, by Herr G. Flink.—On the journey of Dr. Nansen on the inland ice of Greenland, by Baron Nordenskiöld.—Contributions to the knowledge of the Cestodean worms which occur in Sweden, by Herr E. Lönnberg.—On a monstrous individual of *Cottus scorpius*, by Herr E. Nyström.—Annotations of some Scandinavian Pyrenomycetæ, by Herr K. Starbäck.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Meteorological Observations at Stations of the Second Order for the year 1884 (Eyre and Spottiswoode).—A Sketch of the First Principles of Physiography: J. Douglas (Chapman and Hall).—Rocks and Soils: H. E. Stockbridge (Trübner).—The Floating Island in Derwentwater: G. J. Symons (Stanford).—Prodromus of the Zoology of Victoria, Decade xvi.: F. McCoy (Trübner).—The Principles of Cancer and Tumour Formation: W. R. Williams (Bale).—The World's Inhabitants: G. T. Bettany (Ward, Lock).—A Treatise on Hydrodynamics, vol. ii.: A. B. Basset (Bell).—Results of Observations at Stonyhurst College Observatory, 1887: Rev S. J. Perry.—Morphologisches Jahrbuch, 14 Band, 3 Heft (Williams and Norgate).

CONTENTS.

THURSDAY, DECEMBER 6, 1888.

PRJEVALSKY'S FOURTH JOURNEY TO CENTRAL ASIA.

From Kiakhta to the Sources of the Yellow River, the Exploration of the Northern Borders of Tibet, and the Journey viâ *the Lob-nor and the Basin of the Tarim.* By N. M. Prjevalsky. (In Russian.) With 3 Maps, 29 Phototypes, and 3 Woodcuts. (St. Petersburg: Published by the Russian Geographical Society, 1888.)

IN this large quarto volume, which the indefatigable traveller brought out before leaving St. Petersburg for his proposed fifth journey to Central Asia, we have a general account of the journey he began at Kiakhta on November 20, 1883, and ended two years later at Lake Issyk-kul, in Russian Turkestan, after having visited the upper parts of both the Yellow and the Blue Rivers. As to the purely scientific description of the invaluable collections which were brought in by the Expedition, it has been undertaken by specialists and will be published separately.

Prjevalsky's aim during his fourth journey was to reach Lhassa, in Tibet; and it was in the hope that he might more easily make his way to that city that he started from Kiakhta, instead of taking the shorter route *viâ* Turkestan. His plan was to be at Lake Kuku-nor early in the spring; to buy provisions there, and to have them stored somewhere in South Tsaidam; and thence to proceed southwards, and to cross the north-eastern border-ridges of Tibet. The third part of the scheme could not be accomplished; and, after having visited the Upper Hoang-ho and the Upper Yang-tzse-Kiang (the Dy-tchu), Prjevalsky was compelled to return to his store-house, and thence he proceeded home *viâ* Lob-nor and East Turkestan. But the Expedition has thrown so much light upon the orography, the flora, the fauna, and the inhabitants of the outer terraces of Tibet, as well as of Eastern Turkestan, that one can hardly regret that the Russian traveller did not succeed in crossing the highlands to the north of the holy city of Buddhism.

The chapters devoted to the journey to the west of Kuku-nor will certainly be the first to attract the geographer, for only very brief accounts of that part of Prjevalsky's journey have hitherto been published. But the physical geographer and naturalist will also read with interest the pages devoted to the winter journey across the Gobi, to its terrible sand-storms, and the geological work of these storms, and still more the remarks upon the flora and fauna of the Nian-shan plateaus and the spring migrations of birds about Kuku-nor. It was in the Nian-shan that a new species of antelope, the *A. cuvieri*, was discovered. As seen from the drawing given by Prjevalsky, it differs widely from the next kindred species, the *A. gutturosa*.

It was only on May 13, 1884, that the Expedition reached the spot, Dzun-zasak, in South Tsaidam, where a store-house had to be erected. After having overcome considerable difficulties in getting camels and provisions, it started southwards in order to reach Lhassa. It crossed, first, the Burkhan-buda ridge, which rises like a wall from 7000 to 7500 feet high over the plateau of Tsaidam, and reaches the height of nearly 17,000 feet, without, however, attaining the limits of perennial snow. The Burkhan-buda is a border-ridge; it fringes the next and higher terrace of Northern Tibet, the lowest parts of which are from 3000 to 4000 feet higher than the Tsaidam plateau; and therefore it has but a gentle and short slope towards the south. At its southern foot, Prjevalsky found a broad valley, 13,400 feet above the sea, where flocks of wild *yaks* (*Poëphagus mutus*, a new species discovered by Prjevalsky) and *kuku-yamans* (*Pseudois nahoor*) live on the few grazing-grounds which can be found at such a height. A low range of hills separates this valley from the broad depression of Garmatyn, or Odon-tala, where the Expedition reached, on May 29, the upper part of the Hoang-ho and its two lakes, Jirin and Orin, so well known from Chinese sources, but never before visited by a European. "Now we saw the birth-place of the great river, we drank its water, and there were no bounds to our rejoicing," wrote the traveller in his note-book. He tried, of course, to explore the lakes, but a terrible snowstorm overtook the exploring party. Next day the ground was covered with two feet of snow, and the thermometer showed a frost of – 23° C., on June 1, under the 35th degree of latitude! The difficulty of moving on camels under such conditions may be easily imagined. But hunting was excellent, and Prjevalsky's companions killed numbers of the Tibetan bear (*Ursus lagomyiarius*). Alpine meadows, 16,000 feet above the sea, spread towards the south, but high mountain-ridges separate them from the Dy-tchu (the Upper Yang-tzse-Kiang), and it was only on June 22 that the party reached the Dy-tchu, in 33° 47′ N. lat., and 95° 54′ E. long. It flows in a narrow valley, at a height of 13,100 feet above the sea, and soon enters a narrow gorge between high mountains. The camels were in very bad condition, and there were no means of moving down the Dy-tchu, as it entered a narrow stony gorge. Neither was it possible to cross the rapid river, so that Prjevalsky was compelled to return. On the return journey the Expedition explored both the lakes of the Upper Hoang-ho, but it had to carry on regular fighting with several hundreds of Tangutes. It was thus obliged to abandon the exploration of the northern shores of the lakes, and to return to the store-house.

The attempt to reach Lhassa by this route thus proved a failure; and, in our opinion, a worse route could not have been chosen. True, while going due south from Dzun-zasak, Prjevalsky reached the lakes of the Upper Hoang-ho, and solved a great geographical problem; but then he had before him a mountainous region which he would have had to cross in an oblique direction—the series of ridges which accompany the great border-ridge of Eastern Tibet undoubtedly having a direction from the south-west to the north-east. Even if he had had horses, instead of camels, he would have met with insuperable difficulties. The fact is, that, having no knowledge of the orography of the highlands of Eastern Tibet, geographers draw on our maps the mountains of the Amdo as running *between* the problematic courses of the rivers which flow from Central Asia to Burmah and China; the ridges thus have on our maps a south-eastern direction, while in reality there is the greatest probability in favour of the mountain-ridges having a north-eastern direction, and being *pierced* by those rivers. The great border-

ridges of the great Central Asian plateau, which are known under the name of Great Khingan in the north, cannot terminate in the latitude of 35°. Probably they continue south-west as far as the Himalayas, and so it is most likely that the region of the Upper Yang-tzse-Kiang is filled with mountains running from the south-west to the north-east.

This view is supported by the results of Prjevalsky's exploration. In 1880, when he took from Dzun-zasak a south-western direction, and thus followed the foot of the highlands, he reached the Khara-usu with comparative ease. But when he went southwards and reached the Dy-tchu under the 96th degree of longitude, he could move neither east nor south. In the east he saw the gorges by which the Dy-tchu, which flows south-east, pierces the first of the series of the mountains that accompany the border-ridge. And further south he saw before him the continuation of the same ridge, and obviously could not cross it in an oblique direction.

All this becomes obvious when one attentively examines the map which accompanies Prjevalsky's last work. In fact, we see on that map that the two great lines of upheavals—south-west to north-east, and north-west to south-east—which so distinctly appear in Turkestan, the Caucasus, and Siberia (the chains running south-west to north-east being more ancient than those which run in the other direction) are as well pronounced in the southern parts of the great Central Asian plateau as they are in its northern and eastern parts. The whole of Central Asia appears, in fact, as a series of plateaus of various heights, which—very much like Bohemia in Europe—are bordered by chains having a direction from either south-west to north-east, or north-west to south-east.[1]

Although the Expedition failed to reach Lhassa, its observations on the climate, flora, and fauna of Northern Tibet are invaluable. The climate of the region; its poor vegetation, which has every year only a few days for developing its flowers and seeds, and nevertheless is strikingly rich in the numbers of its species; the mixture of Chinese, Himalayan, Tibetan, and Central Asian species which one finds in these highlands,—all this renders the borderlands of Tibet in Prjevalsky's description almost more interesting than the plateau itself.

We shall only refer to that part of the journey during which the Expedition moved north-westwards from the Dzun-zasak store-house to the small lake Gas, at the foot of the ridges Tsaidam, Garynga, Torai, Columbus, and Marco Polo's, which connect the Altyn-tagh with the yet unnamed highlands of the Upper Hoang-ho. Most interesting facts as to the nature of Tsaidam will be found in the chapter devoted to that part of the journey. Prjevalsky's winter excursion from Lake Gas enabled him to obtain an insight into the structure of the border-ridges which separate Northern Tibet from the lower terrace of Tsaidam (9000 feet above the sea). No less than three parallel ridges constitute that border-region. The outer ones are known under the names of Tsaidam, Garynga, and Torai; but these are not the highest of the series. The highest summits are gathered in the next row of mountains—the Columbus, Marco Polo's, and Prjevalsky's ridges (this last name has been given to it by the Russian Geographical Society). Behind them there seems to be a third series of ridges, and then begins the plateau (also intersected by several snow-clad ridges), which is not less than 3000 feet higher than the Tsaidam plateau, and thus reaches about 12,000 feet in its lowest depressions. Much remains, however, to be explored before we can have a clear idea as to the structure of the mountains which separate Northern Tibet from the Lob-nor depression. Mr. Carey has already cleared up some points, and we may hope that this part of Central Asia—relatively easy to explore—will be well mapped before very long. Let us mention also that on the high plateaus which Prjevalsky visited from Lake Gas he discovered a new species of *Ovis*, which he named *Ovis dalai-lamæ;* it slightly differs by its horns and coloration from all known species of the same genus.

Less than 150 miles separate Lake Gas from the Lob-nor, and so the Expedition went to that lake, which was reached early in February 1885. There the Expedition stayed fifty days; and two chapters of the work which we have before us are given to the description of the Lower Tarim and the Lob-nor (which rapidly dries up and becomes a mere marsh), its flora, fauna, and inhabitants. Many photographs illustrate these chapters, so that now we have an accurate idea of the nature and inhabitants of the great depression which ten years ago was quite unknown. From Lob-nor the Expedition went south-westwards to Khotan, and thus followed the foot of the Altyn-tagh, the Toguz-daban, and the "Russian" ridge, which border the Northern Tibetan terrace on the north-west. The Turkestan oases of Tchertchen, Keria, Niya, and Khotan were thus visited, and again we find much valuable information as to the nature and inhabitants of the country in the chapters dealing with that part of the journey, as also excellent photographs of the inhabitants. Especially interesting are the photographs of the moving sands with their "ripple-marks" made by the wind, of the gigantic holy willows around a spring in the oasis of Niya, and of mulberry-trees in Yasulgun. It would be impossible to give in a few lines an adequate impression of the information gathered by Prjevalsky during his journey on a relatively beaten tract in Eastern Turkestan. To take one instance, we have hitherto had a general idea of the oasis of Keria and the high border-ridge which separates it in the south from the high plateaus. Although Prjevalsky made only a short excursion in these highlands, he supplies us with a most vivid description of the ridge (nearly 20,000 feet high) and its outer spurs, its flora and fauna, its inhabitants—the mountaineer Matchins who dig their dwellings in the loess which fringes the highlands (a group of such dwellings is represented by a photograph)—and their manners and customs. One feature of the highlands of Keria is especially worthy of note—the great amount of rain which falls on them in June and July. During the twenty-five days (July 10 to August 5) which Prjevalsky spent on the outspurs of the Keria Mountains, at a height of from 12,000 to 13,000 feet, it rained and snowed nearly all the time; the clouds coming from Tibet being condensed as they reached the high Keria ridge. The water which falls on the loess soil is immediately evaporated

[1] See Mushketoff's "Turkestan"; also the articles "Siberia," "Turkestan," "Transcaspian," "Transcaucasia," and "Ural," in the "Encyclopædia Britannica," and "Asia" in "Chambers' Encyclopædia."

and re-condensed by the cold air of the snow-clad summits. The great amount of rain in the border-ridges is the more striking as in East Turkestan rain is exceedingly scarce, and there is but very little rain at a level below 6000 feet. Prjevalsky concludes that the heavy summer rains which fall in the Keria Mountains every year are due to the south-western winds coming from the Indian Ocean, and he maintains that the effects of the monsoon winds are felt even in the upper parts of the Hoang-ho.

From Khotan the Expedition went northwards, across the desert. It crossed the Tarim at the confluence of the Yarkand-daria and the Aksu-daria, at a height of 3100 feet above the sea. The river has there a width of 185 yards, and an average depth of from 3 to 4 feet at low water. The velocity of the current was 160 feet in a minute. From these facts, as well as from what he saw of the Lower Tarim, Prjevalsky concludes that the river can be navigated by small steamers from Lob-nor up to the confluence of the Yarkand and Aksu Rivers, and that the two latter can also be navigated to some extent.

The last chapter of the work is devoted to a general sketch of the political condition of Central Asia; but those who are acquainted with the recent literature of the subject will find little new in it. Geographers who may propose to explore Central Asia will find more to interest them in the introductory chapter devoted to the ways and means of travelling in Central Asia, with minute instructions as to how to organize expeditions. This chapter is full of practical hints. As to the scientific problems, Prjevalsky remarks that the present period of great discoveries in Central Asia is rapidly coming to an end. Few parts of what was formerly the great *terra incognita* remain to be explored; so that we approach a time when regular scientific expeditions will be necessary. There remain for investigation by means of "scientific reconnoitrings" the plateaus of Northern Tibet, the highlands of Eastern Tibet and Amdo, and Southern Tibet from Lhassa to Gartok. Three expeditions, lasting two or three years each, would suffice for the exploration of these three regions. The Pamir, with the Hindu-Kush and the Karakorum Mountains, the Eastern Tian-shan, the Altyn-tagh and the Nian-shan, the border-ridges of South-East Mongolia, the whole of the great Khingan, and North-Western Mongolia, close to the Siberian frontier, come next. Dealing with the subject of special scientific explorations, he recommends the Karashar and Si-nin for zoologists; the Tchertchen oasis for archæologists; Si-nin for ethnographers; Kashgar and Khotan, and Si-nin and Urga, for meteorologists; and the whole of Central Asia for geologists.

The present volume has three maps. One of them, on the scale of 67 miles to the inch, embodies the results of the four journeys of Prjevalsky, and is an excellent map of the eastern parts of Central Asia. Two maps, on the scale of 33 miles to the inch, embody the original surveys of the Expedition at the sources of the Yellow River and in East Turkestan. Numbers of excellent photo-lithographs, and three drawings representing new species of *Antelope* and *Ovis*, are inserted in the work.

Many years will elapse before the complete descriptions of Prjevalsky's rich collections will be published. All that can now be said is that they are in excellent hands. K. I. Maximowicz, A. A. Strauch, S. M. Hertzenstein E. A. Büchner, A. S. Woeikoff, and A. A. Inostrantseff are busily occupied with them, and the first parts of their common work are already in print. The first instalment of the first volume, containing the "Mammals," by E. Büchner, was issued at St. Petersburg a few days ago.

P. K.

FLOWERING PLANTS OF WILTS.

Flowering Plants of Wilts; with Sketches of the Physical Geography and Climate of the County. By the Rev. T. A. Preston, M.A. 500 pp. With a Map. (Published by the Wiltshire Archæological and Natural History Society, 1888.).

IN Wiltshire, the chalk downs which form so characteristic a feature of the geology and physical geography of the South of England reach their western limits. The area of the county is about 1300 square miles. It does not anywhere reach the coast, and forms a watershed from which small streams run in three directions, to the Severn, Thames, and English Channel. An elevated plateau of chalk on the south-east occupies more than half the area of the county. This is divided into two unequal halves by a natural depression, called the Vale of Pewsey, which runs almost due east and west, from Devizes to Hungerford. Along this hollow runs the Kennet and Avon Canal, of which the highest point, near Savernake, reaches an altitude of 500 feet. The northern portion of this chalk plateau is called Marlborough Downs, and the town of Marlborough stands nearly in the centre of it. The southern part is Salisbury Plain, the word plain as here applied conveying a delusive notion. Salisbury is at the south, and Stonehenge near the centre of this southern chalk tract. The highest points of these chalk downs reach a height of from 800 to 950 feet. The third section of the county, lying west and north of the chalk, is the fertile plain, underlain by greensand and oolite, along which the Great Western Railway runs between Swindon and Bath. The following extract from the introductory essay contributed to this flora by the Rev. J. Sowerby gives, in a few words, an excellent idea of the general characteristics of these different districts:—

"The great characteristic of the chalk plateau is its vast extent of grass-land, where sheep are extensively pastured. This space is more broken up each year for cultivation, but often exhibits great tracts of grass, with only occasional patches of furze. Only on the small patches of Tertiary scattered here and there, especially in the northern part, do we find wood (generally only underwood), excepting the grand forest of Savernake, twenty square miles in area, which can show timber of an age and size unrivalled in any part of England. The upper chalk hills were once, it is probable, covered with extensive copses, chiefly thorns. Remains of these still occur here and there; and individual trees of great size, some yet extant, others only traditional or historical, attest the former existence of a primæval wood. In the hollows of the downs, especially near the villages, there are spaces, often finely timbered, especially with elms. Monotonous as the surface of the downs may seem to be, the changes that present themselves are often singularly picturesque and varied. After passing, it may be for hours, over the gently sloping grass plains, all blue above, all green below, the traveller suddenly sees below him a

village embosomed in woods, with its picturesque church tower, surrounded by fertile and well-tilled land. Such an experience is obtained by the traveller who passes over the downs from Marlborough to Avebury or Wooton Basset, from Heytesbury to Chittern or Imber, from Warminster over Battlesbury, and the projecting spur of Bratton Castle to Steeple and Prood Ashton, on their picturesque hills.

"The part of the county lying north and west of the chalk plateau is of a different character. It is the plain country, as the other is the hill country of Wiltshire. They have been called 'the cheese' and 'the chalk.' This part includes, in the north, the country drained by the Thames and its affluent the Cole, and on the west that drained by the Bristol Avon. It consists of various geological strata, but chiefly of Oxford clay, a band of which, with an average breadth of nearly five miles, traverses this part of the county from north-east to south-west. Viewed from the outer edge of the chalk escarpment, this region presents the appearance of a vast, well-wooded, and fertile plain, bounded in the far distance by the hill-ranges of the adjoining counties of Gloucester and Somerset. The outer slope of the chalk plateau descends a hundred feet or so, its steep sides covered with turf or clothed with hanging wood, and then slopes gently down to the level. Here is a land of pasture-fields and hedges, overshadowed everywhere by elms, growing mostly in the stiff clay soil that overlies the Kimmeridge clay and Coral Rag. Beyond the level, in front, rises a line of hills of the Coral Rag formation. These hills again descend, with an abrupt slope, into the valley (or plain) through which the Avon flows. The most picturesque scenery is found on the outer slopes of the hills. The plain, though often finely wooded, is somewhat tame, though its gentle hills and dales are not wanting in a beauty of their own."

What is said in the introductory chapter about the climate of the county is not satisfactory. Mr. Sowerby sums it up as follows:—

"On the whole, Wiltshire has probably the most elevated surface of any English county. This gives it certain peculiarities of climate. Its average mean summer temperature is higher, its mean winter temperature lower, than those of any other English county."

In the Report issued by the Royal Horticultural Society, edited by the Rev. George Henslow, on the effect on garden plants of the severe winters of 1879-80 and 1880-81, full details are given for the county of the plants that were injured and uninjured. Taking what is there stated in connection with the full details about the native vegetation of the county which are given in the body of the present work, it is quite clear that the whole of the county belongs to the warmest of Watson's six climatic zones, the Inferagrarian, of which *Clematis Vitalba*, *Viburnum Lantana*, and *Ruscus aculeatus*, are three out of many characteristic plants. The Inferagrarian and Midagrarian zones are represented in England at sea-level; and two others, the Superagrarian and Inferarctic, in the hill-country of the North of England; the two coldest of the six, the Midarctic and Superarctic, being reached only amongst the higher mountains of the Scotch Highlands. So far as one can judge from the botanical point of view, Kent, Sussex, Surrey, Hampshire, and Wilts stand substantially on the same level as regards climate, and cover the whole extent of Watson's Inferagrarian zone, without reaching into the Midagrarian. Mr. Preston is an experienced climatologist and phenologist, and no doubt is quite aware that such is the case; but nowhere in the present work can we find a clear explanation of the true state of the case, which from the point of view of botanical geography is all-important.

Besides the introductory sketch of the physical geography of the county by Mr. Sowerby, and the chapter on its temperature and rainfall, there is one on its geology, with a table of the twenty strata represented in the county twelve of which belong to the Oolitic series, four to the Cretaceous, and four to the Tertiary. Mr. G. S. Boulger contributes a sketch of its drainage, on which Mr. Preston founds eleven districts, under which he classifies the localities of the rarer plants. Two of these drain into the Severn, two into the Thames, and the other seven into the English Channel. Out of the 500 pages, 430 are occupied with a catalogue of the flowering plants of the county, with such detailed localities for the rarities as have been noted. Great pains seems to have been taken to identify the species, and to trace out their dispersion through the different districts. The eighth edition of the "London Catalogue" is followed as a standard of nomenclature; and out of the 1760 species there registered for the whole of Britain, 849 have been found in Wiltshire. Of these about 50 are marked as introduced, and at least 50 more are reckoned as varieties in Watson's "Cybele," where the number of wild British plants, including ferns, is estimated at 1425. Of these 1425 species, 530 are spread over the whole of Britain, 600 scarcely reach into Scotland, 200 are characteristically boreal types, and 77 too local to be classified. Wiltshire yields two plants which are not known elsewhere in Britain —*Cnicus tuberosus* and *Carex tomentosa*—both widely spread on the Continent. Out of 600 austral types, 127 are characteristically eastern in England. These are mostly plants that prefer chalk and limestone, and are well represented in the county. Add 30 for ferns, and 780 species for Wiltshire will be about the number on Watson's scale of reckoning, and compares properly with the figures as given in tables on pp. 371-81, and elsewhere in the fourth volume of the "Cybele." This is a smaller number than the plants of Kent, Surrey, and Hants, and about on a par with Dorsetshire and Hertfordshire. In all these counties the boreal element of the British flora is substantially eliminated. The special deficiency or unusual rarity in Wilts is of the plants of sandy heatherland, such as the foxglove, *Ulex europæus*, the fruticose Rubi, and many of the Trifolia, and such grasses as *Aira præcox*, *Deschampsia flexuosa*, and *Nardus stricta*. Altogether the book is one which no one who is interested in the distribution of British plants can afford to neglect.

J. G. BAKER.

MR. DODGSON ON PARALLELS.

Curiosa Mathematica. Part I. A New Theory of Parallels. By Charles L. Dodgson, M.A. (London: Macmillan and Co., 1888.)

THIS small book came into the world a little too soon or a little too late for our comfort. It was offered to us for notice in mid-vacation, and thinking we should find something amusing, we incontinently accepted the offer. We found some amusement, for, contrary to the author's experience, we read the preface, which is rather drawn out, but here and there is brightened by a quaint

fancy or an odd quotation. We read also the appendixes, which are of a similar character with the preface; but then there remained the inner layer of the sandwich, which called for a most careful overhauling. Anyone who has read Mr. Dodgson's "Euclid and his Modern Rivals" (cited in our present notice as "E. and R."), is prepared to find close logical reasoning and acute remarks in any work he undertakes, and the reader of this booklet will not be disappointed in these particulars.

Euclid's Twelfth Axiom the author asserts to be *not* axiomatic, *i.e.* he has not met with any "bimanous biped" who accepts it as *intuitive* truth. His quest is to find a better axiom. At first sight—and the illustrative figure meets us on the cover and in other places—one is disposed to grant the truth of the Dodgsonian axiom, viz. that "in any circle, the inscribed regular hexagon is greater than any one of the segments that lie outside it." But the author is not restricted to a paltry *once;* he can equally well grapple with the problem, if his reader will grant that twice, four times, eight times, or, in mathematical parlance, 2^n times, the hexagon is greater than any one of the above-named segments. The principal part of the text consists of five definitions, six axioms, and seventeen propositions. Appendix I. contains alternative proofs of certain propositions consequent upon the general form of the author's axiom. The "distance" between two points figures as a definition ("the length of the shortest path between them") and as an axiom ("the length of the straight line joining them"). Prop. IV. is headed a "Theorem"; we should have thought it was a "Problem." It is:—"Given a triangle: to describe an equilateral triangle which shall "enclose it" (third line from end—for AE read DE). Prop. V. is an important one; it runs thus:—"Given a certain angle; and given that every isosceles triangle, whose vertical angle is not greater than the given angle, has its base not greater than either of its sides; to describe, on a given base, an isosceles triangle having each base-angle equal to the given angle" (line 5 from end, for "DE produced" read "BE produced"). In the corollary to this, the argument seems to us to be scamped: a hasty reader might think that Mr. Dodgson had *assumed* Euc. I. 32. We would close thus:—"Angle AFB is greater than angle ACB, ∴ greater than angle ABC, and *a fortiori* greater than angle ABF." Prop. VI. is all right, but how are the figures to be constructed if $n > 2$? The sum of the angles of a triangle is called, by Mr. Dodgson, its "amount." In Prop. VII. (interchange E and F in dexter figure) he shows that, "if α, β be two 'possible amounts'—that is, 'amounts' belonging to existing triangles—then every 'amount' intermediate to α and β is also 'possible.'" Prop. VIII. shows that there is a triangle whose angles are together not greater than two right angles (line 7 up, p. 17, for "=" read ">"; line 1 up, p. 18, for "ABD" read "ADB"). At this point comes the axiom, and Prop. IX. follows:—"An isosceles triangle, whose vertical angle is one-twelfth of a right angle, has its base less than either of its sides" (the corollary applies Prop. V. above). Prop. X. shows that "the angles of every equilateral triangle are together not less than one-fourth of a right angle." Prop. XI. establishes that "there is a triangle whose angles are together not less than two right angles." This is a long proposition. Props. VIII. and XI. are sound on the hypothesis which Mr. Dodgson seems tacitly to have adopted, viz. that the "amounts" of triangles are all in the same boat, either *all* greater than two right angles or *all* less than the same quantity. Surely it is, *a priori*, conceivable that the "amounts" may be variable, and then how will his proofs hold? Prop. XII. readily deduces, from the *previous reasoning*, that "there is a triangle whose angles are together equal to two right angles." Prop. XIII. proves that there is a quadrilateral figure with all its angles right angles (*i.e.* a rectangle); and Prop. XIV. shows that the opposite sides of such a figure are equal. But, "tell it not in Gath," Mr. Dodgson *takes up the rectangle*—we repeat it, takes up the rectangle—and *reverses* it, "so that A, B, may change places." We do not object, but how about the Irish bull (E. and R., p. 47)? But this is not the only surprise in store, for in Prop. XV., to prove that "there is a pair of lines, each of which is 'equidistant' from the other—that is, is such that all points on it are equally distant from the other line"—he makes a rectangle slide along on a straight line! Prop. XVI. proves that "the angles of every triangle are together equal to two right angles"; and Prop. XVII. winds up the story with showing "that a pair of lines, which are equally inclined to a certain transversal, are so to any transversal."

The appendixes repay perusal. Appendix II. discusses Euclid's axiom, and argues that, "though true in the sense in which *Euclid meant it*, it is *not* true in the sense in which *we take it.*" In fact, Mr. Dodgson contends that Euclid "excludes from his view both infinities and infinitesimals, and considers *finite magnitudes* only." This is rightly founded on Euclid's Book X. Prop. I. We cannot go into the matter further here, but commend this and Appendix III. ("How should Parallels be Defined?") and Appendix IV. ("How the Question stands To-day") to any reader who is interested in this crucial question. We can quite sympathize with the author, as in times past we have more than once done our little best in the same direction, when he recounts how, more than once, he, too, has "with clasped hands gazed after the retreating meteor, and murmured, 'Beautiful star, that art so near and yet so far.'" To conclude, Mr. Dodgson is "inclined to believe that, if ever Euc. I. 32 is proved without a new axiom, it will be by some new and ampler definition of *the right line*—some definition which shall connote that peculiar and mysterious property, which it must somehow possess, which causes Euc. I. 32 to be true. Try *that* track, my gentle reader; it is not much trodden as yet; and may success attend your search!"

OUR BOOK SHELF.

Primer of Micro-Petrology. By W. Mawer, F.G.S. (London: Office of *Life-Lore*, 1888)

THE task of introducing the student to any particular branch of science requires such selective judgment, such tact both in saying and in leaving things unsaid, that it is not surprising if many so-called "primers" fall short of the good intentions of their authors. Mr. Mawer, in this little book, presupposes "an acquaintance with the phenomena of pleochroism and the polarization of light," and hence refers only in the briefest manner to the methods employed in the examination of thin mineral

sections. He also, in his desire to be abreast of current literature, uses such terms as "allotriomorphic," "microfelsitic," "magma-basalt," without adequate definition or discussion; and in speaking of a "porphyritic groundmass" he will throw many beginners into confusion. If the student is to seek elsewhere for instruction both in the manipulation of the polariscope and in the use of technical terms, the book must be held to fail in its fundamental object as a primer. It will probably serve well, however, to remind the learner of the broader features that mark out one rock-forming mineral from another. The author, moreover, insists, as befits a geologist, on the purely supplementary character of microscopic study—a warning that seems more than ever needed when micro-petro-*graphy*, by the change of a few letters, has been exalted to the level of a science.

The statements in this book are essentially accurate, and the illustrations, excepting that of ophitic structure, may be useful in recalling diagrammatically what has been seen in actual sections. On p. 32, however, there is an incorrect account of the pleochroism of muscovite, which probably has arisen from a blending of two totally independent notes. On p. 36 the sections of augite should be described as having, not six, but eight sides; and talc reappears on p. 50 as a constituent of protogine granite. The cleavages in drawings on pp. 33 and 36 are not in every case consistent with the descriptions.

G. C.

Theoretical Mechanics. By J. E. Taylor, M.A. (London: Longmans, Green, and Co., 1888.)

So long as examinations on prescribed courses are in vogue, so long, we suppose, will text-books be written for them. The book before us has been prepared chiefly to help those who are studying for the elementary stage of the Science and Art Department's examination in the subject, but it also covers the requirements for London matriculation. There is not much scope for originality in a work of this description, and in looking through it we find ourselves in familiar, well-worn paths.

In his preface, Mr. Taylor states that he has endeavoured to make the subject comprehensible to the beginner, but we are afraid that his efforts to explain the difference between mass and weight will be far from successful. This is always a delicate point to touch upon, but we venture to say that few beginners will be likely to understand the explanation given on page 8. This is as follows:—"Whilst mass is always measured by weight, yet the two terms must be kept distinct, the weight being the amount of force which the attraction of the earth exerts on the mass. If g represent this attraction, W, weight of the body, m, mass, we have $W = mg$." Most beginners are likely to imagine from this that W should be equal to g, instead of to mg.

The book is well illustrated throughout with many new diagrams and several old ones from well-known text-books. Numerous examples, worked and unworked, are also given.

With the exception referred to, the book is on the whole well written, and completely covers the Syllabus. The admirable style in which it has been issued, and its comparative cheapness, will commend it to many teachers.

Instructions for Observing Clouds on Land and Sea. By the Hon. R. Abercromby. With Photographs and Engravings. 22 pp. (London: Stanford, 1888.)

The phrase *Nascitur non fit* may be applied to cloud observers with almost the same confidence as to poets; at least, such is the experience of most persons who have attempted to teach an ordinary observer to record cloud phenomena.

Mr. Abercromby's pamphlet, however, contains a valuable stock of instruction which may be placed in the hands of intending observers, and will at least indicate to them what they have to observe.

The actual nomenclature of cloud forms used by Mr. Abercromby is that which has been, for the time at least, rejected by the International Committee at the recent meeting at Zürich (Nature, vol. xxxviii. p. 491), but this is a minor matter. The illustrations of cloud perspective and cloud motion are new and good, while the difference between the motion of advance, the "propagation" of a cloud bank, and the rotation of the clouds within that bank is, for the first time, clearly stated. The importance of the R. point (radiation point), the point towards which the stripes of cirrus converge, is explained.

Mr. Abercromby concludes as follows:—

"It (cloud observing) cannot be learnt in a day, but with a little attention and practice the knowledge is soon acquired. The observer, who begins by taking simple cases of low, fast-moving clouds, till he has fully realized the meaning and importance of R. points, will soon attain such proficiency as will enable him to make valuable observations in the most recent branches of modern cloud science."

Laboratory Manual of General Chemistry. By R. P. Williams, A.M. (Boston: Ginn and Company, 1888.)

After a few preliminary matters, including some good rules for students in the laboratory, each two pages of this book has in large type consecutive directions for performing an experiment or exercise. The rest of the two pages is left blank for written notes. One hundred exercises are given, and they are of a quite elementary character. It is a pity that contractions are so frequently used, especially when there is a large amount of vacant space and so small an amount of matter. "Ap.: p.t., 4 rec., t.t., d.t., r.s." indicates to the student the apparatus he needs for the purpose of preparing hydrogen. It would have been better to adopt a recognized system of shorthand throughout, for that would have rendered the book more useful to some and quite useless to others, instead of troublesome to all.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Mr. Romanes on the Origin of Species.

What the *Times* said is substantially the same as what Mr. Romanes himself says on p. 366 of his paper: "The theory of natural selection is not, properly speaking, a theory of the origin of species: it is a theory of the development of adaptive structures. Only if species always differed from one another in respect of adaptive structures, would natural selection be a theory of the origin of species. But, as we have already seen, species do not always, or even generally, thus differ from one another." Very well then, I say, if this be true, it shrivels up the part played by natural selection to very small dimensions.

The second part of Mr. Romanes's reply consists of a complaint that when I quoted from his paper the words "natural selection not a theory of the origin of species," I did not see that they were "flatly falsified" by the section at the head of which they stood. I do not see it even now, because the section finishes with what Mr. Romanes oddly calls an "insinuation" "that Mr. Darwin's great work on the 'Origin of Species' has been misnamed." If this insinuation be just, then I further do not understand how Mr. Darwin's reputation for candour is to be saved except at the expense of his intelligence.

In the third part of his reply, Mr. Romanes says "he [Mr. Dyer] must surely be aware that other botanists who have more thoroughly considered the subject are dead against him in his general conclusion." I have perhaps as many opportunities as most men of knowing the opinions of botanists, and I cannot

say I am aware of anything of the kind. The "late Prof. Nägeli" may be dead against me possibly; but I was not aware that he was dead in any other sense. Nor do I see that as he insists (quite correctly as I think) on the inutility of family characters he can afford much comfort to Mr. Romanes, who regards them as generally adaptive.

I have devoted a good deal of time to the study of Mr. Romanes's paper published by the Linnean Society. I believe I have stated the conclusions to be drawn from that paper with tolerable accuracy. If I have not done so, I undoubtedly owe him a sincere apology. But I am bound to confess that, the more I study his views, the more I find myself in disagreement with him as to the inutility of specific characters; as to the utility and mode of origin of generic characters and those of higher grade; as to sterility as a primary specific difference; and as to the value of so-called physiological selection. In all these matters he is, I am satisfied, contradicted by botanical experience. I think if he had imitated the example of Mr. Darwin, and had carefully collected a large body of evidence on each of these points with a perfectly open mind, he would have found this out for himself. What, however, I view with less patience than his unsustained generalizations, is his persistent attempt to place them on the shoulders of the Darwinian theory. I have reluctantly arrived at the conviction that his only excuse for so doing is that he has fundamentally misunderstood that theory. At any rate, I cannot in any other way account for the strained interpretation which he has put on passages from Mr. Darwin's writings. I may give, as an example, the passage he quotes "to justify the insinuation" that the "Origin of Species" has been misnamed; the obvious drift of this does not relate to specific differences at all, but to those which are characteristic of families. It is easy to see, in fact, by a comparison of pp. 170 and 176 of the sixth edition, that the passage cited by Mr. Romanes was inserted by Mr. Darwin to meet the point raised by Nägeli to which I have referred above. Certainly I think that no one would have been more surprised than Mr. Darwin when he wrote the words could he have foreseen that they would be used to impugn the validity of the title of his theory and of his book. Everyone knows that Mr. Darwin was the fairest and most generous-minded of men. He constantly admits the possibility of explanations to which he really, however, did not attach much importance. Such admissions Mr. Romanes appears to me to treat as if wrung from a hostile witness. In my judgment this is entirely to misapprehend their significance or the spirit in which they were made. W. T. THISELTON-DYER.

Royal Gardens, Kew, December 1.

Natural Selection and Useless Structures.

IN his letter on "Mr. Romanes's Paradox" (NATURE, November 1, p. 7), Mr. Thiselton Dyer questions the existence of indifferent or slightly disadvantageous specific characters. That letter referred, in a highly laudatory yet somewhat deprecating manner, to a lately published (Proc. Roy. Soc., No. 269) obituary notice of Mr. Darwin; and it implied that Mr. G. J. Romanes, from his unfamiliarity with the study of species, did not quite know what he was talking about when he asserted that such indifferent characters do in fact exist. I, who claim to have had some slight experience in the practical discrimination of species, ask permission to make a few observations in your columns on the subject.

Everyone would, I suppose, regard the frequent absence of the toe-nail on the hallux of the orang as an indifferent matter, but I am inclined to consider the feeble development of that digit itself as a slightly disadvantageous one. However that may be, I am strongly of opinion that the abortion of the index in the Potto can never have saved the lives of the earliest individuals so distinguished. I have, as yet, heard no reason assigned for the life-saving action of the thumbless hands of *Colobus* and *Ateles*, or of the tail of the one chameleon in which alone (so far as I know) that organ is not prehensile. The metallic lustre of the peritoneum of some fishes is hard to explain by either "natural" or "sexual" selection; as also are such specific characters as the extension, or non-extension, of the premaxillæ to the frontals, or the pattern of the foldings of enamel and cement in various Rodents. The complexity of the teeth of *Labyrinthodon*, or the similar multiplicity co-existing in those of *Orycteropus* and *Myliobatis* (which can hardly have been derived from a common ancestor, though their resemblance extends even to microscopic structure), are unquestionably good taxidermic characters; yet they can hardly have been due to the action of natural selection, as I pointed out in my "Genesis of Species" in 1870. But if such "selection" cannot originate characters which form the diagnosis of a species, then it cannot possibly be the origin of such species. To say that the rudimentary index of the Potto is a character which, though itself useless, has been carried on the back, as it were, of some possible but unknown useful simultaneous variation which co-exists with it or did co-exist with it in some unknown ancestor is a purely gratuitous assertion. Such assertions are the less warranted because we have evidence that the energy of Nature's destructive forces has been exaggerated. Prof. Dyer tells us that natural selection is a hard taskmaster; but it is not, I think, so hard a one as some persons suppose. This seems to me clear from such facts as the finding of hares and rabbits in which an incisor tooth has grown so as to complete the circle it always tends to form—a condition which shows a remarkable preservation of life under extremely disadvantageous circumstances. A stoat, three of whose feet had been cut off at different times by traps, has nevertheless (I am informed) lived long enough for its injured limbs to heal so thoroughly that the beast could get a living on its one foot and three stumps. Cases of prolonged life under trying circumstances are not so rare. I recollect the skeleton of a monkey which must have long suffered from acute rheumatism in its native forests.

Prof. Dyer deprecates the admission, by the author of the obituary notice, that indifferent or slightly disadvantageous characters may be evolved in spite of "natural selection." But the obituary notice admits *much more than that*, since, according to its author, a maintainer of "natural selection" is free to affirm the genesis of species by sudden, considerable, definite variations, directly produced by the reaction of the innermost nature of an organism on the stimulus of its environment, according to precise innate laws of its being. This certainly is not "natural selection," as understood and taught by Mr. Darwin, and the inventor of a new term has alone the right to fix what its meaning shall be.

The statement of the obituary notice seems equivalent to an unintentional but virtual abandonment of "natural selection," while still retaining the name—reducing it, in effect, to that merely subordinate *rôle* we all admit that it plays. To call such a mode of origin "origin by natural selection" seems much the same thing as declaring an elaborately prepared theatrical transformation scene to be brought about by the chains and cords which prevent its moving pieces from passing beyond their assigned limits. The true meaning of "natural selection" is frankly declared by that distinguished biologist upon whose shoulders the mantle of the deceased prophet seems to have fallen. Prof. Lankester, in his article "Zoology" (in the last volume of the "Encyclopædia Britannica") has just given a most straightforward, lucid, and forcible representation of Darwinism. Nevertheless, the article (in the same volume) on "Variation" by Prof. Geddes, appears to me to be more in harmony with the facts of biology. It is, of course, open to anyone to say: "All species which succeed do so from some cause, and this may be metaphorically said to 'select' them." Therefore, since all causes are "natural" causes, every species which does succeed must succeed through "natural selection." This is equivalent to saying: "Nature is so conditioned as to produce the results it does produce"—an assertion most true, but somewhat trivial. When a term is so stretched as to mean "anything," it thereby comes to mean "nothing," and its use can serve no purpose save the preservation of a phrase it may be desired, for some reason, not to discard. ST. GEORGE MIVART.

Hurstcote, Chilworth, Surrey, November 28.

A Mussel living in the Branchiæ of a Crab.

LATE this autumn, while searching for Crustacea at Amroth, in South Wales, I found rather an exceptionally good specimen of the common shore crab (*Carcinus mœnas*), which I took back to the hotel to clean and preserve. On removing the carapace, I found a mussel living among the branchiæ, and fastened to them by means of its byssus. It was in good condition, and measured ⅜ of an inch in length. The carapace of the crab measured 2¼ inches wide by 1⅝ inches long. I could find no signs on the exterior of the crab of anything remarkable within, nor was there any damage to the shell, or hole through which the mussel could have passed. It seems that the mussel, while yet minute, or in a larval condition, must have been carried

into the branchiæ, along the ordinary passages, by the flow of water the crab urges through them; it must there have become entangled in the feathery branches, and lived in this unwonted habitat long enough to have grown to its present size, having its food carried to it by the same water that served to oxygenate the lungs of its host. W. R. PIDGEON.

42 Porchester Square, W.

The Pasteur Institute.

IN the article in NATURE under the above title the writer says (p. 74):—"The probability of rabies following the bite of a rabid dog is now definitely ascertained to be from 15 to 16 per cent. of those attacked." It would greatly assist all who desire to form an impartial estimate of the value of Pasteur's researches on rabies as far as they are deducible from a comparison of statistics, if the writer would state the facts and figures on which the above computation of 15 to 16 per cent. rests. The statement is repeatedly made, but the proof is never given along with it. It is obvious that, unless this percentage is proved beyond dispute, the statistical argument will be lacking in cogency and force, and leaves a loophole for attack by those who are ever ready to depreciate and oppose the brilliant investigations of M. Pasteur. ERNEST ALBERT PARKYER.

Blackburn, December 3.

The Zodiacal Light.

IN your issue of October 25 (vol. xxxviii. p. 618), Dr. Muirhead quotes a remark of Cassini's in contradistinction to the relation indicated in your issue of the previous week (October 18, p. 594). The remark has not escaped notice, but is, I think, directed to a variation of shorter period, abundantly exemplified in Weber's observations, and in no wise invalidating the relation in the note of October 18. As far as Cassini's numerical observations go, the relation of the 18th is fairly exemplified, as will be shown by the following figures:—

Year.	Number of Observations.	Mean Elongation, referring to March.
1683	4	51·3
1684	2	67·10
1685 (max.)	33	52·35
1686	26	56·28
1687	16	68·82
1688	2	51·53

There are also observations indicating that the appearance did not pass away in 1688. Missionaries report brilliant appearances in 1690.

Any statement beyond the existence of this shorter variation would be at present premature.

The number of observations do not permit of a sufficiently sharp determination of the critical epoch to assert the amount of lag. The best determination which I am at present able to make is as follows. The sun-spot maxima of 1848, 1860, 1871, and 1883, follow the minima of the zodiacal light by + 1, − 1·5, − 1·5, + 1·5 years respectfully. The sun-spot minima of 1856, 1867, and 1878, follow the maxima of the zodiacal light by + 1, − 1, + 2·5 years respectively.

As to the working hypothesis, the suggestions put forward by Huggins in the Bakerian Lecture for 1885 seem in slightly varied form to meet all the facts which I am at present able to bring to bear upon the subject. There is evidence in the variation in the light of Encke's comet, as well as in the disturbance of its motion, that approaching the time of sun-spot maximum it meets matter moving towards the sun which it does not meet at the time of sun-spot minimum. Whence this matter comes may perhaps be questioned.

Observations seem to have been very nearly dropped since Weber's death in 1883. I am sure Dr. Muirhead will join with me in calling the attention of observers to this subject, and in asking that those observatories favourably situated would give us continuous records both as regards place, spectrum, and polarization. O. T. SHERMAN.

Baltimore, Md., November 15.

The "Tamarao" of the Philippine Islands.

DANS le numéro d'"August 16" (vol. xxxviii. p. 363), vous donnez une lettre du Dr. P. L. Sclater au sujet du *Tamarao* de Mindoro. Je crois que le Musée de Dresde s'en est déjà occupé; mais, sans avoir eu connaissance de ce travail, j'ai publié une note dans le tome ii. de nos Mémoires (Trübner, London) concernant l'histoire naturelle de l'Empire Chinois (p. 90), sur le *Tamarao*. J'y constate que c'est un buffle, et je propose de le nommer *Bubalus mindorensis*. Il n'a rien de commun avec l'*Anoa* des Célébes, au moins en ce qui concerne les dents.

Je suis curieux de voir dans le prochain numéro des P.Z S. une opinion contraire à celle de 1878. En dix ans on fait du chemin.

Je vous serais reconnaissant, Monsieur le Directeur, d'insérer ce petit mot dans votre correspondance. P.-M. HEUDE, S.J.

Musée de Zikawei, près Shanghai, 15 Octobre.

THE EARLIEST RACIAL PORTRAITS.

THE earliest representations of races that are preserved to us have been strangely neglected hitherto. On the Egyptian monuments are carefully sculptured and coloured figures of the various races that fell from time to time within the reach of conquest, or that entered into relations with Egypt, dating from the third millenium B.C.; yet till last year no attempt had been made to secure copies of these, free from the inevitable errors of mere drawings. At the desire of the British Association I took up this work, and made a series of casts of 280 heads from the sculptures, besides noting the colours of all paintings of races that I could find. These casts I then photographed, and the prints of the photographs can be obtained at cost price of printing.[1] These photographs are the source of the blocks (prepared by Messrs. Harper and Brothers) used in this paper, which, therefore, are perfectly automatic copies of the original sculptures.

In a recent article (NATURE, August 2, p. 321) Prof. Sayce has already noticed some of the conclusions to be drawn regarding a fair race in Palestine, so that it is needless here to repeat his statements; the actual portraits will, however, enforce his conclusions. The Amorites, who occupied the whole of Palestine, are seen (Fig. 1) to have fine though powerful features, quite different from the Jewish-Assyrian or the Egyptian types, with dolichocephalic heads; a type of face quite in accord with the light complexion and red-brown hair which they appear with in a painting of about 1500 B.C. They differ thoroughly from the features of the surrounding races of Hittites, Philistines, and Bedawin, as sculptured by the same artists, so that we are clear of the influence of mere conventionality. The Thahennu of Northern Africa, the Kabyles of modern times, show (Fig. 2) closely the same features, with only a slightly different beard and the long lock of side hair characteristic of the peoples of that region. Of the very few other portraits of Aryans that appear in Egypt, one of the most interesting (Fig. 3) is the primitive Greek woman, one of the captive Hanebu, or "lords of the north" (1400 B.C.). This has a very expressive and intelligent face, and the wavy sidelock and back hair recall the archaic Greek sculptures and vase-paintings. The stone has been unfortunately injured, but this precious proto-Greek is the only one remaining of the group.

In considering the origin of the Egyptians themselves, we are met with the difficulty that they are unlike any of the well-known neighbouring races. On the monuments we find, however, the Punites, or people of the southern shores of the Red Sea; and the resemblance between their features and those of the Egyptians is strikingly close. This noble of Pun (Fig. 4), has so precisely the face of Seti II, that either might be intended for the other. The evidence of relationship is not only in feature; the Egyptians coloured themselves as the red race, in contrast to the yellow Libyan, the brown Asiatic, and the black Negro in the four great divisions of mankind: they also colour the

[1] Apply to Mr. Harman. 75 High Street, Bromley, Kent.

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FIG. 1.—Amorites.

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FIG. 2.—Thahennu.

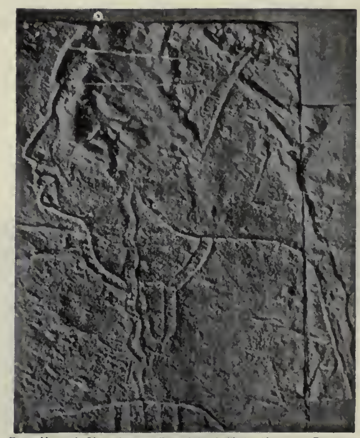

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FIG. 3—Hanebu.

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FIG. 4.—Pun.

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FIG. 5.—Hyksos.

Punites of the same red. In their civil relations likewise they show some signs of a community of origin: the Egyptians never made war on the Punites, while peaceful intercourse is repeatedly found, under Sankhkara (2800 B.C.), Hatasu (1600 B.C.), Ramessu III. (1200 B.C.), and others. The land of Pun was called "the divine land" by the Egyptians, and the typical form of the beard of the Egyptian gods is that of the noble of Pun above. Unfortunately we know so very little of the archæology of Somali Land and Yemen, which appear to be the ancient Pun, that it is hopeless at present to obtain clues from there; but the recent report of Colonel Haig on the extraordinary terracing of the hills on the Arabian side with stone walls to a height of 6000 feet, the great buildings mentioned by Hamdāni (tenth century A.D.), and the massive ruins, with blocks 13 feet long, at 70 miles in the interior from Aden,[1] all show that this region has been at some time a seat of civilization. It is not too much to hope that British or Italian energies there may yield some authentic and accurate accounts of the antiquities around Aden and Asab.

The source of the Hyksos, or "shepherd kings," who

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FIG. 6.—North Syrians.

invaded Egypt about 2300 B.C., has been much discussed, and one special object of my work was to get a good profile of the Hyksos sphinx in the Bulak Museum (Fig. 5).

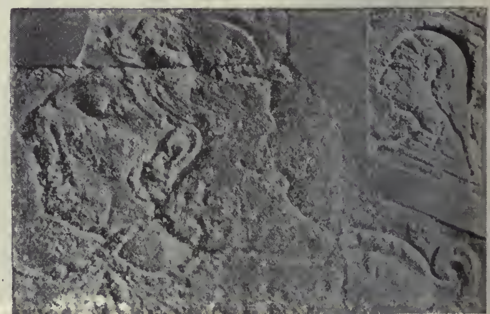
The features are quite peculiar, and unlike those of any Egyptian or other race usually represented. But on the north wall recently uncovered at the temple of Luxor, precisely the same face is found (Fig. 6), both in profile and full face, among people of Northern Syria.

The frontal projection, the horizontal eye, the precise curves of the nose, the peculiar form of slope beneath it, the lips, and the angle of the beard, are identical throughout; while the very thick bushy hair in the Syrian parallels the massive locks with which the Hyksos always represented themselves. Further evidence on the Hyksos has just come to light. On a statue found by M. Naville at Bubastis are the names and titles of a Hyksos king (*Academy*, September 1, 1888, &c.), Khian, which at once links to the Greek form Ianias of Manetho's dynastic lists, and appears to be identical with the name Khaian which occurs twice among the chiefs of North Syria, about 1000 B.C., in the Assyrian annals. Thus there is a confirmation of the view that the Hyksos were a people from Northern Syria, and further research should follow on these lines.

[1] Described to me by Colonel Johnson Pasha.

Of the Khita, or Hittites, there are several portraits, of which the most characteristic (Fig. 7) is that of the king (1200 B.C.). All of these are closely alike, and could not be mistaken for any other race on the monuments. The very low and retreating forehead, the large curved nose, and beardless receding chin are the essential points; and it is just these peculiarities which are most marked in the sculptures of the Hittites executed by themselves in their own cities. The general view now is that they must have been a Mongolian race, who held a military occupation of the lands around their own country, much like the Turkish rule of modern times. Their colour on the paintings is a moderate brown or browny yellow; the eye brown, and the hair black or brown. They are thus completely different from the fair Amorites, the red-brown Bedawin, or the yellow Phœnicians which surrounded them.

The value of the series of photographs lies not only in the study of one or two special races, but in the general information on the characteristics of the people of all the countries around Egypt at about 1400 B.C. What is much needed now is an equally complete and comparable series of portraits, in profile and full face, of the modern races which are supposed to be the representatives of these.



From Harper's Magazine. Copyright, 1888, by Harper & Brothers.

FIG. 7.—Hittites.

Until we have the recent portraits for comparison, the full value of the information which the Egyptians have so carefully recorded cannot be made available. It is to be hoped that some amateur will take scientific photographs in Syria, North Africa, and other countries before long.

An interesting study of the mixture of races may be made from the coloured wax paintings of the Roman age which I discovered this year in the Fayum. From those we see how largely Greek and Italian blood penetrated into Egypt far inland, and how it became mixed with the native race; showing that the Copt, though pure from Arab admixture since the Muhammedan conquest, is far from being of a pure race. We have therefore in the Copts a most interesting example for study; as the effect of climate in unifying a heterogeneous mixture, and subduing elements foreign to the country, can be here observed without any admixture of fresh races for twelve hundred years. A thorough anatomical study of the average Copt in comparison with the elements of ancient Egyptian, Negro, Ethiopian, Arab, Greek, and Roman, would throw light on the great question of climate *versus* race in the causation of characteristics. We have a specimen race duly compounded, and then safely set apart for future examination, owing to the power of fanaticism, which has been an absolute barrier to further combinations.

W. M. FLINDERS PETRIE.

BRITISH TERTIARY VOLCANOES.[1]

DURING the last twenty-seven years, the study of the volcanic rocks of the British Isles has been a constant and favourite pursuit with Dr. A. Geikie. It is now seventeen years since he read before the Geological Society of London the most important of the numerous memoirs which he had from time to time up to that date put forth on this subject. It was the well-known paper on the Island of Eigg, and was intended to be the first of a series of papers descriptive of localities where the characteristic features of the British volcanic rocks are well displayed. But man proposes: for the promised continuation of the series geologists have waited long and anxiously, no further instalments having till now appeared. The delay however, though trying while it lasted, has been productive of good result in the end; for we have now the long-wished-for consummation, not scattered through a long string of isolated papers, but in one connected whole. Dr. Geikie has garnered his harvest, and has summed up the results of the labours of more than a quarter of a century in a memoir which may fairly be looked upon as one of the most important of the contributions to the geological history of Britain which have seen the light since the days of William Smith.

And it gives to this elaborate communication a further importance that it is controversial as well as descriptive. In January 1874, Prof. J. W. Judd read before the Geological Society a paper "On the Ancient Volcanoes of the Highlands." It is a singularly fascinating production; its story is concisely and graphically told, and hangs well together; and I shall not easily forget the interest with which I read it for the first time, and which frequent reference to it subsequently has not abated. But Dr. Geikie's study of the subject has led him to conclusions directly in the teeth of two at least of the most important of those arrived at by Prof. Judd. Both authorities are agreed that the great basaltic plateaus of the Western Islands of Scotland and the North-East of Ireland are formed of sheets of sub-aërial lava piled one above another, the products of a long series of eruptions. Prof. Judd holds that the lavas were poured out from great central volcanoes of the type of Etna and Vesuvius, and he has endeavoured to fix the sites, and form an approximate estimate of the size of these volcanoes. Dr. Geikie is unable to find evidence for the former existence of central volcanic piles, and he believes that the lavas were emitted from fissures and numerous scattered vents of inconsiderable size. Again Prof. Judd thought he had established a threefold order of events during the period of volcanic activity. The first series of eruptions was marked by the discharge of lavas belonging to the acid class. Then came an abatement or cessation of the volcanic energy, and during a quiescent interval the cones and products of this period were largely denuded. Volcanic activity was resumed during the second period, but its products were of the basic class, and now form the basaltic plateaus. During the third period the volcanic energy was dwindling down, and had so far spent itself that it was equal to the production only of sporadic cones of small size, which are paralleled with the "Puys" of Auvergne.

And Prof. Judd further maintained that the great intrusive masses of granite and gabbro, which now form some of the boldest heights of the district, are the hardened contents of the reservoirs which fed the volcanoes of the first two periods. They had originally been buried beneath the cones that were heaped over them by the eruptions, and have been bared by denudation. The granites belong to the volcanoes of the first period, and Prof. Judd maintained that a gradual passage could be traced from them into sub-aërial lavas of acid composition that were emitted during that period. Similarly the gabbros were relegated to the second period, and pass gradually into its basic lavas. Dr. Geikie on the other hand brings an overwhelming mass of evidence to show that the intrusive masses of granite and other acid rocks are younger than the plateau-basalts. He mentions nothing that can possibly correspond with the sub-aërial sheets of acid lava which Prof. Judd states were poured out during his first period; he shows, indeed, that the one solitary known instance of a true superficial stream of acid lava is that of the Scuir of Eigg, which is unquestionably considerably younger than the plateau-basalts.

Though Dr. Geikie does not express a positive opinion on the subject, it seems to me that there is nothing to forbid our looking upon some at least of the smaller vents which he describes as Puys as belonging to Prof. Judd's third period. The vent, for instance, at Faskadale (p. 106) must be later than the great acid protrusions.

It is a serious matter for one whose acquaintance with the field of dispute is but slight to endeavour to hold the balance fairly and evenly between the conflicting views of two such eminent authorities, who have both made a study of the ground itself. Nor is the delicacy of the task diminished by the fact that the disputants have been for many years among the writer's most valued brethren of the hammer, and that to their teaching and example he owes more than can be put into words. This circumstance has fortunately however a certain advantage, for in attempting to decide between the conclusions of two equally valued and equally respected friends he will at least be free from any suspicion of partiality.

Some general considerations may be noticed before coming to detailed criticism. Dr. Geikie has known the ground ever since he was a boy: he has roamed over it again and again; he has had opportunities without number of reviewing, and in some cases of correcting, his first impressions. He has had, to some extent, the assistance and co-operation of his colleagues on the Geological Survey, and has had free access to all the details of their elaborate surveys. I believe I am right in saying that Prof. Judd was able to devote to his examination of the district the summer months of not more than two or three years. Without in the least implying that his observations were hasty, it must be clear that his opportunities for going into detail were very inferior to those of Dr. Geikie.

Further I am bound to confess that, though I was fairly carried away by the charm of Prof. Judd's paper, he did not succeed in bringing conviction to my mind to the same extent as the perusal of Dr. Geikie's memoir has done. His story had on the face of it an air of reality, but his statements were broad and general, and I could not help wishing that he had interpolated among his sweeping conclusions some details of the evidence on which those conclusions were based. I should have been sorry to miss the bold and strikingly graphic sections of his folding-plate; but I should have liked to have had, in the text, woodcuts, such as those which crowd Dr. Geikie's memoir throughout, of the actual exposures out of which those generalized representations had been constructed. It is easy to imagine good reasons for the omission of these details in Prof. Judd's paper, but all those who have made it a business of their life to cultivate a healthy tone of scepticism must have regretted their absence. No such charge can be brought against Dr. Geikie; more than sixty woodcuts, most of them representing actual sections, give ample opportunity for deciding for or against the sufficiency of his evidence.

We may now examine more in detail the main points of difference between the two readings; and, first of all, as to the vents from which the lava-flows were discharged.

[1] "The History of Volcanic Action during the Tertiary Period in the British Isles." By Archibald Geikie. LL.D., F.R.S., Director-General of the Geological Survey of the United Kingdom. Transactions of the Royal Society of Edinburgh, vol. xxxv., Part 2. (Edinburgh: R. Grant and Son, 1888.)

Dr. Geikie relies on the absence of any obvious vent from which the molten matter flowed. But surely the huge orifice of Strath, in Skye, was large enough to have served such a purpose. True there are appearances which seem to show that some of the plateau-basalts once extended right across the mouth of this funnel, but Dr. Geikie himself admits with perfect candour that the relation of this neck to the plateau-basalts does not admit of satisfactory treatment, owing to the destruction of the evidence by later intrusion of masses of granophyre in its immediate neighbourhood, and likewise to enormous denudation. I see nothing unlikely in the supposition that, from this enormous funnel, basaltic lava may have flowed in a manner to be shortly described; that the chimney became afterwards choked by agglomerate, too coarse to be spread far over the neighbourhood; and that, above all, basalt emitted from some new adjoining vent may have afterwards extended itself. Dr. Geikie further lays stress on the uniformity of the plateau-basalts in petrographical character, thickness, and persistent flatness, and on the almost total absence of interbedded fragmental deposits; and he maintains that these distinctive characters lead us to seek the modern analogues of the volcanic phenomena, not in large central cones like Vesuvius and Etna, but in the vast basalt-fields of Western America, where the lavas have issued from innumerable minor, and sometimes almost imperceptible, vents. With the first part of this opinion everyone must, I think, side with Dr. Geikie; but the method of formation which he advocates is by no means the only one possible or likely. Ever since I read Captain Dutton's account of the Hawaiian volcanoes,[1] it has seemed to me that it is to them we must look, if we are to understand the machinery by which great lava-plateaus have been produced. Speaking of the enormous flow which issued from Mauna Loa in 1855, he says: "As I looked over this vast expanse of lava, I was forcibly reminded of the great volcanic fields of the western portion of the United States, where the eruptions are of such colossal proportions that they have received the name of massive eruptions." After noticing Richthofen's view that these lavas had been poured forth through great fissures, and stating that the volcanic rocks of Western America, well as they are laid open to view, would be considered relatively obscure by one who has had an opportunity of inspecting the recent lavas of Mauna Loa, he goes on thus:—"I am by no means certain that Richthofen's conclusions are wrong. But here is a lava-flow, the dimensions of which fully rival some of the grand Pliocene outbreaks of the West, which demonstrably differs in no material respect, excepting in grandeur, from the much smaller eruptions of normal volcanoes" (*loc. cit.*, p. 156). But the differences between the modes of action of volcanoes of the Vesuvian and the Hawaiian types, whether we designate them as material or not, are striking enough, and they are just those which seem to have accompanied the discharge of the plateau-basalts we are now engaged with. Captain Dutton has well described them. "Mauna Loa and Kilauea," he says, "are in many important respects abnormal volcanoes. Most notable is the singularly quiet character of their eruptions. Rarely are these portentous events attended by any of that extremely explosive action which is characteristic of nearly all other volcanoes. The lava wells forth like water from a hot, bubbling spring; but so mild are the explosive forces that the observer may stand to the windward of the grandest eruption, and so near the source that the heat will make the face tingle, yet without danger. A direct consequence of this comparatively mild and gentle behaviour is the absence of those fragmental products which form so large a portion of the products of other volcanoes" (*loc. cit.*, pp. 84, 85). Fissure-eruptions are, to say the least, hypothetical; but here we have a way in which huge lava-fields, of the type of basaltic plateaus, are being produced before our eyes. The universally adopted canons of geological reasoning leave us no alternative as to which of the two explanations we should favour.

[1] United States Geological Survey, Fourth Annual Report, 1882–83.

But if the view just expressed be correct, we ought certainly to find some indications left, even among these ruined volcanoes, of the position of the vents from which the lavas issued. And here I cannot help going a long way with Prof. Judd in thinking that the great eruptive bosses of gabbro in Skye, Rum, Ardnamurchan, and Mull, are plugs filling in some of the main orifices of discharge. Prof. Geikie lays stress on the facts that the gabbros send off intrusive sheets into the plateau-basalts, and even overlie them. But this proves merely that the plugs which now fill the vents are later than the plateau-basalts: the vents themselves may be older. There must be some reason why the great intrusive bosses cluster thickly round a few centres, and are elsewhere conspicuous by their absence, and the following seems not unlikely. It was at these spots that vents were opened early in the volcanic period; from them there flowed, in the mild undemonstrative fashion of the Hawaiian volcanoes, the lavas which now build up the basaltic plateaus; as sheet was laid down upon sheet, the chimney gradually rose in height; and when, for this reason, and perhaps also on account of a temporary abatement of volcanic energy, the lava was no longer able to flow out at the top, it solidified in the vent, and, being under pressure, hardened into gabbro instead of dolerite. And indeed, though Dr. Geikie speaks of the eruption of the gabbro bosses as an event sufficiently marked and independent to characterize a distinct epoch in the volcanic period, he at the same time expresses himself in a way that shows he shares in the view I have just put forward, for he says: "We must remember, however, that the gabbro in many places found its readiest ascent in vents belonging to the plateau-period."

So far then the views of Dr. Geikie and Prof. Judd may admit of modifications which render them less conflicting than they seem at first sight. But there is one point on which reconciliation is impossible, viz. the nature and relative date of the eruptions of acid composition. Prof. Judd recognizes not only acid eruptions of the massive type—granites and their allies—but he speaks of thick bodies of felstones, disposed in regular sheets and of amygdaloidal structure, which alternate with beds of scoriæ, lapilli, and ashes, that lie upon the skirts of the central bosses of granite. These he believes to be the remnants of a volcano formed mainly of acid lavas, which was piled up and largely ruined by denudation before the discharge of the plateau-basalts began. The existence of the granite bosses admits of no doubt; but Dr. Geikie has depicted numerous sections which leave no doubt that these rocks intrude into the basalts and gabbros, and are therefore of later date than them. Now that all these details are before us, the question of relative age can admit of only one answer, but it is evidently a point on which observers, who had not opportunities of entering minutely into details, were apt to go wrong. Both Principal J. D. Forbes and Prof. Zirkel seem to have come to the same conclusion as Prof. Judd, and Dr. Geikie has supplied the explanation. "That there should ever have been any doubt," he says, "about the relations of the two eruptive masses is possibly explicable by the facility with which their junction can be observed. Their contrasts of form and colour make their boundary over crag and ridge so clear that geologists do not seem to have taken the trouble to follow it out in detail. And as the pale rock (granophyre or granite) underlies the dark (gabbro), they have assumed this infraposition to mark its earlier appearance." All this is graphically brought out in Fig. 43 of Dr. Geikie's memoir, which is reproduced here (Fig. 1). Anyone trusting to surface-feature

FIG. 1.—View of the hills on the south side of the head of Loch na Keal, showing the junction of the granophyre and the bedded basalts. One bird, the bedded basalts of the Gribon plateau; two birds, the bedded dolerites and basalts of Beinn a' Chraig adhering to the northern slope and capping the hill; three birds, summit of Ben More, with A'Chioch to the left and the top of Beinn Fhada appearing in the middle distance between them; four birds, the granophyre slopes of Beinn a' Chraig with the great dyke-like mass of felsite on the left.

FIG. 2.—Basic veins traversing Secondary Limestone and Sandstone on the coast cliffs, Ardnamurchan.

FIG. 3.—Outline of the hills of the Island of Rum, sketched from near the Isle of Eigg.

might well fancy that the basalts marked by two birds lay upon, and were newer than, the granophyre marked with four. Let us all take warning thereby.

But it is time to leave this perilous ground, and come to matters on which there can scarcely be difference of opinion. If it were desired to direct a student to a paper from which he could gather a clear and comprehensive view of the manifold forms under which volcanic products present themselves, not treated in the abstract but brought home to him by concrete examples, none could be found better fitted for the purpose than the memoir before us. And if a beginner would learn a lesson of the way in which a geologist goes to work when he wishes to unravel and interpret a complex group of geological documents, he will here find both precept and example. A point or two may be specially noticed. The enormous area which is seamed across by dykes, presumably of the same date, enables us to realize the importance of underground volcanic action, which is necessarily hidden from view in the case of volcanoes now in activity. I first learned this lesson while traversing a similar district, fully three times as large as that treated of by Dr. Geikie, in South Africa In connection with the striking parallelism of a large number of the dykes, reference is fittingly made to the classical paper of Mr. Hopkins, which he used so pathetically to complain had proved of interest neither to geologists nor mathematicians. But the mention of this paper again makes me lapse into criticism. When I first, many years ago, made acquaintance with Mr. Hopkins's investigations, two of his conclusions struck me as on the face of them so improbable physically, that, though I felt the presumption of the notion, I could not help suspecting some hitch in his analysis. One such oversight, so obvious that I can now hardly believe it to have been made by so first-rate a mathematician, I then detected. The other I have no doubt will reveal itself to careful inquiry. But from a hasty reperusal of the paper I do not think that either of these slips, supposing both to exist, affects the conclusions appealed to by Dr. Geikie; and the agreement, as far as they are concerned, between theory and observation is as complete as can be. The skill with which Dr. Geikie uses his pencil to bring out the geological features of a landscape is well known: that his right hand has not lost its cunning will be evident from the two illustrations here reproduced (Figs. 2 and 3).

Reference has been repeatedly made to the proofs of enormous denudation since Tertiary times which the volcanic rocks we are dealing with furnish in lavish abundance; it has not been so often noticed that denudation has during the same interval made its effects felt on harder and more intractable rocks. But dykes furnish proof of this in a way which I believe has not been made the subject of comment. "The evidence of this denudation," says our author, "is singularly striking in such districts as that of Loch Lomond, where the difference of level between the outcrops of the dykes on the crest of the ridges and the bottom of the valley exceeds 3000 feet. It is quite obvious that, had the deep hollow of Loch Lomond lain, as it now does, in the pathway of these dykes, the molten rock, instead of ascending to the summits of the hills, would have burst out on the floor of the valley. We are therefore forced to admit that a deep glen and lake basin have in great measure been hollowed out since the time of the dyke." A point this in favour of the "gutter-theory." A. H. GREEN.

THE THEORY OF PLANETARY MOTION.[1]

IN the work the title of which is printed below, Dr. Otto Dziobek seeks to develop the theory of the motion of bodies subject to attraction according to Newton's law. The author, in his preface, draws attention to the objectionable practice of the majority of writers of the present day, of treating the subject so briefly that many students scarcely get beyond Kepler's laws in their knowledge of the theory of the solar system. He has therefore prepared a work which is intended not only as an introduction to the study of this branch of astronomy but especially for those desiring an acquaintance with the higher productions of the masters in this science.

The book is divided into three sections. The first begins with the assumption of Newton's law, and then treats of the motion of two bodies about their centre of gravity, giving the usual deductions relating to the motion of the centre of gravity, to the projections upon the three co-ordinate planes of the areas swept out by the radius-vector in a given time, and to the form of the orbit described. In determining Gauss's constant of attraction, k, the author says that the unit of length is the major axis of the earth's orbit (he doubtless means semi-axis, though the statement is repeated on the same page, and a like oversight occurs on pp. 11 and 16); and then with 1 : 354710 as the earth's mass and 365·2563835 mean solar days as the length of the sidereal year, k is found = 0·017209895. This is the value found by Gauss, and given in his "Theoria Motus." This constant has been incorporated in many tables, and any change in its value would be attended with considerable inconvenience. But since the time of Gauss more accurate values of the earth's mass and of the length of the sidereal year have been found, and consequently a more accurate value of k may be deduced. To avoid this inconvenience, the above value of k is retained, and with the new values of the earth's mass and the length of the sidereal year the unit of length is determined. This unit of length is slightly greater than the earth's mean distance from the sun, but differs from it by less than a unit of the eighth decimal.

A collection of formulæ giving the relations between the radius-vector, the mean, eccentric, and true anomalies, as in Gauss's "Theoria Motus," is added, together with the usual expansions in series of these quantities. The expressions for the expansion of the eccentric anomaly and of the radius-vector by means of Bessel's functions are also added.

We next come to the general treatment of the problem of the motion of any number of bodies projected in any manner in space, and subjected only to their mutual attractions. Here, considering n bodies, we have the usual deductions relating to the invariable plane of the system, and to the sum of the products of the mass of each body into the area described by its radius-vector. The author then proceeds to simplify the case by discussing the motion when $n = 3$, and thus the case of the celebrated problem of the three bodies. Of this the usual outline is given, together with certain special cases of the problem, the lines of the investigations of Lagrange and of Jacobi being chiefly followed. A brief historical outline of the problem, and of the chief investigations thereon from the time of Lagrange up to almost the present day, closes the first section of the work.

The second section of the book treats of the general properties of the integrals introduced in the consideration of the problem of n bodies. The investigations of Poisson and Lagrange are discussed, and the development by these writers of formulæ for the elements of the elliptic orbit of a planet is given. And here, on p. 98, we again note the oversight before referred to, viz. that of putting a = the major axis of the orbit instead of the semi-major axis. Of course such a proceeding if it were carried on throughout would have no effect upon the developments which are obtained, except on their symmetry, but the author, after mentioning that the quantity a represents the major axis, immediately proceeds to use the quantity with its usual signification, viz. the semi-major axis. The oversight occurs again on p. 112, and again in discussing the canonical constants for the elliptic motion of a planet, and again

[1] "Die mathematischen Theorien der Planeten-bewegungen." By Dr. Otto Dziobek. (Leipzig: Johann Ambrosius Barth, 1888.)

in the investigations relating to the partial differential equation of Hamilton and Jacobi, where the author deduces Lambert's important theorem concerning the relations between the time of describing an arc of the orbit, the chord of the arc, the bounding radii of the sector, and the major axis. This last-mentioned theorem for the special case of the parabola was first discovered by Euler, a point on which the late Prof. Oppolzer used to insist; the extension of the theorem for any value of the eccentricity of the orbit being due to Lambert. A short historical sketch of the matter contained in this section, referring chiefly to the labours of Lagrange, Hamilton, and Jacobi, concludes this portion of the subject, and we come to the third section of the book.

This last section of the work treats of the theory of general perturbations. Here, of course, Lagrange's theory of the variation of constants plays an important part, and we have that part fully dwelt upon by the author. The development of the disturbing function is given, and here and there a simplification in the symbols might, we think, with advantage be introduced. In the expansion of $(r_1^2 - 2r_1r_2\cos\theta + r_2^2)^{-\frac{s}{2}}$ we have given the simple expression for half the coefficient of $\cos m\theta$ in terms of Gauss's hypergeometric series, viz.—

$$r_1^{-s}\frac{s(s+2)(s+4)\dots(s+2m-2)}{2m \,.\, m!}a^m\mathrm{F}\left(\frac{s}{2}, \frac{s}{2}+m, m+1, a^2\right)$$

The secular and periodic changes in the elements of the orbit receive the usual treatment, the stability of the solar system is discussed, and also the influence upon the results of terms in the higher powers of the eccentricity and inclination.

A few pages are also devoted to a point which writers are accustomed to say never occurs in the solar system—viz. commensurability of the mean motions of two planets. The importance of the subject treated in this section induces the author to extend the limits of the historical sketch with which he has concluded the two previous sections, and to give a little more fully the history of the important theory of perturbations; and he adds, in conclusion, that the best proof of the truth of Newton's law is in the discovery of the cause "of the observed irregularities in the motion of Uranus," a cause suspected by Bouvard and by Bessel, and a problem which death prevented the latter from undertaking, but which was "von zwei anderen Astronomen Leverrier und Adams gelöst." Speaking of the latter, the author remarks that "er seine Resultate einige Monate früher dem Astronomen Airy mittheilte"; the want of a "Durchmusterung," however, placed the optical discovery of Neptune in the hands of Dr. Galle.

At the end of the book are given a few small tables chiefly Leverrier's elements of the orbits of the major planets, except for Uranus and Neptune, Newcomb's more correct values of these quantities being adopted.

We note a few misprints. On p. 5, at the bottom, referring to the rotation of the axes, "$+x$ nach $+z$" should obviously read "$+y$ nach $+z$." On p. 11, for k_2 read k^2. On p. 45, in differentiating V, a homogeneous function of degree -1, the factor z of $\frac{dV}{dz}$ is omitted. On p. 46 it might be mentioned that $M = \Sigma m$. In the copy before us, pp. 225 to 240 are omitted, and pp. 273 to 288 have been bound in their place.

Regarding the whole book, we may say that there is much that may be found in any ordinary text-book on the subject. But the author has endeavoured to do more than give a mere sketch, as writers of the present day usually do, leaving the reader to search the pages of *Crelle's Journal*, the *Comptes rendus*, or some similar publication, for important papers connected with the subject. Where these have appeared useful, they have been introduced in a modified form if necessary; and where such papers are interesting, but beyond the scope of the present work, full references are given—a practice much to be commended. The author expresses a hope that he will be able to deal later with the theory of the rotation of bodies about their centres of gravity, the figure of the earth, &c., and with the theory of the tides; and we wish him the success which the present work augurs.

R. B.

NOTES.

Dr. Alfred R. Wallace has in the press a new work on Darwinism, which aims at establishing the theory of natural selection on a firmer basis, and also deals with the various supplementary theories which have been put forth since the publication of the sixth edition of the "Origin of Species." The book will be published early next year by Messrs. Macmillan and Co.

Prof. Giard's first lecture at the Sorbonne is published in the *Revue Scientifique* (December 1). It was delivered before a large audience, and many hundreds of persons had to be content to stay at the door. The Thursday lectures of M. Giard are devoted to an historical sketch of embryology in its relation to the theory of evolution. The Saturday lectures are devoted to embryological phenomena, considered generally.

On the 19th inst. a monument of the astronomer Leverrier is to be unveiled by the French Minister of Public Instruction, in the Cour d'Honneur of the Paris Observatory. The likeness of Leverrier is said to be very striking. The statue of Arago is finished, and has been sent to the foundry. It will be situated close to the Observatory Gardens, but is not to be put in its place until after the Exhibition.

The well-known botanist, Dr. C. J. de Maximowicz, writing from St. Petersburg to Kew about Prjevalsky, whose last book we review to-day, says:—"Yes, poor Prjevalsky is dead, and I mourn for him like a brother. He was a splendid character and a highly gifted man. He died with his Expedition fitted out and ready to start. Under these circumstances, the Russian Geographical Society intends to appoint, as head of the Expedition, Colonel Pentsow, a good topographer, who has already twice been in Northern Mongolia. Lieutenant Roborofski, Prjevalsky's associate, and a very capable officer and good collector, who did the botanical work during the two last journeys, is to go also. The plan is to remain the same, perhaps with the exception of Lhassa and the investigation of Northern Tibet. But the Society will appoint this time a geologist, which it is indeed high time to do."

We learn that the Hon. John Collier has just completed a portrait of Dr. A. W. Williamson, For. Sec. R.S. This portrait, which is to commemorate the thirty-eight years of Dr. Williamson's professorial work at University College, will be presented to the College by Sir Henry Roscoe, on behalf of the subscribers, on Wednesday, December 12, at 4.30 p.m. The subscribers to this portrait will give a dinner to Dr. Williamson on the same evening at the Freemasons' Tavern.

Last Saturday, a very large meeting, convened by the Council of the Teachers' Guild of Great Britain and Ireland, was held at the rooms of the Society of Arts, Adelphi, to consider a subject which is likely soon to attract much serious attention—the organization of secondary education. The Guild numbers among its Presidents some of the most eminent authorities on higher education, as Heads of Colleges, Professors of the English, Scotch, and Irish Universities, the President of the Royal Academy, Prof. Huxley, and Mr. Mundella. Sir Philip Magnus, who presided, said that at present no public body was responsible for the secondary education of the country. There was no

department which was cognizant of the secondary schools, or of the character of the education which they provided. He insisted that it was necessary that some kind of machinery should be brought into existence for several specified objects, including (1) provision of a sufficient number of efficient secondary schools duly related to one another, and to the elementary schools beneath them, and to the Universities above them; (2) the adaptation of the instruction given in these schools to the wants and requirements of different towns and districts; (3) the registration of teachers employed in these schools; (4) the utilization of the present enormous secondary scholarship fund to provide free places in these schools, and to enable pupils from the elementary schools to receive a good secondary education, and, in certain cases, higher technical or University education; (5) the regulation and inspection of all schools in receipt of funds derived from public sources, and the recognition of all private schools that submit to such inspection; (6) the annual publication of reports showing the number and distribution of schools, the curriculum of studies, the qualifications of the teachers, the character of the teaching appliances, and the general and sanitary condition of the schools. The meeting almost unanimously passed a resolution, proposed by Prof. Gladstone, F.R.S., to the effect that an Educational Council should be called into existence, in whose hands the organization of the secondary education of the country should be placed.

THE yearly volume of the *Kew Bulletin* for 1888 is now ready. This most useful publication, as our readers are aware, contains notes on the economic products of plants which have been made the subject of particular study and investigation at Kew, and it serves as a means of communication to persons interested in botanical subjects and products in India and the colonies. In the December number there are papers on Inhambane Copal, the cultivation of rice in Bengal, silkworm thorn, Jamaica india-rubber, seedlings of sugar-cane at Barbadoes, and ramie. In the paper on seedlings of sugar-cane at Barbadoes, attention is called to the fact that Mr. J. B. Harrison, Professor of Chemistry and Agricultural Science at Barbadoes, acting in conjunction with Mr. T. R. Bovell, superintendent of Dodd's Reformatory, has been engaged during the last three years in cultural and chemical experiments with various kinds of sugar-canes. A statement sent by Prof. Harrison appears to prove, in a perfectly natural and circumstantial manner, that a few mature seeds may occasionally be produced by the sugar-cane under certain circumstances. This discovery, if it is fully confirmed, may have an important effect on the practical treatment of the question whether the saccharine qualities of the sugar-cane are capable of being improved on the same lines as those successfully adopted with regard to the beet.

WE are glad to see that a German translation of Miss A. M. Clerke's "Popular History of Astronomy during the Nineteenth Century" has just been issued. A most appreciative review of the work appears in the current number of the *Naturwissenschaftliche Wochenschrift*.

MESSRS. CASSELL AND CO. have begun to issue, in monthly parts, a new edition of their well-known "Popular Educator." This work, we need scarcely say, has been of essential service to many a student who has undertaken in earnest the task of self-education, and in its new form it may be even more useful in the future than it has been in the past. The lessons are being revised throughout, and a large portion of the work will be entirely rewritten. Among the new illustrations is a series of coloured plates, prepared for the benefit of students of ethnology, geology, astronomy, physical geography, botany, &c.

MESSRS. MACMILLAN AND BOWES, Cambridge, announce that the first volume of the "Mathematical Papers," by Prof. Arthur Cayley, will be ready in January 1889.

A LARGE number of new fluorine compounds of the rare metal vanadium have been prepared by Dr. Emil Petersen, of Copenhagen. No fluoride of vanadium has hitherto been obtained, the only compounds previously known containing fluorine and vanadium being the fluoxy-vanadates of Baker, and a few other oxy-compounds, recently described by Piccini and Giorgio, which latter appear to have been independently obtained by Petersen. The most important of the new compounds is sesquifluoride of vanadium itself, V_2F_6, which has been obtained in fine large rhombohedrons of a dark-green colour, containing six molecules of water of crystallization, and very soluble in water. Next in importance are two probably isomorphous double fluorides of vanadium with potassium and ammonium, $V_2F_6 . 4KF . 2H_2O$, and $V_2F_6 . 4NH_4F . 2H_2O$; the former was obtained in the form of a bright-green crystalline precipitate, and the latter in brilliant emerald-green and tolerably large octahedra. Besides this compound with ammonium fluoride, another, of the composition $V_2F_6 . 6NH_4F$, was isolated in small grass-green, regular octahedra; this salt is especially interesting as being isomorphous with the analogous chromium and titanium compounds. To complete the isomorphous group, Dr. Petersen has also prepared the aluminium compound $Al_2F_6 . 6NH_4F$. Another interesting pair of isomorphous salts are the compounds $V_2F6 . 2CoF_2 . 14H_2O$ and $V_2F6 . 2NiF_2 . 14H_2O$, the former of which was obtained in dark-green, and the latter in grass-green monoclinic prisms. The remarkable similarity of the sesqui-vanadium and sesqui-chromium compounds is again beautifully shown by the fact that two precisely analogous salts containing cobalt or nickel and chromium instead of vanadium were successfully prepared, containing also fourteen molecules of water of crystallization and crystallizing in green monoclinic prisms. In addition to these important double fluorides of vanadium sesqui-fluoride, a number of oxy-fluorides, derived from vanadic anhydride, V_2O_5, and analogous to the well-known oxychlorides of phosphorus, have also been obtained in combination with alkaline fluorides. The two most important of these appear to be the oxyfluorides, $VOF_3 . 2KF$ and $VO_2F . 2KF$, the latter forming beautiful golden-yellow hexagonal prisms. This preliminary communication of Dr. Petersen, which will be found in the current *Berichte*, just received, forms a rich addition to our information concerning the element vanadium, and the details of the preparation of these well-crystallized salts, a small selection of which only have been described above, will be looked forward to with considerable interest.

THE new Ethnological Museum in Sydney, nominally opened last January, has now really been made accessible to the Australian public. The collection, which includes a large number of weapons and implements obtained from aboriginal races, is described by the *Sydney Daily Telegraph* as one "of absorbing interest."

WE have received Nos. 2 and 3 of vol. lvii. part 2, of the Journal of the Asiatic Society of Bengal. They contain notes on Indian Rhynchota, by E. T. Atkinson; a paper on the tornado which occurred at Dacca on April 7, 1888, by A. Pedler and A. Crombie; notes on the Amphipoda of Indian waters, by G. M. Giles; a paper on *Eupetaurus*, a new form of flying squirrel from Kashmir, by O. Thomas; and notes on Indian Chiroptera, by W. T. Blanford, F.R.S.

MR. THOMAS's account of the new form of flying squirrel from Kashmir, is very interesting. In connection with its dental evolution Mr. Thomas says it would be advisable for naturalists and sportsmen in Kashmir to notice what its food is, as compared with that of other squirrels. Judged from its blunt claws, it probably frequents rocks and precipices rather than trees, and it is therefore possible that its ordinary food may consist of lichens,

mosses, and other rock-loving plants, which, by being mixed with sand and particles of rock, would necessitate the development of such long lasting molars as it is remarkable for possessing. Additional specimens of *Eupetaurus* would be most valuable for scientific examination, especially if of different ages; and Mr. Thomas expresses a hope that some of the many British sportsmen who annually visit Kashmir will help to enrich either the Indian Museum in Calcutta, or the National Museum at home, with examples of this, the latest addition to the mammal fauna of our Indian Empire.

In the new number of the *Zoologist* Mr. T. Southwell has an interesting article on Pallas's sand grouse in Norfolk. Speaking of a large flock which Mr. Wood, of Morston, had under his close observation for some months, Mr. Southwell says that they frequented the same fields with great regularity; their favourite feeding-place being a large clover layer, from which, if disturbed, they flew across to some adjacent turnip-fields, choosing the bare patches for their feeding-ground. Here they spread over a circle of some 30 or 40 yards, separating, and diligently searching the ground until they appeared to have exhausted the food in that particular locality, when they all rose together and repaired to a fresh spot, which they exhausted in like manner. At stated times they departed for the salt marshes adjacent. The bird is usually extremely shy, but not always. Mr. A. Napier was shooting on the Holkham sand-hills with Lord Leicester and party, on October 13, when they met with a flock of about thirty-five. "A single bird," says Mr. Napier, "which I came upon, I felt convinced must have had either a nest or young. When first I saw it, it fluttered along in front of me just like a partridge with young. It was so tame that I called Lord Leicester and the others up to see it, and it did not fly up until we had approached to within 3 or 4 yards of it. At first I thought it must have been a wounded bird, but I do not think so now, for it flew away very strongly, calling out most lustily. Its action reminded me very much of the turtle-dove." Other incidents of a like kind are recorded by Mr. Southwell. On August 5 the gardener at Shernbourne Hall came to Mr. Parsons to say that a sand grouse was running about on the lawn. Mr. Parsons went out to catch it, thinking his son's pinioned bird had escaped. On being approached, the bird "ran and skulked in a little ditch," and did not rise till Mr. Parsons was about to put his hand on it, when it flew away "quite strong." Another, now in Mr. Gurney's aviary at Northrepps, was found, on October 31, floundering in a wet ditch at Suffield, and taken by hand.

We learn from the *Canadian Record of Science* (vol. iii. No. 3) that in June 1887 a small collection of graptolites was obtained by Dr. G. M. Dawson, on Dease River, in the extreme northern and inland portion of British Columbia, about lat. 59° 45', long. 129°. These fossils were derived from certain dark-coloured, carbonaceous and often calcareous shales, which, in association with quartzites and other rocks, characterize a considerable area of the lower part of the Dease, as well as the Liard River, above the confluence. In 1886 a similar small collection was obtained by Mr. G. R. McConnell, near the line of the Canadian Pacific Railway, in the Kicking Horse (Wapta) Pass. No other locality in the western portion of the Dominion has yet been found to yield graptolites. Prof. Lapworth, to whom Dr. Dawson's collection has been transmitted, thinks that the graptolite-bearing rocks are clearly of about Middle Ordovician age. They contain forms he would refer to the second or Black River Trenton period; *i.e.* they are newer than the Point Lévis series, and older than the Hudson and Utica groups. The association of forms, he says, is such as we find in Britain and Western Europe, in the passage beds between the Llandeilo and Caradoc Limestones.

The following are the lecture arrangements of the Royal Institution before Easter:—Prof. Dewar, six lectures (adapted to a juvenile auditory) on clouds and cloudland; Prof. G. J. Romanes, twelve lectures constituting the second part of a course on before and after Darwin (the evidences of organic evolution and the theory of natural selection); Prof. J. W. Judd, four lectures on the metamorphoses of minerals; Dr. Sidney Martin, four lectures on the poisonous action of albuminoid bodies, including those formed in digestion; Prof. J. H. Middleton, four lectures on houses and their decoration from the classical to the mediæval period; Prof. Ernst Pauer, four lectures on the characters of the great composers and the characteristics of their works (with illustrations on the pianoforte); and eight lectures by the Right Hon. Lord Rayleigh, on experimental optics (polarization; the wave theory). The Friday evening meetings will begin on January 25, when a discourse will be given by Prof. G. H. Darwin; succeeding discourses will probably be given by Prof. W. C. McIntosh, Sir William Thomson, Prof. A. W. Rücker, Mr. Harold Crichton Browne, Prof. Oliver Lodge, Prof. Archibald Geikie, the Rev. Alfred Ainger, the Right Hon. Lord Rayleigh, and other gentlemen.

The Russian naturalist, M. K. Nossilow, has been making geological investigations in Nova Zembla, and has discovered traces of gold.

Prof. Oliver J. Lodge writes to us as follows about his letter on the "Velocity of Sound" (Nature, November 22, p. 79):—"In equation (6), $U + v$ should, strictly, be $-U + v$, because the sign of U has changed with its signification. Equation (7) is therefore wrong. In the paragraph between equations (3) and (4), the words 'condensation' and 'rarefaction' should be transposed."

The additions to the Zoological Society's Gardens during the past week include two Squirrel Monkeys (*Chrysothrix sciurea*) from Guiana, presented by Master H. B. Young; a Silvery Gibbon (*Hylobates leuciscus* ♂) from Burmah, presented by Captain D. L. de la Chevois; a Pig-tailed Monkey (*Macacus nemestrinus* ♀) from Java, presented by Mr. W. Merryweather; a Polecat (*Mustela putorius*), British, presented by Mr. F. D. Lea Smith, F.Z.S.; a Raven (*Corvus corax*), British, presented by Mr. C. Petrzywalski; a Sparrow Hawk (*Accipiter nisus*), British, presented by Mr. G. Skegg; two Barn Owls (*Strix flammea*), British, presented by Mr. E. Hart, F.Z.S.; a Lion Marmoset (*Hapale rosalia*) from Brazil, deposited; a Blue-cheeked Barbet (*Megalæma asiatica*) from India, a Golden-crowned Conure (*Conurus aureus*) from South-East Brazil, a Golden-winged Parrakeet (*Brotogerys chrysopterus*) from the Amazons, purchased.

OUR ASTRONOMICAL COLUMN.

Stonyhurst College Observatory.—The Report of this Observatory for 1887, which has been recently published, is of the usual character, giving the results of the magnetic and meteorological observations for the year. The daily areas of the spots observed upon the sun during 1886 and 1887, expressed in millionths of the sun's visible hemisphere, are also given in both tabular and graphical form. The latter shows in a very striking manner the remarkable depression in spot-activity which marked the seven months from the end of September 1886 to the end of April 1887, and the regular series of gentle undulations which succeeded it. A note on the "Upper Glows in 1887" records that the white haze round the sun, and the pink "fore" and "after" glows consequent upon the Krakatão explosion, were still observed occasionally in 1887, but more feebly and less frequently than in 1886.

The Hopkins Observatory.—The little Observatory of this name attached to Williams College, Mass., is the oldest public Observatory in the United States, and during the past summer the jubilee of its dedication was duly celebrated. This interesting commemoration was made the occasion for the delivery of a discourse by Prof. T. H. Safford, Field Memorial Professor of Astronomy at Williams College, on the development of astro-

nomy in the United States, with especial reference to its earliest days; indeed Prof. Safford in his address went back not merely to the surveying work of Mason and Dixon, but even glanced lightly at the history of the institution where the former had been trained—Greenwich Observatory. The Hopkins Observatory was the work of the two brothers, President Mark Hopkins and Prof. Albert Hopkins, the latter of whom worked with his own hands at the erection of the building. Both were gifted men, and of advanced ideas, and their purpose in erecting the Observatory seems to have been the hope that the practical work of observing would increase their students' interest in the science, and develop their powers in fresh directions. It is still used by the students for occasional star-gazing, but for scientific purposes it has been superseded of late years by the meridian instrument of the "Field Memorial Observatory." The Hopkins Observatory was soon followed by others, at West Point, at Harvard College, at Washington, and other places, but though there had been previously one or two private observatories, and also a few telescopes in the possession of some public bodies, as, for example, at Yale College, yet until 1838 no permanent structure had been erected for any public observatory, so that the credit of being the pioneer of the long and distinguished succession of American Observatories belongs to the little building erected by the energy of Prof. Hopkins.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *DECEMBER* 9-15.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 9

Sun rises, 7h. 56m.; souths, 11h. 52m. 48·6s.; sets, 15h. 49m.: right asc. on meridian, 17h. 7·4m.; decl. 22° 54′ S. Sidereal Time at Sunset, 21h. 4m.

Moon (at First Quarter December 10, 7h.) rises, 12h. 36m.; souths 17h. 43m.; sets, 23h. 0m.: right asc. on meridian, 22h. 58·8m.; decl. 10° 51′ S.

Planet.	Rises.		Souths.		Sets.		Right asc. and declination on meridian.				
	h.	m.	h.	m.	h.	m.	h.	m.	°	′	
Mercury..	7	1 ...	11	7 ...	15	13 ...	16	21·8 ...	21	23	S.
Venus.....	10	41 ...	14	37 ...	18	33 ...	19	51·7 ...	23	4	S.
Mars......	11	4 ...	15	19 ...	19	34 ...	20	34·3 ...	20	5	S.
Jupiter....	7	52 ...	11	51 ...	15	50 ...	17	5·7 ...	22	27	S.
Saturn....	20	53*...	4	19 ...	11	45 ...	9	32·1 ...	15	40	N.
Uranus...	2	39 ...	8	4 ...	13	29 ...	13	18·4 ...	7	37	S.
Neptune..	14	54 ...	22	38 ...	6	22*...	3	54·6 ...	18	34	N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Variable Stars.

Star.	R.A.		Decl.							
	h.	m.	°	′				h.	m.	
U Cephei	0	52·4 ...	81	16 N. ...	Dec.	9,		23	25	*m*
					,,	14,		23	5	*m*
Algol	3	0·9 ...	40	31 N. ...	,,	9,		23	53	*m*
					,,	12,		20	42	*m*
U * Orionis... ...	5	49·2 ...	20	9 N. ...	,,	9,				*M*
T Monocerotis ...	6	19·2 ...	7	9 N. ...	,,	11,		3	0	*M*
ζ Geminorum ...	6	57·5 ...	20	40 N. ...	,,	9,		6	0	*m*
					,,	14,		6	0	*M*
R Canis Majoris ...	7	14·5 ...	16	12 S. ...	,,	9,		19	9	*m*
			and at intervals of					27	16	
T Canis Minoris ...	7	27·8 ...	11	59 N. ...	Dec.	9,				*M*
S Libræ	15	15·0 ...	19	59 S. ...	,,	12,				*m*
T Herculis	18	4·9 ...	31	0 N. ...	,,	9,				*M*
β Lyræ...	18	46·0 ...	33	14 N. ...	,,	13,		20	0	*m*
R Serpentis	19	10·1 ...	19	3 S. ...	,,	13,				*M*
S Aquilæ	20	6·5 ...	15	17 N. ...	,,	15,				*m*
T Vulpeculæ ...	20	46·7 ...	27	50 N. ...	,,	9,		19	0	*m*
Y Cygni	20	47·6 ...	34	14 N. ...	,,	10,		1	48	*m*
					,,	13,		1	42	*m*
δ Cephei	22	25·0 ...	57	51 N. ...	,,	11,		6	0	*M*
					,,	15,		0	0	*m*

M signifies maximum; *m* minimum.

* Mr. Gore's new variable discovered in 1885. This star has hitherto been more generally known as T Orionis, but as Mr. Chandler gives it the above denomination in his new Catalogue of Variable Stars, reserving T Orionis for the tenth magnitude variable in the great Orion nebula discovered by Bond in 1863, it will be well for observers, in order to avoid confusion, to follow his nomenclature.

Meteor-Showers.

	R.A.	Decl.	
Near Castor	108 ...	33 N. ...	Swift; short. The *Geminids.* Max. December 10-11.
From Leo Minor	144 ...	38 N. ...	Swift; streaks.
,, Sextans	145 ...	7 N. ...	,, ,,
Near λ Draconis	160 ...	70 N.	

GEOGRAPHICAL NOTES.

The rumour brought from the Cameroons as to the position of Mr. Stanley is too vague to be of much value. He is said to be behind "the Oil Rivers and the Niger," annexing territories wholesale for the British Crown. He may possibly enough be coming out in this direction. If so, he must have been with Emin, for it is inconceivable that, if able to get so far, he would fail in the chief object of his mission. If he has been with Emin, that must have been some time ago, and surely some word of it would have oozed out. We should not be surprised to find Mr. Stanley coming out by the West Coast; it would be quite in accordance with the purpose he had of settling, if possible, the problem of the Shari and Wellé. He may have sought to discover the parting that separates the basins of Lake Chad and the Congo, and the upper waters of the Binué. If he has really been on the Binué, we should have expected some definite news from the officials of the Royal Niger Company.

Though Mr. Joseph Thomson was summoned home from Morocco to lead an expedition to Emin Pasha, we regret to learn that the British East African Company are hesitating to carry out the purpose they entertained when they telegraphed for Mr. Thomson.

M. Rabot, in describing to the Paris Geographical Society the results of a visit which he recently made to Western Greenland, states the following conclusions:—In comparing the inland ice of Greenland with the glaciers of Lapland, it appears to him absolutely certain that the latter are nothing more than inland ice in miniature. The Lapland glaciers are simply the remains of the Glacial period in Scandinavia, which have persisted to the present time owing to special circumstances. The great glacier of Jakobshavn, on the west coast of Greenland, has been advancing during the last few years. Its front edge is at present 3 kilometres in advance of the point where it was seen by Lieut. Hammer in 1878. The drift ice of the south-west coast transports only a very small quantity of material. M. Rabot saw only one piece among fifty or sixty which bore debris of detritic origin, while traversing pack-ice 60 miles broad. Only one piece was black with earth.

In connection with Dr. Nansen's journey across Greenland, a paper by Dr. Rink, in No. 137 of the *Zeitschrift* of the Berlin Geographical Society, is of interest. Dr. Rink discusses the data which have been obtained by the various Danish Expeditions to Greenland, as well as by the parties which at different times have attempted to cross the land. He enters in some detail into the general subject of glaciation, and the relation between glaciers and icebergs. He seems to be of opinion that the ice of Greenland is shrinking, as he points out that there are evidences that at one time the ice covered the whole of the coastland, which is at present free, as well as the peninsulas and islands in its vicinity.

The same number contains a paper, by Dr. von Danckelmann, on the altitudes of the country at the junction of the Kassai and Congo.

In No. 8 of the *Verhandlungen* of the Berlin Geographical Society, Dr. Schweinfurth gives a useful sketch of his explorations in Egypt during the past fifteen years. In a letter to the President, in the same number, Dr. Hettner describes his observations on the Peruvian coast between Mollendo and Arequipa.

NOTES ON METEORITES.[1]

VI.

COMETS ARE METEOR-SWARMS WHICH HAVE ENTERED THE SOLAR SYSTEM SOME TIME OR OTHER.

THESE swarms, then, are comets. The final demonstration, as we have seen, we owe to the labours of Newton, Adams, and Schiaparelli chiefly. But long before their time the connection between shooting-stars (and even meteorites) and comets had been suspected on various grounds.[2]

Many shooting-stars pass through the air with a trail. This appearance is certainly suggestive of a very rapid comet. Hence, perhaps, it was that such an appendage, often noticed in the case of bright meteors, was sometimes in ancient records described as a comet. It is known that Cardano described as a comet the great meteor from which fell 1200 stones on the territory of Crema on September 4, 1511.[3]

Not only, as we have seen, Kepler (1600) regarded shooting-stars as akin in nature to meteorites, but he held that both had the same origin as comets:—"Falling stars are composed of inflammatory viscous materials. Some of them disappear during their fall, while others indeed fall to the earth, drawn by their own weight. Nor, indeed, is it improbable that they have been formed into globes from feculent materials mixed with the ethereal air itself, and thrown from the ethereal region in a straight line through the air like very small comets, the cause of the motion of both being hidden."[4]

Halley (1700) though he thought that the phenomenon of shooting-stars[5] was produced by a material disseminated through celestial space falling upon the sun and meeting the earth in its passage, did not associate it with cometary phenomena; but Maskelyne (1765) held that meteors were of celestial origin, and was inclined to assimilate them to comets. He wrote as follows in a letter to the Abbé Cesaris, the astronomer at Milan, about December 12, 1783:—"Freely accept, I pray you, this map, which I have lately published in order to stir up learned men rather than the unlearned, to observe more keenly the phenomena called fire-balls. In all probability they will turn out to be comets. . . ."[6]

To Chladni belongs the credit of having broached the theory which modern observations have established.

We have already seen that Chladni formulated the view, in 1794, that space is filled with matter. In 1819 he extended it by stating that both shooting-stars, meteorites, and comets were but different manifestations of it.[7]

Chladni made a step in this matter of which, as pointed out by Schiaparelli, only to-day are we able to appreciate the importance. In suggesting the cosmical hypothesis, he regarded two possible cases: either the meteors were formed of masses of independent materials which had never formed part of the larger celestial bodies, or they are the result of the destruction of a celestial body previously existing. Chladni held the second hypothesis as possible. but held to the first as more probable. He stated that we could not doubt the existence in the celestial space of small bodies endowed with movement, which are now and then visible by passing before the sun.

He held, therefore, that the small masses which appear under the forms of bolides and falling stars do not differ essentially from comets. It is also probable, he says, that comets consist of clouds composed in great part of masses of vapour and dust, which are kept together by mutual attraction. That this attraction is not enough to sensibly disturb the planetary movements is a proof of the exceeding tenuity and dispersion of the materials in such clouds, through which, however large, it is possible, to observe the fixed stars.[8]

In 1839 the Abbé Raillard suggested a connection between luminous meteors and comets and the aurora,[9] and Dr. Forster noted that the years marked by the appearance of a large comet are remarkable also for the abundance of falling stars, especially of white ones.[1]

Perhaps the first to give a more solid support to the cometary theory of falling stars on geometric grounds was Boguslawski, who conceived the idea of representing by means of parabolas the apparent orbits observed in some of the August meteors of 1837.[2]

For the next important advance in thought upon this subject we have to come down to 1858, in which year Baron Reichenbach published a most important memoir[3] attacking the question from an entirely new point of view. Reichenbach, accepting as proven by the then knowledge the most intimate connection between meteorites and falling stars, reasoned in the following manner, that both were connected with comets. He first recapitulated the facts then accepted with regard to comets:—

(1) Comets, both tail and nucleus are transparent.

(2) Light is transmitted through comets without refraction; hence the cometary substance can be neither gaseous nor liquid.

(3) The light is polarized, and therefore borrowed from the sun.

(4) Comets have no phases like those of moon and planets.

(5) They exercise no perturbing influences.

(6) Donati's comet (which was then visible) in its details and its contour is changing every day—according to Piazzi, almost hourly.

(7) The density of a comet is extremely small.

(8) The absolute mass is sometimes small (von Littrow having calculated very small comets, tail and all, as scarcely reaching 8 pounds).

From these data the following conclusions might be drawn:—

(1) That a comet's tail must consist of a swarm of extremely small but solid particles, therefore granules.

(2) That every granule is far away from its neighbour—in feet, so far that a ray of light may have an uninterrupted course through the swarm.

(3) That these granules, suspended in space, move freely and yield to outer and inner agencies—agglomerate, condense, or expand; that a comet's nucleus, where one is present, is nothing else than such an agglomeration of loose substances consisting of particles.

Hence we must picture a comet as a loose, transparent, illuminated, free-moving swarm of small solid granules suspended in empty space.

The next step in Reichenbach's reasoning was to show that meteorites (of which he had a profound knowledge) were really composed of granules.

He pointed out that these granules (since called chondroi) formed really the characteristic structure both of irons and stones, so that both orders were chiefly aggregates of chondroi—stony ones in iron meteorites, iron ones in stony meteorites.

In some irons, such as Zacatecas, they exist as big as walnuts, firmly adherent, but they can be separated; inside these are balls of troilite, often firmly embedded, so that on breaking the meteorite they will divide, but in other cases so loose that they fall out, and they are smooth enough to roll off a table.

Sometimes chondroi have smaller ones sprinkled in them, sometimes dark chondroi have white earthy kernels.

In some cases these chondroi are so plentiful as to form nearly the whole mass of the meteorite. They are often perfectly round, but not always, and they are often so loose that they tumble out and leave an empty smooth spherical cavity.

The stones chiefly consist of such chondroi and their *débris*.

He adds that each magnesic chondros "is an independent crystallized individual—it is a stranger in the meteorite. Every chondros was once a complete, independent, though minute meteorite. It is embedded like a shell in limestone. Millions of years may have passed between the formation of the spherule and its embeddal."

He then goes on to remark that the chondroi of meteorites indicate a condensation of innumerable bodies such as we see must exist in the case of comets; further, that they have been formed in a state of unrest and impact from all sides. Many meteorites are true breccias; they have *many times* suffered mechanical violence. He then shows that in comets we have precisely the conditions where such forces could operate, and

[1] Continued from vol. xxxviii. p. 605.

[2] For many references in what follows I am indebted to the historical notice in Schiaparelli's "Stelle Cadente."

[3] Humboldt, "Cosmos," iv. p. 587 (Otté). Cardani, "Opera." Lugduni, 1663, t. iii. p. 279. See also Schiaparelli, "Stelle Cadente."

[4] Kepler, "Opera," ed. Frisch, vol. vi. p. 157.

[5] Coulvier-Gravier et Sargey, "Introd. Historique," p. 5.

[6] *Memorie della Società Italiana*, vol. iii. p. 345, Verona, 1786.

[7] "Ueber Feuermeteore, und ueber die mit denselben herabgefaltenen Massen" (Wien, 1819). See also "Ueber den Ursprung der von Pallas gefundenen Eisenmassen," p. 24.

[8] "Feuermeteore," p. 395; see Kaemtz, "Meteorologie," vol. iii. p. 316.

[9] *Les Mondes*, t. xii. p 649, et t. xiii. p. 606.

[1] "Essai sur l'Influence des Comètes," &c. (Bruges, 1843).

[2] Coulvier-Gravier et Sargey, "Introd. Historique," p. 103.

[3] *Poggendorff's Annalen*, vol. cv. p. 438.

hence arrives at the view that "comets and meteorites may be nothing else but one and the same phenomenon." [1]

This was in 1858, eight years before Schiaparelli's discovery. Newton, as we have seen, referred the comet of 1862 to the largest meteorite in the August swarm.

We may assume from the work which has already been done that Reichenbach's view is more probably the true one, and that the head of a comet is merely the denser part of the swarm. Whether that denser part is at the end or at the beginning of the long line to which reference has been made, it does not very much matter, but where that is there we shall have the appearance of a comet presented to us in the heavens. That being so, we are able to apply everything that we have learned about comets to the movements of meteorites in space ; in the case of meteors and falling stars we were limited to what took place in our own air.

The Appearances presented by Comets away from the Sun.

When a comet first becomes visible, it appears in the telescope as a round misty body, and moves very slowly in consequence of its still great distance from the sun. At this time, too, its light is very feeble. Its appearance under these con-

Fig. 12.—A comet near aphelion.

ditions strikingly resembles that of a nebula, and in fact comets have often thus been mistaken for nebulæ.

Occasionally the appearance put on is that of a planetary nebula in small telescopes and a globular one in larger ones.

Fig. 13.—The Pons-Brooks comet, January 13, 1834 (Thollon).

The globular form, after a time, gives way, and the concentration of light is now a star-like concentration at one end of an elliptic patch.

[1] For this analysis of a part of Reichenbach's memoir, I am indebted to my friend Mr. L. Fletcher, of the British Museum.

In the next phase, both the star-like object and the elliptic patch lengthen, and the appearance becomes more like what is ordinarily recognized as a comet.

As the comet approaches nearer the earth, so that observations of its several portions may be seen, we get a still greater differentiation of the phenomena.

Fig. 14.—The first beginnings of a tail.

Fig. 16, which is a representation of Donati's comet as it appeared in 1858, will serve to illustrate the main characteristics of comets. The brighter part is called the *head* or *coma*, and sometimes there is within this a still brighter and smaller portion called the *nucleus*. The *tail* is the dimmer part radiating from the head,

Fig. 15.—The lower portion represents the elongation of the star-like luminosity ; the upper one. the concomitant extension of the whole comet (Comet 1882 October 25, Seabroke).

and this varies greatly in different comets ; it may be long or short, straight or curved, single, double, or multiple. The comet of 1744 had six tails, that of 1823 two. In others the tail is entirely absent. The tail of the comet of 1861 was 20,000,000 miles in length, and that of the comet of 1843 was 112,000,000 miles long.

Both head and tail are so transparent that all but the faintest stars are easily seen through them. The star Arcturus was seen through the tail of Donati's comet in 1858 at a place where it was 90,000 miles in diameter.

As a comet approaches the sun, its velocity, like that of the planets, increases, and it gradually gets hotter and gives out more light.

When the comet gets sufficiently hot, *aigrettes* or *jets* make their appearance ; these are so called because they seem to shoot

Fig. 16.—Donati's comet (general view).

out from the nucleus like sparks shoot out from a squib. The jets rapidly change their positions and directions, and the tail is formed, apparently at the expense of the matter of which the head was in the first instance built up. The tail is always turned from the sun, whether the comet be approaching or receding.

Drawings of a comet, as seen at different times, show how the jets vary in appearance and direction. Instead of jets, some comets present phenomena of a very different character, called envelopes, which are thrown off concentrically from the nucleus.

Fig. 17.—Comet with single nucleus (Cruls's comet, 1882, Riccò).

These are among the chief physical peculiarities about the heads of comets; and we see at once that we have something perfectly distinct from the planets, and that some comets are at first sight different from others. The envelopes have been observed to rise from the nucleus with perfect and exquisite regularity in exactly the same way that the jets swing backwards and forwards.

The enormous effect produced by a near approach to the sun may be gathered from the fact that the comet of 1680, at its perihelion passage, while travelling at the rate of 1,200,000 miles an hour, in two days shot out a tail 60,000,000 miles in length.

We must now enter somewhat more into details with regard to some of these cometary characteristics.

First of all, it must be pointed out that the meteoritic swarms

Fig. 18.—Nucleus surrounded by ellipsoidal head (Comet 1882 October 25, in Washington refractor).

are not always single, for in some comets the nuclei are double or triple.

In the case of single nuclei the nucleus may be the origin, and lie in the brighter region the extension of which forms the

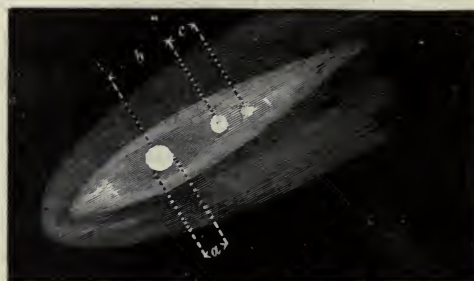


Fig. 19.—Compound nucleus (same comet November 5).

tail. But this is not invariable: the nucleus may be caught forming part of an elliptic head (Fig. 18) before any very great extension of the tail begins to take place, owing to reasons which will be stated further on.

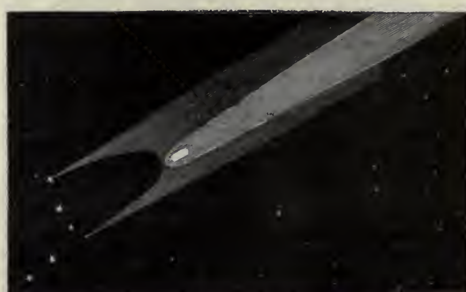


Fig. 20.—Commander Sampson's sketch of the great comet, 1882, October 10.

In the case of double or multiple nuclei we have a clear indication of the existence of more than one chief meteoritic swarm, whether they be enveloped in the same atmosphere or give rise to the same tail (Fig. 19). But it would seem that, in

some cases, different nuclei may give rise to separate tails; such would seem a possible explanation of Commander Sampson's observation of the comet of 1882 (Fig. 20).

J. NORMAN LOCKYER.

(*To be continued.*)

THE ANNIVERSARY MEETING OF THE ROYAL SOCIETY.

THE anniversary meeting of the Royal Society was held on Friday last, St. Andrew's Day. The President read the anniversary address—a copy of which has not yet reached us—and presented the medals. Prof. Huxley received the Copley Medal, and Mr. Crookes the Davy Medal in person. Prof. Osborne Reynolds was also present to receive one of the Royal Medals. The other Royal Medal was received on behalf of Baron von Mueller by Sir Graham Berry, Agent-General for Victoria, and the Rumford Medal, which had been awarded to Prof. Tacchini, was received on his behalf by the Chevalier Catalani, the *Chargé d'Affaires* at the Italian Embassy. The Society next proceeded to elect the officers and Council for the ensuing year. The selected names we have already published.

In the evening about 175 Fellows and guests dined together at Willis's Rooms. Among the guests were eminent representatives of the English Government, of foreign nations, and of art and literature. Sir Frederick Leighton, in proposing "The Royal Society," said:—

"A great honour is done to me in intrusting to my hands the toast which I have risen to propose, for it is the toast round which the chief sympathies of those who sit at this table are centred, be they hosts or be they guests—namely, prosperity to that ancient and honoured body, the Royal Society. It is, indeed, a toast favoured in this—that no inadequacy of presentment could rob it of your warm reception, but it is one, also, which, in one sense, the individual now before you is so little fitted to propose that I could almost suspect you, Sir, of a little prompting of humour in your selection. I do not mean because the bodies with which you and I have respectively the honour to be connected are now, in Piccadilly, as they were in former days in Somerset House, next-door neighbours, and because it is not habitually to one's next-door neighbour that one looks in life for a kind word, but on this other and more cogent ground—that the subject on which you bid me speak is one in regard to which I am entirely ignorant, and that my attitude is therefore not free from ludicrous aspects in the face of a body to which grasp and accuracy of knowledge is the one thing needful, and precision of statement the first duty of man; and this, Sir, certainly not least in the day of your headship. And yet, on closer view, it is not knowledge, perhaps, that you require of the proposer of this toast so much as respectful sympathy; and that you find in me to the full. No, gentlemen, you do not demand in me knowledge beyond that of the average ignoramus who watches you in wonder as you sound with divining eyes the realms of the heavens above and of the earth beneath and of the water under the earth, and lay bare before us the very beat of the life-pulse of Nature. You demand in me, I say, rather, some sympathetic sense of your magnificent missions, some adhesion to the faith that you profess, and for these you do not look to me in vain. It happens to me, Mr. President, from time to time to have to acknowledge words of recognition of the services of the great institution to which I am bound in a like capacity with your own; and, knowing how earnestly that body is bent on the worthy discharge of an arduous task, such words are deeply grateful to me, but in every such case I see in my inner mind, behind and above the institution which I serve, the sweet and serene countenance of our divine mistress—of Art herself; and so, also, in offering this toast to the acclamation of your guests and to the acceptance of your flock, I am thinking less of the noble services of your renowned Society, less of the many names which are its high adornment at this time and our country's pride, than of your mistress beneficent and supreme, the scatterer of darkness—Science. All of us walk in the daylight of her illumination, the humblest layman can bear witness to her, and the most ignorant concerning the paths she treads may yet not unbecomingly declare his gratitude to her ministers, and express, as I now express, the hope that they and their successors may in the bond of this constituted brotherhood long continue to tend the flame and feed the increasing splendour of her sacred inextinguishable lamp."

The President of the Royal Society responded in a short speech, in which he compared the Royal Society to a wave of light moving onward through space, conveying intelligence from one portion of the universe to another far-distant portion. The molecules which it set in motion had but a brief existence, but the wave moved ever onward.

SCIENTIFIC SERIALS.

THE *Journal of Botany* is still largely occupied with the discussion of points connected with botanical nomenclature, in which English, American, and Genevan botanists take part. The October number contains also a description of a new genus of Berberidaceæ by the Japanese botanist Tokutaro Ito.—In the November number are papers on the genus *Carex*, by Mr. L. H Bailey; on Ferns from West Borneo, by Mr. J. G. Baker; on South Derbyshire plants, by Rev. W. R. Linton; and on the Desmids of Maine, by Mr. W. West. Mr. W. H. Beeby records the interesting fact that of the two very nearly allied s ecies of valerian, *Valeriana Mikanii* and *sambucifolia*, one is very attractive to cats, while to the other they are quite indifferent.

IN the *Botanical Gazette* for September, Mr. C. Robertson completes his essay on zygomorphy and its causes, summing up the results of his observations. The remainder of the number is largely occupied by abstracts of botanical papers read at the Cleveland meeting of the American Association for the Advancement of Science.—In the October number are two important anatomical papers, by Miss Emily L. Gregory on the development of cork-wings on certain trees, and an illustrated one by Mr. W. H. Evans on the stem of Ephedra. Mr. G. Vasey contributes an interesting article on the characteristic vegetation of the North American desert.

THE number of the *Nuovo Giornale Botanico Italiano* for October 1888 is entirely occupied by reports of the papers read before the annual meeting of the Botanical Society of Italy held at Florence in September, many of which are of considerable interest.—Sig. C. Massolongo describes the germination of the spores of three new species of Sphæropsideæ—*Phyllosticta Bizzozeriana*, *P. Aristolochiæ* and *Phoma Orobanches*. He maintains that the only difference between pycnidia and spermogonia is that the sporules (stylospores) contained in the former are capable of germinating directly, while those formed in the latter (spermatia) have no such power.—Sig. A. N. Berlese adds to the very numerous fungus-parasites of the vine two new ones, *Greeneria fuliginea*, S. et V., and *Ascochyta rufo-maculans*, Berk.—Sig. G. Gasperini has investigated the nature of the organisms which bring about the fermentation of the palm-wine known to the Arabs under the name of "*legbi*." He finds it to be due to *Saccharomyces cerevisiæ*, which is always accompanied by *Bacillus subtilis*. On the surface is also commonly found a pellicle of *Saccharomyces Mycoderma*.—Prof. A. Borzi describes a new species and genus of Ascomycetes—*Eremothecium Cymbalariæ*, found on half-ripe capsules of *Linaria Cymbalaria*.—The little-known germination of the seeds of the water-lily, *Euryale ferox*, is described by Sig. G. Arcangeli, the chief peculiarity being the almost entire suppression of the elongation of the radicle.—Prof. L. Macchiati claims to have discovered an entirely new substance, which he calls *xanthophyllidrin*, as a constituent of the green colouring-matter of plants. It is crystallizable, and altogether distinct from xanthophyll and from the pigment of yellow petals.—Prof. A. Borzi describes the mode in which *xerotropism* displays itself in some ferns—*Ceterach officinarum*, *Notochlæna Marantæ*, *Asplenium Trichomanes*, and several species of *Cheilanthes*; understanding by this term the mechanical contrivances by which an organ protects itself against excessive desiccation.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 22.—"The Waves on a rotating Liquid Spheroid of finite Ellipticity." By G. H. Bryan, B.A. Communicated by Prof. G. H. Darwin.

The hydrodynamical problem of finding the waves or oscillations on a gravitating mass of liquid which when undisturbed is rotating as if rigid with finite angular velocity in the form of an ellipsoid or spheroid, was first successfully attacked by M. Poincaré in 1885.

Poincaré's analysis, however, did not appear to admit of any definite conclusions being formed as to the nature and frequencies of the various periodic free waves. The present paper contains an application of Poincaré's methods to the simpler case when the fluid ellipsoid is one of revolution (Maclaurin's spheroid). The solution is effected by the use of the ordinary tesseral or zonal harmonics applicable to the fluid spheroid and the auxiliary spheroid required in solving the differential equation.

Denoting by κ the ratio of the frequency of the free waves to twice the frequency of rotation of the liquid about its axis, the values of κ are the roots of a rational algebraic equation, and depend only on the eccentricity of the spheroid as well as the degree and rank of the harmonic, while the number of different free waves depends on the degree of the equation in κ. At any instant the height of the disturbance at any point of the surface is proportional to the corresponding surface harmonic on the spheroid multiplied by the central perpendicular on the tangent plane, and is of the same form for all waves determined by harmonics of any given degree and rank, whatever be their frequency, but the motions of the fluid particles in the interior will differ in nature in every case.

Taking first the case of zonal harmonics of the nth degree, we find that according as n is even or odd there will be $\frac{1}{2}n$ or $\frac{1}{2}(n + 1)$, different periodic motions of the liquid. These are essentially oscillatory in character, and symmetrical about the axis of the spheroid. Taking next the tesseral harmonics of degree n and rank s, we find that they determine $n - s + 2$ periodic small motions. These are essentially tidal waves rotating with various angular velocities about the axis of the spheroid, the angular velocities of those rotating in opposite directions being in general different.

With regard to the question of stability, the author shows that in the present problem, in which the liquid forming the spheroid is supposed perfect, the criteria are entirely different from the conditions of secular stability obtained by Poincaré for the case when the liquid possesses any amount of viscosity, which latter depend on the energy being a minimum. In fact for a disturbance initially determined by any harmonic (provided that it is symmetrical with respect to the equatorial plane, since for unsymmetrical displacements the spheroid cannot be unstable), the limits of eccentricity consistent with stability are wider for a perfect liquid spheroid than for one possessing any viscosity. If we assume that the disturbed surface initially becomes ellipsoidal, the conditions of stability found by the methods of this paper agree with those of Riemann.

Finally the methods of treating forced tides are further discussed.

The general cases of a "semi-diurnal" forced tide or of permanent deformations due to constant disturbing forces are mentioned in connection with some peculiarities they present, and the paper concludes with examples of the determination of the forced tides due to the presence of an attracting mass, first when the latter moves in any orbit about the spheroid, secondly when it rotates uniformly about the spheroid in its equatorial plane.

The effects of such a body in destroying the equilibrium of the spheroid when the forced tide coincides with one of the free tides are then considered.

Anthropological Institute, November 13.—Francis Galton, F.R.S., President, in the chair.—Dr. E. B. Tylor read a paper on a method of investigating the development of institutions applied to laws of marriage and descent. With the view of applying direct numerical methods to anthropology, the author had compiled schedules of the systems of marriage and descent among some 350 peoples of the world, so as to ascertain, by means of a "method of adhesions," how far each rule co-exists or not with other rules, and what have been the directions of development from one rule to another. As a first test of the results to be obtained by this means, Dr. Tylor first examined the barbaric custom which forbids the husband and his wife's parents (though on a friendly footing) to speak or look at one another, or mention one another's names. Some seventy peoples practise this or the converse custom of the wife and her husbands relatives being obliged ceremonially to "cut" one another. On classifying the marriage rules of mankind, a marked distinction is found to lie between those peoples whose custom is for the husband to reside with his wife's family and those where he removes her to his own home. It appears that the avoidance custom between the husband and the wife's family belongs preponderantly (in fourteen cases, as compared with eight computed as likely to happen by chance) to the group of cases where the husband goes to live with the wife's family. This implies a causal connection between the customs of avoidance and residence, suggesting as a reason that the husband, being an interloper in the wife's family, must be treated as a stranger; to use an English idiom expressing the situation, he is not "recognized." Other varieties of the custom show similar preponderant adhesions. Another custom, here called *teknonymy*, or naming the parent from the child, prevails among more than thirty peoples; as an example was mentioned the name of Ra-mary, or Father of Mary, by which Moffat was generally known in Africa. This custom proves on examination to adhere closely to those of residence and avoidance, the three occurring together among eleven peoples—that is, more than six times as often as might be expected to happen by chance concurrence. Their connection finds satisfactory explanation in the accounts given of the Cree Indians of Canada, where the husband lives in his wife's house, but never speaks to his parents-in-law till his first child is born; this alters the whole situation, for though the father is not a member of the family, his child is, and so confers on him the status of "Father of So-and-so," which becomes his name, the whole being then brought to a logical conclusion by the family ceasing to cut him. These etiquettes of avoidance furnish an indication of the direction of change in social habit among mankind: there are eight peoples (for instance, the Zulus) where residence is in the husband's family, with the accompanying avoidances, but at the same time avoidance is kept up between the husband and the wife's family, indicating that at a recent period he may have habitually lived with them. The method of tracing connection between customs was next applied, with the aid of diagrams, to the two great divisions of human society, the matriarchal and the patriarchal, or, as Dr. Tylor preferred to call them, the maternal and paternal systems; and the method showed that the drift of society has been from the maternal to the paternal system. Examination was next made of the practice of wife capture, recorded among about a hundred peoples, as a hostile act, a recognized and condoned mode of marriage, or a mere formality. It appears from the tables that the rules of human conduct are amenable to classification, so as to show by strict numerical treatment their relations to one another. It is only at this point that speculative explanation must begin, guided and limited in its course by lines of fact. In the words of Prof. Bastian, the future of anthropology lies in statistical investigation. Dr. Tylor's paper showed that the institutions of man are as distinctly stratified as the earth on which he lives, succeeding one another independently of difference of race and language, by similar human nature acting through necessarily changing conditions of savage, barbaric, and civilized life.

Royal Meteorological Society, November 21.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—Results of an investigation of the phenomena of English thunderstorms during the years 1857–59, by Mr. G. J. Symons, F.R.S. This paper was written nearly thirty years ago; it has now been communicated to the Society at the request of the Thunderstorm Committee. The paper contains a summary, chiefly in statistical form, of some of the results of an investigation into English thunderstorms and the accidents produced by lightning during the years 1857–59. The author found that in sheet-lightning the most prevalent colour is white, then yellow, blue, and red. In forked lightning the order is nearly reversed, blue being more than twice as frequent as any other colour, then red, white, and most rarely yellow. Sheet-lightning was seen about twice as often as forked.—Notes on the meeting of the International Meteorological Commitee at Zurich in September 1888, by Mr. R. H. Scott, F.R.S. The Committee recommended certain rules for the publication of data by travellers, &c., so as to insure their being useful for the advancement of sound climatological knowledge. The proposals for an international cloud nomenclature, as recommended by Mr. Abercromby and Prof. Hildebrandsson, did not commend themselves to the Committee, who suggested that the subject should be further studied. At the conclusion of the meeting the Committee was dissolved.—On a method of photographing cirrus clouds, by Dr. A. Riggenbach. The author exhibited some photographs of cirrus and other fine clouds which had been obtained by using the surface of the lake as a polarizing mirror.—Mr. A. C. Stratten exhibited some models of very large hailstones—spheres about $2\frac{1}{2}$ inches in diameter—which fell at Montereau, about forty miles south-east of Paris, on August 15, 1888.

Geological Society, November 21.—W. T. Blanford, F.R.S., President, in the chair.—W. Whitaker, F.R.S., who exhibited a series of specimens from the deep boring at Streatham, made some remarks upon the results obtained.—The following communications were read:—Notes on the remains and affinities of five genera of Mesozoic reptiles, by R. Lydekker. This paper was divided into five sections. In the first the author described the dorsal vertebra of a small Dinosaur from the Cambridge Greensand, which he regarded as probably identical with the *Syngonosaurus*, Seeley. The second section described an axis vertebra from the Wealden of the Isle of Wight, which is evidently Dinosaurian, and may possibly belong to *Megalosaurus*. In the third section the femur of a small Iguanodont from the Oxford Clay, in the possession of Mr. A. R. Leeds, was described. The imperfect skeleton of a Sauropterygian from the Oxford Clay near Bedford, which formed the subject of a previous communication, was redescribed. The paper concluded with a notice of the affinities of the Crocodilian genus *Geosaurus*.—Notes on the Radiolaria of the London Clay, by W. H. Shrubsole.—Description of a new species of *Clupea* (*C. vectensis*) from Oligocene strata in the Isle of Wight, by E. T. Newton.

PARIS.

Academy of Sciences, November 26.—M. Janssen in the chair.—On the difficulty of obtaining the exact latitude of the Paris Observatory, by M. Mouchez. In connection with M. Faye's recent communication on this subject, the author states that some improved instruments will soon be fitted up in the Observatory with a view to overcoming some of the almost insurmountable difficulties attending the accurate determination of the latitude of this spot. But even so, it is feared that perfect accuracy cannot be expected, the errors of a few tenths of a second being apparently due rather to the irregularity of the astronomic refractions in the Paris atmosphere than to defective instruments and errors of observation.—On the traction of canal and river craft, by M. Maurice Lévy. It is shown that in the present state of science the mechanical method of traction by means of the telodynamic cable is preferable to any electric system. Various improvements are also described, by which the author and his associate, M. Pavie, have succeeded in surmounting the many obstacles hitherto attending the successful application of the telodynamic cable to inland navigation.—Fresh experiments on the quantitative analysis of the nitrogen present in vegetable soils, by MM. Berthelot and G. André. The researches here described have been carried out for the purpose of testing the accuracy of the analyses hitherto made by various practised chemists operating at different intervals of time and under diverse conditions. Incidentally an attempt has also been made to determine the degree of stability possessed by the nitrogen present in different soils and exposed to varying influences.—On the results of the fourth scientific expedition of the *Hirondelle*, by Prince Albert of Monaco. This expedition, like the previous, was mainly confined to the Azore waters, which were explored in all directions during the summer of the present year. Amongst the improved appliances were two detachable bagging nets with 4000 metres of steel wire, a submarine electric lamp of the newest type, a Thibaudier sounding apparatus with 8000 metres of steel wire, special boats and camping fittings for the exploration of inland waters. Besides rich marine captures in depths ranging from 20 or 30 to 2200 metres, fourteen lakelets were visited, of which thirteen had never been explored and five not yet figured on any maps.—On the application of electrolysis to the treatment of tumours, by M. Darin. Since the recent foundation of the Henry Giffard Clinical Establishment in Paris, the author has effected several remarkable cures by this process. The apparatus is of a very simple character, easily controlled, and fitted with the chloride of zinc pile of the Gaiffe system.—On the determination of the coefficients of expansion at high temperatures, by M. H. le Chatelier. A new process is described, by means of which the author hopes to overcome the great difficulty attending the exact determination of these coefficients, which, apart from their scientific interest, are of such great importance for industrial purposes. The results of some preliminary experiments are given for Bayeux porcelain, iron, steel, and nickel at temperatures ranging from 20° to 970° C.—On an astatic electrometer, by MM. R. Blondlot and P. Curie. The instrument here described is a modification of Sir W. Thomson's electrometer with quadrants, and amongst the various uses to which it is applicable is that of a wattmeter.—Influence of water-surfaces on atmospheric polarization, and observation of two neutral points right and left of the sun, by M. J. L. Soret. Marine and lacustrine surfaces are shown to produce important perturbations on the phenomena of atmospheric polarization. Under certain conditions the curious phenomenon is also observed of two neutral points at the altitude, and to the right and left, of the sun. The polarization is then in a vertical plane between these points, and in the opposite direction beyond them.—On a new process of disinfecting the hands after surgical operations, by MM. Jules Roux and H. Reynès. The process in question is that recently introduced by M. Furbringer; but the experiments here carried out for the purpose of testing its efficacy have given unsatisfactory results in the case of microbes deposited under the finger-nails.—M. Charles Brongniart communicates a paper on Entomophthoræ and their application to the destruction of noxious insects; and M. Marcel Bertrand has a note on a new problem in the geology of the south of France, suggested by the appearance of certain Triassic marls cropping out above the Cretaceous rocks in the neighbourhood of Marseilles.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Lessons in Elementary Mechanics, 1st Stage: W. H. Grieve (Longmans).—A Classified List of Mr. S. W. Silver's Collection of New Zealand Birds (at the Manor House, Letcombe Regis): Sir W. L. Buller (Petherick).—The Alphabet of Economic Science, Part 1, Elements of the Theory of Value or Worth: P. H. Wicksteed (Macmillan).—The Civilization of Sweden in Heathen Times: O. Montelius, translated by Rev. F. H. Woods (Macmillan).—Review of the Planting and Agricultural Industries of Ceylon: J. Ferguson (Haddon).—Electric Bells and All about Them: S. R. Bottone (Whittaker).—Thirty Thousand Years of the Earth's Past History: Major-General A. W. Drayson (Chapman and Hall).—Practical Electrical Measurement: J. Swinburne (Alabaster).—Antiseptics: A. M. Hewer (Lockwood).—Catalogue of Canadian Plants, Part 4—Endogens: J. Macoun (Montreal, Dawson).—Kirchoff's Laws and their Application: E. C. Rimington (Alabaster).—Bibliography of Astronomy for the Year 1887: W. C. Winlock (Washington).—Energy and Vision: S. P. Langley.—Archives Italiennes de Biologie, Tome x. Fasc. 3 (Turin, Loescher).—Proceedings of the Academy of Natural Sciences of Philadelphia, Part 2. 1888 (Philadelphia).—Annalen der Physik und Chemie, 1888, No. 12 (Leipzig, Barth).—Brain, Part 43 (Macmillan).—Geological Magazine. December (Trübner).

CONTENTS.

THURSDAY, DECEMBER 13, 1888.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873–76, *under the command of Captain George S. Nares, R.N., F.R.S., and the late Captain Frank T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., &c., Director of the Civilian Staff on board, and now of John Murray, LL.D., Ph.D., &c., one of the Naturalists of the Expedition. Zoology—Vol. XXVII. Published by Order of Her Majesty's Government. (London: Printed for Her Majesty's Stationery Office, and sold by Eyre and Spottiswoode, 1888.)

THE first Report in this volume is by Prof. J. R. Henderson, M.B., on the Anomura. Some time after the return of the *Challenger*, the collection of Anomura was placed in the hands of Dr. Jules Barrois; but, not finding time sufficient for their investigation, Dr. Barrois was compelled to return them, and they were placed in Dr. Henderson's care towards the end of 1884.

This group of Crustacea, taken in the sense of Dana, is intermediate between the Brachyura and the Macrura, but in the classification adopted in this Report the author has, to a certain extent, followed the arrangement of Boas, though retaining the Dromidea and Ranidea within the limits of the group—this latter not without some hesitation.

The Anomura are found in all seas, but much more abundantly in those of tropical or temperate climates; some few forms are terrestrial or fluviatile. The greater number inhabit shallow water or moderate depths; two groups, however, the Pagurids and the Galatheids, are numerously represented in the great ocean depths. The collection contained 161 species or well-marked varieties, referable to fifty-two genera, and of these, over one-half of the species (eighty-six) and seven of the genera are described as new. While some of the common shallow-water forms are absent from the collection, still it adds very considerably to our knowledge of the distribution even of this section.

The main interest, however, is in the deep-sea forms, and these chiefly belong to the Paguridea and the Galatheidea, more than four-fifths of the species taken belonging to these groups, and the latter contains the greater proportion of the species.

While the structural modifications met with in the deep-sea species of the Paguridea are comparatively few and unimportant, in the species of Galatheidea the abyssal forms are blind, and the eyes have undergone a process of degeneration which is tolerably uniform in all. In the majority of the species—with the exception of those belonging to the genus Munida—the eggs carried by the females were found to be few in number and of remarkably large size, leading to the inference that their enemies were but few. No facts in reference to their coloration were observed, as the strong alcohol in which the specimens were preserved reduced them all to a dull white colour. Thirty-one plates accompany this Report.

The second Report is by Prof. P. Pelseneer, on the anatomy of the deep-sea Mollusca. The material for this Report could not be placed in Dr. Pelseneer's hands until the systematic Report on the species had been completed, and it consisted exclusively of Gastropoda (not including Isopleura or Amphineura) of Scaphoda, and of Pelecypoda; there was no great wealth of either species or of specimens. Of certain forms there were but single specimens, and in the case of others the soft parts had been injured in removing them from the shells; still, many new and very interesting facts have been noted and recorded, the chief conclusions from which, so far as the special sense-organs are concerned, are as follows. The organ of vision may atrophy and disappear, in consequence of the absence of sufficient light, in great depths; correlatively, the organs of general sense may multiply, and acquire a high degree of development, such as the labial palps of *Trochus infundibulum*, the siphonal tentacles of varied structure in the deep-sea Anatinacea and in Malletia; and, lastly, the respiratory activity may diminish, and the gills become rudimentary in various ways, or these may retain a great simplicity of structure. Four plates accompany this memoir.

The third Report is by Prof. W. C. McIntosh, F.R.S., on *Phoronis buskii*, n. sp. The genus Phoronis was established in 1856, by Dr. Strethill Wright, for a minute Polyzoan, about 1½ millimetre in length. Since this species (*P. hippocrepia*, St. Wright) was described, other species, also of more or less small size, have been described by various authors, from the coasts of Scotland, the Mediterranean, and Eastern America. One, from Australia, is of considerable length. The species dredged by the *Challenger* in shallow water at depths varying from 10 to 20 fathoms (Station 212) south of the Philippine Islands, measures 52 millimetres in length, with an average diameter of about 2 millimetres at the anterior region, and of 4 to 5 millimetres at the enlarged posterior end. The tentacular or branchial region is from 6 to 7 millimetres in length. There has always been a great deal of interest taken in the species of this genus, owing to their strange metamorphosis, and to the uncertainty that seemed to attach to their position in the animal kingdom; this now is settled to be in Ray Lankester's section "Vermiformia," of the Polyzoa. In this Report the minute structure of the *Challenger* species, called after the late George Busk, is given in detail, and figured on four plates. The history of the development of this fine species remains to be written.

The fourth Report is by Prof. W. A. Herdman, on the Tunicata. The first part of this Report, published in 1882, treated of the Simple Ascidians, while the second part, published in 1886, was occupied mainly with an account of the Compound Ascidians, with a supplementary account of some Simple Ascidians which had been found after the publication of the first part. The present part treats of the "free-swimming" Ascidians, which, however, fall into three very distinct groups, less allied to one another than the Simple Ascidia are to the Compound Ascidia. One of these is the group of Salpiform Ascidia; the other two are the Thaliacea, including

such genera as Doliolum and Salpa, and the Larvacea, containing the Appendicularia.

By far the greater number of the pelagic Tunicata collected during the voyage of the *Challenger* belonged to the genus Salpa. Of these, vast numbers were taken at the various stations, so that a great deal of labour was spent in a critical examination of these before it could be determined that they were all the same or different species.

The collection of pelagic Tunicata contained about twenty-six species, of which nine are new to science. No new genera are established, but a new family, the Octacnemidæ, has been formed for the reception of the remarkable deep-sea genus described by Moseley as Octacnemus.

Prof. Herdman gives in some detail, at the close of his account of the species and of their geographical and bathymetrical distribution, the conclusions at which, after a prolonged study of this group, he has arrived, as to their relationship and phylogeny; this is accompanied by a graphic representation of the phylogeny of the Tunicata. This important Report is illustrated by eleven plates.

THE BRITISH FARMER AND HIS COMPETITORS.

The British Farmer and his Competitors. By W. E. Bear. (London: Cassell and Co., Limited, 1888.)

THIS small volume of 160 pages is in some respects a reprint of articles published in the *Quarterly Review*, revised and brought down to date. The first chapter is devoted to the condition of British agriculture, in which the somewhat Radical doctrines of the Farmers' Alliance (an organization which has never succeeded in winning the confidence of the farmers) are promulgated. "Before this country will be cultivated to the best advantage, those who cultivate it must be either the owners of their farms, or tenants who are entitled to sell their improvements to the highest bidder, and who are free to crop the land as they please, provided that they be liable for actual damage done to the property of the owner." "Our farmers must have complete security for their capital invested in improvements, and freedom of enterprise as well, if they are to do the best they can with the land." This is the panacea for agricultural distress, and yet we may well ask why it is that Britain is exceptionally well cultivated, and that farmers as a rule farm as well or better than landlords? Landlord farming has, in fact, for the most part not been satisfactory, either when carried out on the large or on the small scale, and whether any advantage would accrue from its extension is exceedingly doubtful. Allotments, too, are put forward as amongst the requirements of our time, and small farming is also advocated, although condemned by experience.

Mr. Bear is more happy as a statistician than as a politician, and his chapters upon foreign competition and the prospects of the wheat-grower, and the breeder and feeder of live stock, are deeply interesting. The first welcome truth is that in almost all articles of agricultural production the crisis of injurious foreign competition appears to have been passed about the year 1883 or earlier. Such was the case up to date, with regard to wheat, barley, oats, and cattle. The maximum importation of sheep, hops, and potatoes, took place in 1882; of bacon, hams, and preserved meats in 1880; of pigs and cheese in 1878; and of beans in 1877. It must be understood that quantities in quarters and hundredweights, and not values, are indicated; and so considerable has been the shrinkage that the present imports of meat fall short of the maximum reached some eight years ago by about one million hundredweights, chiefly bacon.

Taken in connection with this diminution of foreign supplies of grain and meat, is to be noted the increase in population, not only at home and in Europe, but throughout our colonies and in the United States. In the last-named country alone, population has increased from 38,500,000 in 1870 to 62,500,000 at the beginning of the present year, and it is estimated that it will have reached 66,000,000 by 1890. During the five years ending with 1884 the average annual consumption of wheat in the United States was nearly 324,000,000 bushels, and the average export was 140,000,000 bushels. If the production in the five years ending with 1894 does not become greater, all but 43,000,000 bushels, or less than 5,500,000 quarters, will be required for home consumption, and the surplus will not suffice for the increased population of the next five years. Thus, unless the area of wheat-growing is greatly extended, the United States must cease to be a wheat-exporting country before the close of the present century! There is certainly a somewhat large "if" to swallow in accepting this statement, but it seems pretty evident that wheat-growing is not profitable at present prices, and that American farmers are becoming tired of it. Higher prices can alone cause the necessary increased supply, and the influence of such higher prices would be found in Europe to the advantage of the farmers. Considerable space is devoted to show that American and Canadian farmers grow wheat at a direct loss. It appears that the average gross money return from an acre of wheat in the United States is £1 13*s*. This figure is based on official information, and is arrived at by a yield of 12·2 bushels per acre, and a price of 68·1 cents. the bushel. As, however, the farmers have been often obliged to sell at 48 cents. per bushel, and the yield is in one State, not 12·2 but, 5 bushels, and in another 7 bushels, and in eight States it is below 8 bushels, the gross value of an acre of wheat must in many cases fall much below the average. It is held that unless 20 to 25 bushels can be secured no profit is possible. The cost of growing an acre of wheat in the States cannot, it appears, be placed under 14·11 dollars per acre, *i.e.* about or near £2 18*s*.; and if these figures are even approximately correct, the wheat-growers of the Far West must be in a worse plight than our own. Although nominally rent free, the Western farmers have generally been obliged to mortgage their farms at an interest of from 8 to 10 per cent. per annum, and according to one authority, "teams, tools, stock, and grain, all are being rapidly mortgaged." It is generally admitted that the American farmer's life as a rule is one of "excessive and almost incessant toil, and the scantiest reward—in money, at any rate; while his wife is held up in America as a common object of pity." With such encouragement, Mr. Bear does not expect wheat-growing to spread in America unless prices generally rise.

The second part of the book is devoted to our meat supply and dairy produce. The greatest scare among home meat-producers has been occasioned by the increasing imports of frozen meat—chiefly mutton. The future of this trade is, however, very dubious, and exportation completely collapsed when prices fell in 1886 and 1887. A New Zealand colonist, writing to the *Otago Witness*, says:—"The producer, when he sends his meat to London, realizes about 4½*d*., perhaps only 4*d*., per pound; and when he deducts expenses, say 2½*d*., he has only about 1½*d*. per pound for the choice of his flock. Now, this will not pay him, and some of our largest exporters of meat have decided that it will not pay them to send home their meat."

Mr. Bear's views on the future of English farming are, on the whole, hopeful, but he is accused by some of his critics of being an optimist. He has also brought down the wrath of the Canadian Press upon him for decrying the climate of Manitoba as a wheat-growing area, and discounting the reports of its fertility. As published under the auspices of the Cobden Club, the bias of the work is in favour of free trade, if we may except the trade in live stock, where contagious diseases are involved. Some political or economical bias ought to be accepted as inseparable from a book so issued. The reader will no doubt exercise judicious discrimination in accepting all the deductions, but will not fail to see that Mr. Bear's arguments are well supported by facts and official figures. The book is, in fact, a valuable contribution towards the solution of a question of vast importance—the future of our agriculture.

COLEOPTERA.

Biologia Centrali-Americana—Zoology: Coleoptera. Vol. I. Part II. By David Sharp, M.B., F.Z.S., &c. (London: R. H. Porter, 1882–87.)

ALTHOUGH nearly six years have been required for the completion of this volume, entomology has received a valuable contribution, which is at the same time an evidence of the untiring industry of its author and of the great liberality and enterprise of its editors. The volume covers about 840 pages, illustrated by nineteen plates, including in its scope nine divisions of the Coleoptera, called families, as follows: Haliplidæ, Dytiscidæ, Gyrinidæ, Hydrophilidæ, Heteroceridæ, Cyathoceridæ, Parnidæ, Georissidæ, and Staphylinidæ, in dealing with three of which Dr. Sharp had already shown a rare combination of analytical power and synthetic skill.

While the arrangement of the families in the order indicated above might be criticized as somewhat unnatural, it is to be presumed that the convenient division of labour among the different authors, and the approximation of the labours of each in one volume, had more to do with the sequence than the desire to indicate affinities.

The family *Haliplidæ* presents no point worthy of special mention; there are three new species in a total of six.

The *Dytiscidæ* is represented by 168 species, of which about seventy-one are new, nearly all small species, while the thorough analytical study previously given to the family by Dr. Sharp has left but one generic division to be indicated.

The *Gyrinidæ*, represented by twenty species, of which four are new, presents nothing of note, except the evident tendency of Gyretes to replace Gyrinus in the warmer parts of America.

The *Hydrophilidæ* contains 141 species, four-fifths of them new, requiring the indication of thirteen new genera. In the study of this family, Dr. Sharp gives evidence of the close attention he has devoted to it from the commencement of his career as an author, and he has shown how much new work may be done even in those families moderately well studied. The point seems well taken that the Hydrophilidæ constitute a family, and not a complex equivalent to the Adephaga. While no new arrangement of the family is proposed, the inaccuracy of our present method is shown, and numerous structural differences are indicated, which may form the basis of a better system when more is known of the genera from other regions than Europe and North America.

The next four families, Heteroceridæ, Parnidæ, Georissidæ, and Cyathoceridæ are all of small extent, containing between them but fifty species. While these are closely related among themselves, their position in mass between the Hydrophilidæ and Staphylinidæ is unnatural, and obscures their evident relationship with the Byrrhidæ and certain Dascyllidæ.

The greater portion of the volume is occupied with the treatment of the Staphylinidæ, in which more than 1400 species are enumerated, seven-eighths of them new; of the remaining eighth a fair proportion had already been described by Dr. Sharp elsewhere. The mere numerical statement will give but an inadequate idea of the labour expended in this part of the volume. Those who have had occasion to deal with the Aleocharinæ will realize the amount of minute examination required, almost ruinous in its effects on the eyesight. It is evident that the Staphylinide fauna of Mexico is far from being exhausted, and had as enthusiastic collectors as Mr. G. C. Champion collected in other parts as he did in his regions, it is safe to believe that the number of species would have been more than doubled. In a notice like the present it seems unnecessary to enter more deeply into details. Although much has been done in Staphylinidæ, our knowledge of the fauna of Europe is the only one approximately complete, and it gives a very narrow basis for comparison.

As a whole, Dr. Sharp's work will receive the recognition due to careful, conscientious, and erudite labour. It is to be regretted that the descriptions are at times too brief; and how much difficulty future students may find in following them may be inferred from the experience of Dr. Sharp with the longer and very able descriptions of Erichson.

The volume concludes with nineteen plates, with about 450 figures, which will prove useful in the identification of the species. The omission of details is to be regretted, although their representation would have given Dr. Sharp an amount of labour which he could hardly be expected to undertake.

G. H. H.

OUR BOOK SHELF.

A Sequel to the First Six Books of the Elements of Euclid. Fifth Edition. By J. Casey, F.R.S. (Dublin: Hodges, 1888.)

THIS handy book has been a decided hit, and has supplied something that was really needed. The main body of the work is little altered in the present edition, but corrections have been made of slips which we had occasion to point out. The special part, *i.e.* the supplementary chapter on the recent elementary geometry, continues to grow. In the last edition, pp. 165–222 were devoted to it; in this, pp. 165–248. Additional articles are devoted to Taylor's circle (Mr. Taylor's paper in the *Messenger of Mathematics*, vol. xi., appeared before his article in the Mathematical Society's Proceedings, vol. xv., and some of the properties of it were given in a Trinity College, Cambridge, examination paper (*l.c.*); see, however, Simmons, "Recent Geometry," in "Milne's Companion," p. 181). Much of Section vi., on "The Theory of Harmonic Polygons," has been rewritten, and indebtedness to Messrs. Neuberg and Simmons is admitted. The impression conveyed to a reader is that the latter's important article, referred to above, has not been seen by Dr. Casey, for, if it had been seen by him, frequent reference must, we should suppose, have been made to it, whereas the only reference is to a note in the Mathematical Society's Proceedings, April 1887. We now commend the article in question to Dr. Casey's notice. Section vii., on the "General Theory of Associated Figures," is for the most part new to the volume,[1] and there are additional exercises. We would point out that Questions 76, 77 (p. 217, fourth edition, and p. 241, fifth edition) are not consistent. 76 is right; in 77 read, for "*orthocentre* of pedal triangle," "symmedian point," as in the author's "Conics," p. 325.

Elementary Theory of the Tides. By T. K. Abbot, B.D. (London: Longmans, Green, and Co., 1888.)

FULL discussions of tidal action and its effects have hitherto been confined to treatises which employ higher mathematics, and any successful attempt to simplify matters ought therefore to receive a hearty welcome. The book before us is an attempt at this, and although it only consists of some forty pages, it simplifies many points. The proofs of the various theorems require no special knowledge beyond that of the resolution of forces, but the quantitative determinations necessarily demand a little mathematical knowledge.

There is a common notion that without friction there would be high water under the moon, but Mr. Abbot easily demonstrates that it would occur at quadratures. A simple construction is given and proved for the determination of the amount of the disturbance at any point on the equator. The influence of tides upon the length of the day is also discussed. Airy's analytical method is given in an appendix.

The book is mainly a compilation of papers by the author which were published in 1871-82 in the *Philosophical Magazine* and other journals.

Pictures of Native Life in Distant Lands. Depicted by H. Leutemann. With Explanatory Text by Prof. A. Kirchoff; translated from the German by George Philip, Jun. (London: George Philip and Son, 1888.)

TWELVE coloured plates, illustrating what are called the typical races of mankind, are brought together in this volume. The subjects have been well selected, and the workmanship of the pictures is sufficiently good for the artist's purpose. The letterpress, by Prof. Kirchoff, contains much valuable information, and it has been translated by Mr Philip into clear and simple English. The work will both amuse and instruct any young readers who may be fortunate enough to obtain a copy.

[1] M. Torry's paper reached the author as his fourth edition was in the press, see pp. 221, 222 of that edition.

The Zoo. By the Rev. J. G. Wood. (London: Society for Promoting Christian Knowledge, 1888.)

MR. WOOD is so well known as an expounder of the facts of natural history that it is unnecessary to say much about the present volume. His object is to interest children in some of the animals which they may see in the course of a visit to the Zoological Gardens. He begins with an account of monkeys, and then goes on to talk about lions, tigers, leopards, the chetah, the jaguar, the lynx, wolves, foxes, hyænas, the Aard wolf, bears, and the racoon. In each section he contrives to say something that is worth remembering, and perhaps a good many of his descriptions will be at least as interesting to older readers as to the young people for whom they are primarily intended. The book is prettily illustrated.

Alpine Winter in its Medical Aspects. By A. Tucker Wise. Fourth Edition. (London: J. and A. Churchill, 1888.)

THE present edition of this work contains all the subject-matter of previous publications of the Alpine climate series, with extracts from Dr. Wise's papers read at the Harveian Society of London, the Royal Meteorological Society, and the International Medical Congress held at Washington in September 1887. The writer knows his subject thoroughly, and he has too ardent a belief in the treatment of chest disease in the mountains to wish to make extravagant statements about it. In his representations of Alpine climate he has not forgotten to include those unpleasant details which are generally somewhat vaguely described as "drawbacks." The work contains a series of careful notes on Davos Platz, Wiesen, St. Moritz, and the Maloja.

Animal Physiology. By William S. Furneaux. (London: Longmans, Green, and Co., 1888.)

IF the necessity be granted that a separate text-book should be published to meet the requirements of every examination body in each department of learning, Mr. Furneaux may be said to have met his share of that necessity, and with more success than many of those who have recently set to themselves a similar task. The book covers a slightly wider field than that indicated by the "Elementary" Syllabus of the Science and Art Department for human anatomy and physiology; it is clear and well arranged, and the illustrations are good and carefully selected. Such loose statements as that "bone is produced by the gradual hardening of cartilage" (p. 34) appear to be almost a necessity in works of this stamp, but with Mr. Furneaux they are unusually rare. The volume forms one of Messrs. Longmans' series of "Elementary Science Manuals."

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Hailstones.

A SEVERE hailstorm occurred here yesterday evening, between 6 and 7 o'clock, and lasted for about a quarter of an hour. The hailstones being fairly large, I was able, by the aid of a good lens, to examine them somewhat carefully. The result of this examination I give shortly in the following statement.

All the hailstones were pyramidal in form, and the pyramids were nearly all four-sided. Their bases were in almost every instance more or less rectangular in outline, the contours of a few only being triangular, and the surface of each base was convexly curved. The general length from the centre of the base to the apex was about a quarter of an inch; the longer diameter

of the base was nearly three-sixteenths of an inch, while the shorter basal diameter was about a sixteenth less. Of course, the above measurements are merely general, and were necessarily taken in a rather rough fashion.

The figures 1, 2, 3 below, although diagrammatic, will give a better idea of the usual shape of the hailstones than could be readily furnished by any further detailed description.

When sixteen hailstones, all of which were practically of the same size, were placed closely together side by side, so that all their apices terminated at the same point, a half spheroid was constructed, the curved basal surfaces of the pellets running neatly into one another to form the external globular surface. It

Fig. 1.—Magnified.

Fig. 2.—Magnified.

Fig. 3.—Basal view; magnified.

is evident, therefore, that those particular hailstones, at least, were originally portions of spheroids, each hailstone being a segment (the 1/32nd in this case) of a globe. How the icy globes were formed, and what was the cause of their breaking up into segments, are problems, I believe, that yet await serious investigation.

When the substance of any of the hailstones was observed closely by means of a lens magnifying four times, it was seen to be a transparent mass of ice, and fairly homogeneous in texture, having apparently little or none of the fibrous structure which has been previously noticed in other cases, and recorded by myself and other observers (see NATURE, vol. xxxv. pp. 413, 438,

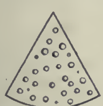


Fig. 4.—Showing numerous liquid cavities in a hailstone; magnified six times.

536). Each icy mass, however, contained scattered about in its substance numerous small cavities, round, oval, or elliptical in form, which were filled wholly or partially with water (see Fig. 4).

In some cases these liquid cavities were so numerous and so crowded together as to interfere seriously with the diaphaneity of the hailstone and to give it quite a clouded or granulated appearance. I may add that the convex basal surfaces of the hailstones were not quite smooth and glassy, but exhibited a rather grainy appearance, and were slightly but distinctly rough to the feel.

ALEXANDER JOHNSTONE.

Edinburgh University, November 29.

The Renaissance of British Mineralogy.

THE following passage occurs on p. 116 of NATURE of November 29, in an article on the above subject :—

"Crystallography should be taught as a special subject; and a knowledge of it should be required not only of the mineralogist but of the chemist, and even of the physicist. Hitherto, at least, the chemists of this country have been too content either to leave the crystalline forms of their artificial products undetermined, or to impose the task of their determination on the already sufficiently occupied mineralogist. It seems obvious, that in a satisfactory system of education every chemist should be taught how to measure and describe the crystalline characters of the products which it is his fate to call into existence. On various occasions expression has been given to this view, but the only chemist who has yet seen his way to act upon it is Prof. Henry Armstrong, who I am happy to say, has introduced the subject into the educational course of the City and Guilds Technical Institute. I trust that before another generation passes away his excellent example will be followed throughout the country. A knowledge of the elements of crystallography, including the mechanics of crystal measurement, ought to be made a *sine quâ non* for a degree in chemistry at every University."

The views thus expressed are shared by many chemists, and are, I believe, generally known. It is, however, not exactly correct that "the only chemist who has seen his way to act upon it is Prof. Henry Armstrong."

Long before the City and Guilds Institute was established or thought of, mineralogy, including crystallography, was a part of the curriculum in the Department of Engineering and Applied Science in King's College, London.

Until the year 1879, mineralogy and crystallography were studied in the Royal College of Science, Dublin, by students in the Faculty of Mining only, but at my suggestion these subjects were made compulsory on the students in all Faculties.

Furthermore, this subject was introduced into the course pursued in this College by the candidates for the Associateship of the Institute of Chemistry, the Council of the Institute accepting attendance at the course of mineralogy as equivalent to an equal number of hours in the chemical laboratory. This arrangement was carried out in 1881. It will be seen that mineralogy and crystallography are subjects which have by no means been neglected by chemists, though it is quite true that, like many other science subjects, they have not taken their proper places at the Universities. To chemists generally, there are without doubt more important subjects of study than crystallography which are not adequately taught; I refer to the use of the microscope, polarimeter, and spectroscope. These instruments are employed, and are absolutely indispensable, in certain methods of chemical analysis and research.

In how few laboratories are any accurate measurements of spectra made, or is the spectroscope used for any other observations than for detecting the alkalies and alkaline earths!

We have as yet no Professor of Spectrum Analysis, though it is undoubtedly a fit subject for a distinct Professorship, and this fact has, I believe, been recognized in Germany. If the advances in chemistry made respectively by students of crystallography and of spectroscopy be compared, it will appear that we owe very much more to the latter than to the former.

W. N. HARTLEY.

Royal College of Science, Dublin, December 1.

"Weather Charts and Storm Warnings."

I HAVE just been reading Mr. Allan Broun's review of Mr. Robert H. Scott's "Weather Charts and Storm Warnings," in NATURE, vol. xiv. p. 566, and note that the reviewer says :—

"Why, in all the disquisitions on fluid equilibrium, are the constant low (barometric) pressures in the Antarctic regions south of 60° neglected? How shall we account for the permanent depression in the neighbourhood of Iceland referred to by the author (p. 64)? And, to come to our own country, how will cyclonic winds explain the fact that the pressure of the atmosphere diminishes, on the average of the whole year, at the rate of one-tenth of an inch of mercury for 4° of latitude as we proceed northwards?"

I have no suggestion to offer respecting the depression near Iceland, but the other two—the depression about the South Pole, and the diminution of pressure going northwards in Great Britain—are parts of one general fact—namely, the diminution of pressure in going from about lat. 30° to either Pole, which, however, is most marked in the southern hemisphere. This, I think, admits of a simple explanation. The zones on each side of the equator are occupied by the trade-winds, blowing from the east: their cause is too well known to need statement here. But, by the law of reaction, they necessitate the existence of winds of equal total force from the west, and those west winds are formed in the regions between the trade-winds and the Poles. Winds blowing continuously round the Poles, in the same direction as the earth's rotation, constitute vortexes, and the pressure at the bottom of the vortex—that is to say, at the earth's surface—necessarily diminishes towards the centre—that is to say, towards each Pole. The diminution of pressure towards the Pole is much greater in the southern than in the northern hemisphere, because in the latter the unequal heating of continents and oceans produces currents of wind which, though on a large scale, are local currents in respect to the entire hemisphere, and tend to break up the vortex.

The cause I have assigned is a *vera causa*—that is to say, it is known to exist, and its effect must be of the nature of the effect actually found. Perhaps some of your mathematical correspondents will discuss the question whether it is of sufficient magnitude to account for the effect.

Belfast, December 2. JOSEPH JOHN MURPHY.

The Philippine "Tamarao."

In 1878 I reported in a letter to Dr. Sclater, the existence of a species of *Anoa* in the Island of Mindoro, on the strength of an example of the *Tamarao* labelled *Anoa depressicornis* in a Museum at Manilla. Having since seen living specimens of the Celebean *Anoa*, I have no hesitation in affirming that the latter animal has not even a superficial resemblance to the *Tamarao* which I saw at Manilla. I have now no doubt that the *Tamarao* of the Manilla Museum is a buffalo,—not, however, an immature example of the common buffalo, as has been suggested, but a distinct species, with short flattened horns sloping directly backwards.

A. H. EVERETT.

41 York Terrace, Regent's Park.

A Pheasant attacking a Gamekeeper.

As the keeper was walking home, a distance of half a mile, through the plantations near his cottage, a pheasant flew at him three times, attacking his legs in a most savage manner. The keeper got to his cottage with the pheasant after him, and called his wife out to witness the incident.

The keeper was able to secure the pheasant and return it to the cover. I should be glad to know if such conduct is exceptional on the part of game birds.

M. H. MAW.

Walk House, Barrow, Hull, November 30.

THE MORPHOLOGY OF BIRDS.[1]

I.

THIS magnificent work, consisting of two folio volumes, with more than 1700 pages of closely printed text, and illustrated by more than thirty artistically executed plates, is the latest of the "Bijdragen tot de Dierkunde" ("Contributions to the Knowledge of Animals"), published by the Royal Zoological Society, *Natura Artis Magistra*, of Amsterdam, on its fiftieth anniversary. It is the parting gift to that Society of its grateful author, who, one of Prof. Gegenbaur's ablest pupils, now fills the Chair of Anatomy in the University of Jena; and it is needless to say that the publication of so monumental a work reflects the highest credit upon the Society of Amsterdam. It is monumental not merely from its bulk, but chiefly from the enormous amount of information it contains, much of it bearing upon some of the most deeply-rooted questions of importance to the general morphologist, and above all on the natural affinities—that is to say, the phylogeny—of birds both living and extinct.

In what follows I make no attempt at a critical review, but give as complete a summary as possible of Prof. Fuerbringer's work, which I trust will be acceptable to English readers, for few of them will have the opportunity of seeing these costly and weighty volumes, or the perseverance to master their contents, and yet it cannot be but that many would like to know the results at which the author's investigations have led him to arrive.

The whole work consists of two parts.

The special part comprises the first 837 pages, and is devoted to a minute and most comprehensive description of the bones, nerves, and muscles of the avian shoulder-girdle in the widest sense. The investigations extend over many hundreds of birds of all orders and families; frequently numerous specimens of the same species have been examined in order to ascertain the extent of individual variability.

The author justly asks himself if it is not too much to offer such a bewildering mass of mere detail to the public; but he considers it indispensable that the reader, who may not easily yield acceptance to the generalizations, should be offered the fullest opportunity to re-examine the facts in detail, and to follow step by step the road which has led the author to his conclusions.

[1] "Untersuchungen zur Morphologie und Systematik der Voegel, zugleich ein Beitrag zur Anatomie der Stuetz- und Bewegungsorgane." Von Max Fuerbringer, Professor der Anatomie, und Direktor des anatomischen Instituts und des Museum Vrolik der Universitaet zu Amsterdam. Mit 30 Tafeln. (Amsterdam: T. van Holkema, 1888.)

At the same time, it must be borne in mind that the first part of the work is not merely descriptive, but that it contains a series of complete essays on the morphology of the organs under consideration. The treatment of the structure, development, and modifications of the sternum, for instance, takes up not less than 78 pages. In the myological part particular attention has been bestowed upon the proper naming and homologizing of the muscles.

The descriptive detail deposited in the special part has been used in the *second or generalizing part* as the material for reflections. These lead (*a*) to morphological results, which are important chiefly for the phylogenesis of the skeletal, nervous, and muscular systems; (*b*) they form a basis for a new systematic arrangement of birds. Physiological questions are less dwelt upon, but there are numerous contemplations on the theory of flight, and a remarkable chapter on cold- and warm-bloodedness.

The author remarks that the study of the morphology of birds well repays the labour bestowed upon it, not so much because of the great or fundamental variety which this class of vertebrates exhibits, but rather because several organic systems have reached a height of development which they have not attained in any other class of animals. We often find a richness of organic differentiations within the limits of small groups of birds. It is therefore possible to form a judgment, approaching almost to certainty, as to the primary or secondary significance of these differences. It is interesting to follow the steps which lead to such astonishing heights of specialization.

Pp. 839–996 are devoted to results and reflections of general morphological importance. For instance, the changes in the configuration of the sternum which are brought about by the modifications of the muscles of the pectoral girdle. There is not unfrequently an apparent discord between the passive or skeletal and the active or muscular elements; of these the latter are by far the more progressive, so that the more conservative skeletal parts have not always kept step with the newly introduced changes of the muscles. An example of this is afforded by the wings. By the reduction of the wing, beginning at the distal end, those muscles are first affected which arise from the wing bones, next are affected the bones themselves, and lastly those muscles which are inserted on the same (p. 855).

Syndesmology receives much attention, chiefly by an extensive treatment of the shoulder-joint. Joints are certainly not formed by the action of the muscles during embryonic life, but they are phylogenetically preformed, and only during the post-embryonic stages can the finer configuration of the joints be modelled and influenced by the muscles.

P. 862.—Questions of the greatest importance are involved in the transformation of mere ligamentous connections into symphyses and joints, with the accompanying neoblastic appearance of cartilage. This new cartilage is either homoblastic or heteroblastic. It arises from latent cartilaginous cells, as is the case with the addition of new vertebræ at the end of the Ophidian tail, and probably with the multiplication of the Cetacean phalanges; or the cartilage is due to transformation of periosteal cells, like the patella ulnaris. In such cases the original ligament can be supplanted by bone. On the other hand, the clavicle is sometimes transformed into a ligamentum claviculare.

Fasciæ are often strengthened into aponeuroses and into tendons; they are used as such, not only by their own muscle, but also by neighbouring ones, and this leads to the formation of paratenons or tendinous slips. Birds afford numerous instances in which muscles have gained extra support by "anchoring" themselves to neighbouring fasciæ.

Pp. 877–82 treat of *sesamoid* bodies, of which the author recognizes three sorts. (1) Skeletogenous sesamoids, like the pisiform bone, are, strictly speaking, not

sesamoids, as they are retrograded skeletal parts, which in most cases have been preserved by the surrounding muscles. (2) Arthrogenous sesamoids, like the os humeroscapulare, are derived from the capsule of a joint. (3) Tenontogenous or desmogenous, like the patella, are formed heteroblastically inside of a tendon.

The eighth and ninth chapters (pp. 894–947), form a critical essay on muscles with regard to their connection with the nervous system. After having exhaustively criticized the neuro-muscle theory of Kleinenberg, the various views of Huxley, those of the brothers Hertwig with reference to the cœlom theory, and, lastly, the theory of the secondary connection of muscle- and nerve-fibres as promulgated by Claus and Chun, the author considers the ontogeny, degeneration, and regeneration of muscles and nerves. Lastly, he proceeds to attempt a decision (pp. 920–41).

In connection with this attempt stands a discussion of the inheritance of acquired faculties, and the continuity of germinal and somatic plasma. Fuerbringer believes in Haeckel's law of accumulative adaptation through inheritance. What the individual has acquired during and through its incessant contact with the world can greatly influence its descendants; hence the great importance of the investigation of post-embryonic developmental changes. Throughout the whole book, Fuerbringer, without denying the importance of the ontogenetic features as a recapitulation of the ancestral history, lays more stress upon the study and comparison of the adult forms. In almost every chapter, we come across instances in which the embryonic development does not help to explain certain organs; the recapitulation of their previous stages is too much hurried or condensed, and at the best only that is repeated which had last been acquired.

Fuerbringer accepts Kleinenberg's neuro-muscle theory as the most probable solution. The whole apparatus, which consists of a ganglionic cell, a nervous and a muscular fibre, has been developed from one and the same cell, and is therefore to be looked upon as one organ. The muscle is the end-organ of its nerve, consequently the innervation of the muscles forms the most trustworthy means for the determination of their homologies.

Chapter x. (pp. 947–72) deals with the variability of muscles. Neither the point of the origin, nor that of the insertion, of muscles is a safe guide to their homologies. This shows why muscles are almost valueless for the determination of the homologies of skeletal parts.

Pp. 972–91, on the shifting or migration of the extremities with their girdles along the vertebral axis. This shifting has reached its highest degree in birds. Even individual and one-sided variations are frequent. As a rule, the shifting has been directed backwards, resulting in an increase of the length of the neck. Large birds show a greater amount of shifting than the smaller ones of the same family. A retrograde or secondary shifting towards the head seems to stand in correlation with the degradation of the wings. Hand in hand with the changes of the relative position of the limb and girdle goes a change of the whole thorax. Thoracic vertebræ are turned into cervical, and lumbar into thoracic vertebræ. In most cases, but not always, the number of thoracic ribs remains the same. It looks as if, roughly speaking, the whole trunk with all the organs inclosed in it, did slide along the vertebral axis. The accompanying metamerical transformation of the plexus brachialis is not effected by inter- or excalation of nervous segments, but by the diminution and reduction of one anterior nerve-stem, and the contemporary formation and addition of a nerve nearer to the posterior end of the plexus. The peripheral parts of the plexus retain their configuration in spite of all the changes, and since the only trustworthy safeguard in the homologies of spinal nerves is their number in the series of metameres, two plexuses may be homodynamous, although, strictly speaking, not homologous. This is expressed by the term "imitatory homodynamy," more happily by "parhomology."

The same considerations apply to the muscles. They, together with the nerves, undergo metameric changes until they likewise are only parhomologous. The various muscles of the shoulder-girdle of a bird with thirteen cervical vertebræ may present, in shape, position, and distribution of the nerves, features identical with those of a bird with fifteen cervical vertebræ, but still they are only parhomologous to each other.

This metameric transformation cannot, of course, be watched on those muscles which arise from the shoulder-girdle, but on those which, like the mm. rhomboidales et serrati, arise from the trunk, and are inserted into the girdle. The migration of the whole anterior extremity tailwards necessitates first elongation, then a thinning out, and even total reduction, of those muscles which extended from the neck to the anterior end of the shoulder-girdle. In this way the m. levator scapulæ of the reptiles has become lost by the birds.

Of course the whole problem of the metameric transformation and new formation of muscles and nerves cannot be considered as solved without an explanation of the histological changes which are involved in the question of the parhomology of the muscles together with their nerves. Such an explanation Fuerbringer has not been able to present, but he tries to suggest one by showing how we may imagine those changes to take place.

Most muscles, Fuerbringer argues, are polymetameric, *i.e.* they receive nervous fibres from two or more spinal roots. Moreover, the nerves of the more proximal muscles belong chiefly to the pre-axial or anterior, whilst those of the distal or more peripheral muscles receive their nerves mostly from the post-axial roots of the plexus. The author discards the idea that nerve-fibres can send out buddings into neighbouring new muscles, but thinks that in many cases the formation of new muscles and nerve-fibres is initiated by a splitting. This splitting begins peripherally with the muscle-fibre, is followed by that of the nerve-fibre, and perhaps leads to a division of the ganglionic cell. Ganglionic cells with two axial cylinders of motory nerves are known to occur.

Another possible explanation of the increase of the number of fibres of one nerve and those of one muscle is their derivation from cells which had remained latent in an embryonic or primordial condition between the fully formed muscle- and nerve-cells. Traces of such primordial elements Fuerbringer has found between the fibres of motory nerves, and between the fibres of fully developed muscles; in the latter case they may be identical with the myoblasts of other authors.

Everywhere in nature, in the organism, there is superfluity of material. Tissues and organs seem to be trained by the struggle for existence in such a way that they produce at their beginning an abundance of formative germs and cells, of which under ordinary circumstances only a small part becomes developed into specifically functional tissue-cells. The rest remain in their primitive embryonic condition. They form stored-up plastic material, which may or may not be called upon to meet such extraordinary requirements as may arise from the necessity for the organism to adapt itself to new conditions.

Still greater is the difficulty when the neomorphism (by which word the reviewer has on a previous occasion tried to render into English the meaning of the German term "*Neubildung*,"—new formation not being exactly identical with it) is not confined to the same, but takes place in the next following metamere. For instance, when a muscle, which previously was innervated by the fibres from the 15th and 16th spinal nerves, now receives its supply from the 15th, 16th, and 17th nerves, the explanation given above will be of no avail. The permanent continuity of the two later components of the original neuro-muscle

cells forbids the assumption of a secondary junction; in our case the junction of fibres of the 17th spinal nerve with those fibres of the old muscle which, by migration or by increase in number, have come to be situated on the 17th metamere.

Fuerbringer explains these changes by an ingenious hypothesis, viz. by a peculiar mode of growth, which he terms "growth by metameric opposition or reduction of the muscles and nerves." The primordial extremity was perhaps somewhat broader, and extended over a greater number of metameres. This surplus of locomotory elements was reduced by the following concentration of the limb, but not so completely that those metameres did not in later generations retain *vi inertiæ* the faculty of reproducing some of the nearly lost elements in a primitive condition. The reduction of these germs, that are not called upon, would become complete, they would be lost, but for the particular stimulus which they and their metameres receive from the extremity. The real nature of the interdependence between a stimulus and its effect is still an obscure problem; but we can imagine that the shifting limb exercised a stimulating influence upon the newly overlaid metameres, and that this stimulus awakened the latent nerve-muscle germs, which then joined the already existing apparatus of the approaching limb. Such latent germs, when once started into activity, may well be required to fully develop in order to make up for the reduction of the motory elements at the opposite end of the limb. As a support for this speculation, Fuerbringer alludes to the fact that the anterior limb of birds now tends to shift backwards, and it is the last root of the brachial plexus which contains relatively the greatest number of those immeasurably fine elements that he is inclined to consider as latent germs. The absolutely gradual mode of growth, which this metameric apparition postulates, makes it less perplexing that parts which are only parhomologous should be the very counterfeits of each other.

Pp. 984–91. What has caused the backward shifting of the anterior extremity? To inquire into the causation of the length of the neck means the same problem in a different form. A correlation between the length of the neck and that of the legs is not always there, *e.g.* swans. The assumption of intercalation of vertebræ is still unjustifiable, being quite unsupported by proofs. There remain lengthening of the single vertebræ and the shifting of the extremity along the axis.

In stretching our neck, we bring into play chiefly two sets of muscles, viz. the extensors of the neck and the depressors of the shoulder and anterior limb. There is no reason for assuming that the ancestors of recent birds did not effect the stretching of their necks in the same way. Continued habit results in permanent conditions. In a well-extended neck the vertebræ are in more perfect equilibrium and in conditions more favourable to their nutrition and growth than if the neck were much curved and doubled up. The pectoral girdle, the sternum, and the whole anterior limb of birds are *in toto* retracted by the incessant pulling of the mm. rhomboides, serrati, latissimus dorsi, and the abdominal muscles, the latter so far as they are attached to the sternum. Many of the thoracic muscles show a pronounced tendency to extend their origin tailwards. As a rule, large birds possess more cervical vertebræ than smaller birds, and they are noteworthy for their soaring and more lasting mode of flight. During the time that the wings are not moved, but are kept in a spread-out position, they offer a greater resistance to the air than the far smaller but heavier bulk of the body. The momentum gained by the body will therefore tend to move the latter forwards with more velocity than the resisting wings. In other words, the wings will remain behind the body, and the strain produced by this difference in equilibrium will act upon the ribs, since these form the weakest connection of the sternum + shoulder-girdle + wing with the rest of the body. The anterior thoracic ribs will lose their sterno-costal character, and be transformed into cervical ribs, *i.e.* the neck is lengthened, and the whole pectoral girdle, with the whole apparatus of flight, will be shifted backwards. The reduction of thoracic into cervical ribs can be proved on grounds independent of this question. The long necks of the Ratite birds seem to offer a serious objection to the view just explained, but Fuerbringer pleads in another part of his work (p. 1504) for their being descendants of birds which possessed well-developed power of flight.

Pp. 991–95 contain some remarkable observations about the size of birds. On the whole, small birds show more primitive and simpler conditions of structure, whilst the larger members of the same group exhibit a more one-sided development, and consequently greater deviation from the common stock. The first birds were probably smaller than Archæopteryx. Reptiles and mammals show likewise in their earlier and smaller types more primitive features than do their larger descendants. It is therefore the study of the smaller members within given groups of animals which promises the best results as to their phylogeny.

H. GADOW.

(To be continued.)

STATISTICS OF THE BRITISH ASSOCIATION.

IT may prove instructive to see, in diagram-form, some statistics connected with the history of the British Association. We go back only to 1850 (the Association, it is known, dates from 1831), and our curves relate to the total attendance in each year to the present, to attendance of ladies, and to grants in aid of scientific research.

As regards attendance, it will be seen that the maximum is that of the Manchester meeting in 1887, the figure 3838 having been then reached. Newcastle, in 1863, comes next, with 3335; then Manchester again, in 1861, with 3133. The curve reaches high points also in the case of Liverpool in 1870, Glasgow in 1876, Southport in 1883, &c.

On the other hand, we find the curve reaching its lowest, in this period, at Ipswich, in 1851, with 710; while Hull, in 1853, Swansea in 1880, Cheltenham in 1856, Cambridge in 1862, and Plymouth in 1877, furnish other low points, in rising series.

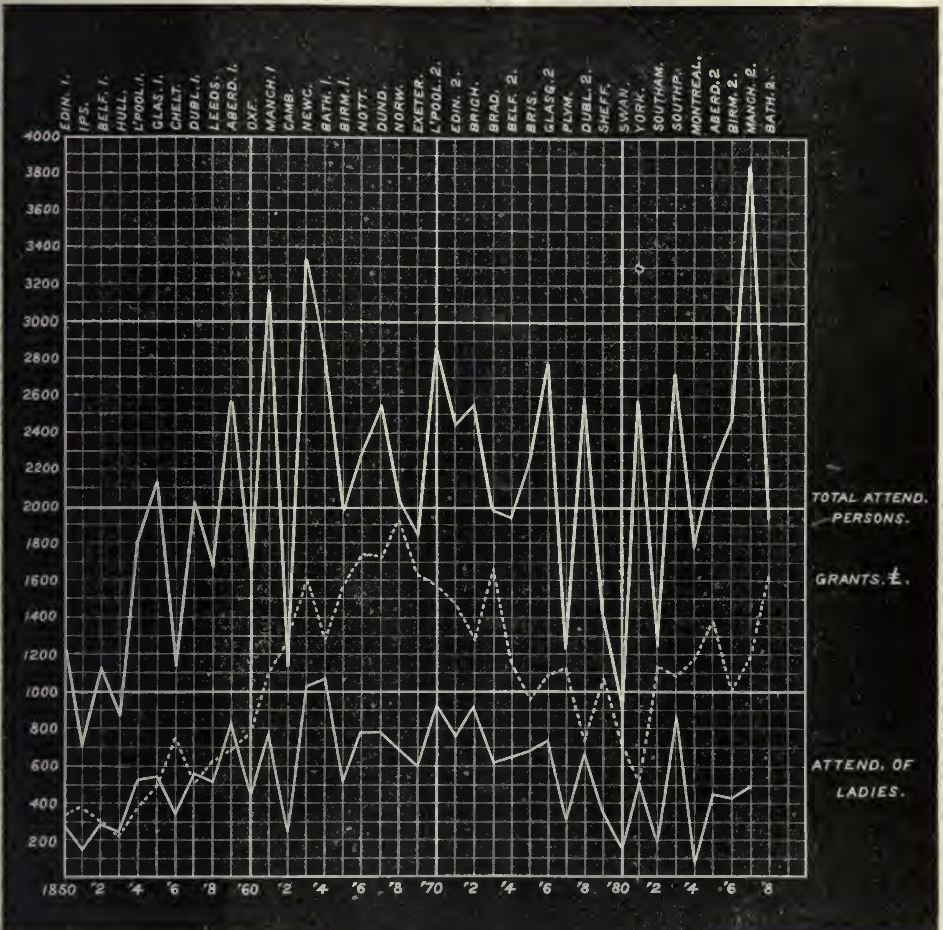
The general course of the curve seems to be that of rise to a maximum at Newcastle in 1863, then descent to Swansea in 1880, followed by another rise to the peak representing Manchester in 1887. The meeting at Montreal (in 1884) it will be seen, takes a fairly good position as to numbers (1777).

Several places had a second visit *in the period considered* (not necessarily second in the whole series); these are indicated by figures after the abbreviated names. How do those repeated visits compare with the earlier? In general, the second visit considerably exceeds the first in numbers. But in the case of Aberdeen the second figure (2203) is less than the first (2564); and the same holds for Bath, 1950 this year, as against 2802 in 1864. The former Bath meeting had no doubt exceptional attractions in the visit of Livingstone, &c. The earlier Aberdeen meeting was presided over by the Prince Consort.

Of the total number, something like one-half are usually associates for the year (paying £1), the others annual or life members, ladies, and foreigners. These groups do not call for special remark here; but we have given a curve of ladies' attendance, as there is room for it, and it is somewhat curious. It seems to have a

slightly downward tendency from the maximum of 1058 at Bath in 1864. Whether this has any significance as revealing change of character in the Association (deterioration or otherwise) we cannot say. Subjects of discussion within the range of average feminine comprehension and interest, have not, we think, been growing in scarcity of late, but rather the reverse.

We might note the much smaller attendance of ladies at Manchester in 1887 (viz. 493) than in 1861 (viz. 791), though the general attendance was much larger in the later year. Again, more ladies attend the meetings in a place like Bath than in one like Newcastle, while the total attendance is greater at the latter: this is only natural. In Brighton, as compared with Liverpool, similarly, there is a larger proportional attendance of ladies. And further discrepancies between the two



curves will be found on examination. The minimum attendance of ladies was at Montreal in 1884, viz. 74.

In the dotted curve showing grants of money for research, we find a rapid rise to a maximum in 1868, when £1940 was paid; then a rapid fall to £476 in 1881; after which the curve mounts again to £1645 this year. In 1872 the grant to Kew Observatory fell from £600 to £300, and the following year it disappears.

A curve of the yearly income corresponds, in the main, with that of the attendance.

The history of an institution, clearly comprehended, should furnish hints as to how the welfare of the latter may best be promoted in the future. Should the teaching of our diagram in this relation be educed through discussion in the columns of NATURE, it may serve a good purpose.

THE MOVEMENTS OF CYCLONIC AREAS.[1]

THE Meteorological Council having undertaken a special discussion of the weather over the North Atlantic and adjacent continents for the thirteen months ending with August 1883, the period agreed upon for the international circumpolar observations, the preparation of the synoptic charts for the same area originally commenced by Captain Hoffmeyer, and since his death carried on conjointly by the Institute at Copenhagen and the Seewarte at Hamburg, was suspended for the time, and a new series was started from September 1, 1883. The charts are issued in quarterly volumes, and the "Vierteljahrs-Wetter-Rundschau" is a carefully-prepared summary of the principal meteorological features in each volume, to which is added a critical examination of the routes followed by sailing-ships from Europe to and from America, and to and from the equator, with tables showing the time occupied by each ship in sailing between certain latitudes and longitudes. Passage tables have always been a favourite study with mariners. Here there are hundreds of voyages tabulated in order of date, so that the sailing qualities of several ships are comparable from being under the same conditions of weather, and the value of the tables is therefore greatly enhanced.

The novel point in the work, however, is the introduction by Dr. Köppen of a new system of discussing the paths of storms. For some years past, meteorologists have been slowly coming round to the opinion that anticyclonic areas exercise a very important influence on the movements of cyclones; indeed, it may be safely asserted that the latter are almost entirely dependent upon the former for every stage of their progress. But although this is daily becoming more manifest, little has been done in the way of improving our mode of tracking cyclones. Ordinary track-charts represent the paths of storms during fixed periods of a calendar month, as if those of the early days were comparable with those of the end of the month—that the tracks, in fact, were regulated by time rather than by changing circumstances. A month's storm-tracks are so numerous, the direction of translation so varied, disturbances travelling in every conceivable direction—north, south, east, and west—and twisting round and round within a small radius, that to trace an individual disturbance is a work of no small difficulty. Even when the tangle is unravelled, there is nothing on the chart to show why one storm travels direct across the Atlantic to Europe in two or three days, another takes ten or eleven days, while others dart off through Davis Strait or by the east coast of Greenland to the Arctic regions; and the consequence is that we are left entirely to theorizing to explain the different movements, and theory has generally sought for the cause within the cyclones themselves. The more we examine synchronous weather-charts the more patent is it that cyclonic areas are strictly limited in their movements, both in rate and direction, by the surrounding conditions.

In NATURE, vol. xxxiii. p. 206, a diagram is given showing the track of a low-pressure system and its influencing anticyclone, the position of the lowest and of the highest barometer being indicated for each day. In the volume for 1880 of "Aus dem Archiv der Seewarte," the daily positions of the cyclonic centres for the first three months of 1878 are given, but the movement of maximum pressure is shown by a simple line without any indication of the position every twenty-four hours. Dr. Köppen has considerably improved on these plans. He has before him the daily charts for several weeks, and finds that for a number of consecutive days there is a general resemblance in the distribution of high and low pressure areas, the former almost stationary, the latter travelling along on the edges of the anticyclones. He selects the ruling type for the whole area of the charts, and then proceeds to represent the period of the type on one chart, the number of such periods in the twelve months ending with August 1884 being fifty-seven, ranging from three to eleven days each. The movements of the cyclones during the type are represented by lines joining the ascertained positions each day, and by a simple arrangement the lowest pressure and the force of the wind are represented daily. The anticyclones are treated differently, they are considered as practically stationary, and the isobar of 765 mm. (30·12 inches) being found on the charts every day, it has been adopted as the representative of the high-pressure systems. This isobar for the several days of the type produces a mean line which shows the average position of the anticyclone during the period. The maximum barometric values are shown near the centre, and also the direction in which the highest pressures moved, as in the volume already alluded to.

An examination of the results as shown by this method indicates that the question of storm tracking has been greatly simplified. Instead of twelve monthly charts full of tangled curves exhausting our patience to unravel them, we have about five for each month, the paths of disturbances during the prevalence of a type not varying greatly, and individual cases are followed without difficulty. The question of expense in lithographing the charts has apparently compelled Dr. Köppen to represent two, and even three, types on one chart, which tends to confusion; and if more of this kind of work is to be done, it will be well to strain a point to give each period by itself, so as not to perplex those who only occasionally deal with such charts. There are, however, several single periods to which a chart has been devoted, and a study of these only will suffice to convince us that an advance in the right direction has been made. Take the sixth and seventh charts representing the conditions between October 26 and November 1, and November 2 to 10, 1883, and the importance of the work is at once apparent. In the former the permanent anticyclone over the ocean is well south, while the European anticyclone covers all but the most northern parts of Scandinavia and Russia. The cyclones follow a well-marked path from America to Iceland and the White Sea. In the second case the Atlantic anticyclone is further north, and the European area has moved away to the eastward, and in keeping with these changes, the disturbances have also modified their directions. Speaking very generally, storm areas run parallel with the edge of the anticyclone. According to the position of the latter so do the cyclones advance, recede, or stand still. We see that land does not seem to offer any resistance to the advance of a storm area, the changes of direction in mid-ocean being quite as frequent as near the land. Tropical storms may either march due north, curve round by the American coast, or proceed to the western side of the Gulf of Mexico, and then cross the American continent to the Atlantic; but in each case we find the position of the anticyclone to govern the course followed. From a similar cause the rate of movement is also affected, varying quite as much on the level surface of the sea as in the midst of mountainous continents. In the chart for March 30 to April 11, 1884, the anticyclones over Europe, the Atlantic, Greenland, and America form a barrier allowing no means of escape for the disturbance which is seen to wind about from Davis Strait to the coast of Ireland and back again to the neighbourhood of its starting-point. The question of direction and rate, therefore, depends largely upon the position and stability of the anticyclones; and if we can in any way discover the resisting power of the latter we should no doubt greatly increase our ability to forecast changes. For the purposes of storm-warnings, it seems to be quite as necessary to know the conditions over Eastern as over Western

[1] "Vierteljahrs-Wetter-Rundschau an der Hand der täglichen synoptischen Wetterkarten für den Nordatlantischen Ocean des Dänischen Meteorologischen Instituts und der Deutschen Seewarte." Parts 1 to 5, September 1883 to November 1884, with 41 Charts. (Berlin: Mittler und Sohn, 1887-88.)

Europe, because it is on the existence or non-existence of anticyclonic areas over Russia that bad weather may either steer clear of our coasts, or pass clean over us and cause enormous damage to life and property.

Dr. Köppen's adaptation of composite portraiture for anticyclones will, when fully developed, doubtless lead to the discovery of principles which must be of considerable practical importance in our every-day life, and meteorologists should direct their attention to the discovery of some law which will indicate the approach of a change in the distribution of high barometer readings. At present we know absolutely nothing of what is taking place over the Atlantic out of sight of our own coasts, and the ordinary weather-charts cover such a small extent of North-Western Europe that they do not give us a fair idea of what the conditions are to the eastward. As the facts indicated become more fully known, we shall probably see an extension of the Weather Report area, and a corresponding improvement in forecasting.

The "Vierteljahrs-Wetter-Rundschau" appeared originally in various numbers of the *Annalen der Hydrographie und Maritimen Meteorologie*, the official publication of the Deutsche Seewarte, and are now reprinted in separate form.

NOTES.

THE members of the "Provisional Committee" appointed at the International Geological Congress in London, with reference to preparations for the next meeting of the Congress at Philadelphia, met at New Haven on Thursday, November 15. All were present except Dr. T. Sterry Hunt, who was confined to New York by illness. By vote, twenty-four members of the Permanent or Organizing Committee were appointed, as follows:—C. A. Ashburner, J. C. Branner, T. C. Chamberlin, G. H. Cook, J. D. Dana, W. M. Davis, C. E. Dutton, G. K. Gilbert, James Hall, A. Heilprin, C. H. Hitchcock, Joseph LeConte, Dr. J. Leidy, J. P. Lesley, O. C. Marsh, J. S. Newberry, J. W. Powell, J. R. Procter, N. S. Shaler, J. J. Stevenson, C. D. Walcott, R. P. Whitfield, H. S. Williams, Alexander Winchell. The Committee has power to add to its number. Dr. J. S. Newberry was appointed temporary Chairman. With this action, the duties of the Provisional Committee ended. The first meeting of the Permanent Committee will be held in Washington in the month of April. One of the publications presented to the recent session of the Congress in London was a voluminous Report on some important questions in American stratigraphical geology. We learn from an article in the December number of the *American Journal of Science*, by Prof. Dana, that, in his opinion, the views of the great majority of American geologists are not fairly represented in that Report. The Committee now appointed will certainly be regarded by geologists in Europe as a thoroughly representative one, which will, no doubt, take good care that the general body of geological opinion in the United States shall be adequately put before the world at the Philadelphia meeting.

ON Tuesday the completion of the ninth edition of the "Encyclopædia Britannica" was celebrated by a dinner given by Dr. W. Robertson Smith, the editor, in the hall of Christ's College, Cambridge. Upwards of a hundred contributors were present, and among them were many eminent men of science. Responding for "Science," Dr. Archibald Geikie said the old limits within which culture was confined had proved to be altogether too small for the progress of the present day. The soldiers of the republic of science had sometimes been accused of a strong desire to attack University culture and carry it by storm. For his part he wished that it might stand, but that no barrier should be interposed against the freest communion between the people inside and the newer and wider city around their borders.

AN Anthropological Congress is to be held at Vienna during August next.

AN important paper by Prof. Virchow, on "Land and People in Ancient and Modern Egypt," appears in the current number of the Transactions of the Berlin Gesellschaft für Erdkunde. It embodies some of the results of Prof. Virchow's ethnological researches during his recent visit to Egypt. It has hitherto been thought that the fellaheen of to-day are of exactly the same physical type as that of the most ancient known Egyptian population. Prof. Virchow, however, holds that the evidence afforded by the oldest sculpture, as well as by the skulls of the earliest period, tends to show that the primitive type in Egypt was brachycephalic, whereas the types which exist at the present time, and have existed for ages, are dolichocephalic and mesocephalic. Whether the change was produced by the influence of environment, or by the influx of new races, cannot, according to Prof. Virchow, be definitely determined by the evidence at present at our disposal; but, of the two views, the latter, he thinks, is the more probable.

THE inaugural lecture delivered before the University of Glasgow by Prof. Max Müller, as Lord Gifford's Lecturer in Natural Theology, has just been published by Messrs. Longmans, Green, and Co. In this lecture, Prof. Müller presents a most interesting account of the ideas which have regulated the work of his life. His conception of "the science of religion" will serve as the test, and, he hopes, the confirmation, of his theories relating to language, mythology, and thought.

THE New England Meteorological Society proposes to have a Loan Exhibition of Meteorological Apparatus, Photographs, &c., at the Institute of Technology, Boston, in connection with its fourteenth regular meeting in January 1889. The Society has issued a request for contributions.

THE Report of the Norwegian party of the International Polar Investigation contains the results of the observations which were made according to the programme decided upon at the Polar Conference at St. Petersburg, in August 1881. The observations were made at Bossekop in Alten, in lat. 69° 58′ N. and long. 23° 15′ E. of Greenwich. The first volume, issued in 1887, contains the astronomical and meteorological observations, and the second volume (Christiania, 1888) gives the results of the magnetic determinations and observations of auroræ. In the meteorological section, tables are given of the hourly readings of the temperature and humidity of the air, height of barometer, direction of winds and nature of clouds, extending from August 1882 to August 1883 inclusive, and concluding with monthly averages. In vol. ii. are given the records of the determinations of the magnetic declination and horizontal and vertical intensity. These were made fortnightly during the year, at intervals of five minutes during the day, so that the carefully-executed curves expressing the results of the observations are very detailed and convenient for comparison with the complete descriptions of auroral displays, the records of which are also contained in this volume. No doubt these results will be of great service in correlating auroræ with magnetic disturbances.

THE Royal Meteorological Society has published its "Meteorological Record" for the first quarter of this year, containing the monthly results of observations made at its stations, with remarks on the weather by Mr. W. Marriott. The Society commenced the organization of stations on a uniform plan in 1874, and these were supplemented by another class of stations, termed climatological, in 1880. Since 1881 the results have been published in a separate form under the above title. A map of the stations is issued annually, and shows that they are fairly well distributed, except in Wales. In addition to the monthly results, tables of daily rainfall are given for a number of stations, and of the

daily temperature and sunshine in London and the suburbs. The monthly values published by the Registrar-General are also appended, and the whole forms a valuable record of the meteorological statistics of England and Wales, issued well up to date.

AT the meeting of the French Meteorological Society on November 6, M. Lemoine presented a summary of the rainfall observations of the basin of the Seine in 1887. He stated that the rainfall was everywhere below the average; in the Department of the Seine-Inférieure the totals for the year were the lowest in a series of twenty-one years. M. Renou stated that the late M. Hervé-Mangon having expressed the wish that his observations made at Ste.-Marie-du-Mont should be published, Mme. Mangon had handed them over to him for publication at her expense. M. Renou presented a note on the temperature of October at Paris since 1757. He pointed out that during the last 130 years the month of October presented either a low or a high mean every twenty or twenty-five years. Means as low as that for October 1887, viz. 44°·1, were very rare. Since 1757 the lowest averages for October had occurred in 1784 (45°·3), and 1817 (45°·1).

BIOLOGISTS will be glad to learn that the posthumous works of the late M. Severtsoff are being issued by M. Menzbier, at Moscow, and that a new part has been added this year to the part which appeared in 1886. Severtsoff not only was a first-rate zoologist and explorer of unbeaten tracts in Turkestan; he was also a powerful thinker, and everything he wrote about the philosophy of zoology deserves attention. Unhappily, his frequent expeditions to Central Asia rendered it impossible for him to bring to an end the works of a general character which he was preparing. He had begun an "Ornithology of Turkestan," based upon his exceedingly rich collection of birds, which contains no less than from 12,000 to 13,000 specimens. This work will be completed by M. Menzbier, and it could not have been put into better hands. As for the two parts of his "Posthumous Works" (*Mémoires de la Soc. des Natur. de Moscou*, vol. xv. fascs. 3 and 5), they consist of two papers: one in German, on two insufficiently known species of Russian hunting hawks (*Hierofalco grebnitzky*, n. sp., and *Hierofalco uralensis*, Sev. et Menzbier); and the other, in French, on variations due to age of the Palæarctic *Aquilinæ*, and the taxonomic importance of those variations. The former contains, besides the description of the two species (with coloured plates), a sketch of the geographical distribution of the Icelandian, Norwegian, Uralian, and Labradorian species of *Hierofalco*. The second paper contains, first, a discussion of some principles of zoological classification, being an answer to Dr. Seebohm's reproach of having "too closely followed the steps of the elder Brehm." and of having aimed at "hitting the happy medium between 'lumpers' and 'splitters.'" This is followed by a discussion of the natural extent of the family of *Aquilinæ* (which, according to Severtsoff, must include only the three genera, *Aquila*, *Haliaëtos*, and *Milvus*); the geographical distribution of all known species of the *Aquilinæ*; and finally, the description of those species which Severtsoff had himself studied closely in the field,—special attention being given to the variations of coloration dependent upon the age of the individuals Seven beautifully coloured plates accompany the work.

WE have received the twelfth volume of the systematic Catalogue of the Museum of Natural History of the Netherlands. The present volume deals with Mammifers, and has been compiled by F. A. Jentink. No fewer than 5379 individuals, representing 900 species of Mammifers, are enumerated in this Catalogue. M. Jentink hopes that his work may be of service to zoologists by enabling them to see what rare or interesting species are to be found in the National Museum, the value of which, in this particular branch, has not hitherto, apparently, been sufficiently appreciated.

A PAPER on "Energy and Vision," by Prof. S. P. Langley, was read by abstract before the American National Academy of Sciences on April 19 last. This paper was printed in the November number of the *American Journal of Science*, and has now been separately issued. The writer does not profess any competence in physiological optics, and points out that his observations, and the conclusions reached from them, are both to be understood from the purely physical point of view. This being premised, he summarizes the paper in the following conclusions:—"The time required for the distinct perception of an excessively faint light is about one half-second. A relatively very long time is, however, needed for the recovery of sensitiveness after exposure to a bright light, and the time demanded for this restoration of complete visual power appears to be the greatest when the light to be perceived is of a violet colour. *The visual effect produced by any given, constant amount of energy varies enormously according to the colour of the light in question.* It varies considerably between eyes which may ordinarily be called normal ones, but an average gives the following proportionate result for seven points in the normal spectrum, whose wave-lengths correspond approximately with those of the ordinary colour divisions, where unity is the amount of energy (about $\frac{1}{1000}$ erg) required to make us see light in the crimson of the spectrum near A, and where the six preceding wave-lengths given correspond approximately to the six colours—violet, blue, green, yellow, orange, red.

Colour.	Violet.	Blue.	Green.	Yellow.	Orange.	Red.	Crims.
	μ	μ	μ	μ	μ	μ	μ
Wave-length	·40	·47	·53	·58	·60	·65	·75
Luminosity (visual effect).	1,600	62,000	100,000	28,000	14,000	1200	1

Since we can recognize colour still deeper than this crimson, it appears that the same amount of energy may produce *at least* 100,000 times the visual effect in one colour of the spectrum that it does in another, and that the *vis viva* of the waves whose length is $0·75^{\mu}$, arrested by the ordinary retina, represents work done in giving rise to the sensation of crimson light of 0·000000000000003 horse power, or about 0·001 of an erg, while the sensation of green can be produced by 0·00000001 of an erg."

A NEW and highly interesting method of obtaining gaseous carbon oxysulphide, COS, perfectly pure and in large quantities, has been discovered by M. Armand Gautier. The methods of preparing this gas already known are somewhat difficult to carry out, and the only effectual means of purifying it from the persistent presence of carbon disulphide vapour with which we have been hitherto acquainted, is by utilizing the fact that carbon disulphide is absorbed by triethyl phosphine, a most costly reagent. M. Gautier's new method is extremely simple. A large porcelain tube is partially filled with calcined kaolin, the purest variety of natural silicate of alumina, and heated to whiteness in a good furnace. The air having been first expelled by means of a current of carbon dioxide, a gentle stream of dry carbon disulphide vapour is allowed to slowly pass through the tube. The mixture of gases which issues from the tube is found to consist of a little over 60 per cent. of carbon oxysulphide, and about 35 per cent. of carbonic oxide, together with traces of carbon dioxide and sulphuretted hydrogen, and a slight excess of vapour of carbon disulphide. This mixture is now passed (1) through a flask half filled with iced water, in which is deposited the greater portion of the excess of carbon disulphide; (2) through a wash-bottle containing potash solution, which absorbs carbon dioxide and sulphuretted hydrogen; (3) through a solution of cuprous chloride in hydrochloric acid, which removes the carbonic oxide; (4) through an alcoholic 12 per cent. solution of aniline, which

takes up the last traces of carbon disulphide, with formation, according to Hofmann, of sulpho-diphenyl-urea, $CS(NH . C_6H_5)_2$; and finally (5) through an ordinary drying tube containing pumice and sulphuric acid. The gas which issues from the apparatus thus arranged is found on analysis, provided the most ordinary precautions are taken not to allow the passage of too rapid a stream, to consist of chemically pure carbon oxysulphide. As regards what happens in the kaolin tube, on allowing it to cool in contact with the mixture of gases and afterwards carefully breaking it, at the end from which the gas issued a quantity of brilliant white needles of silicon sulphide, SiS_2, are found partially obstructing the tube. In place of the kaolin is found a graphitic mass studded with very hard tolerably large crystals of a substance which evolves sulphuretted hydrogen in contact with moist air, and is decomposed by water with precipitation of gelatinous aluminic and silicic hydrates. This most interesting substance is in reality a sulpho-silicate of alumina, or a kaolin in which oxygen has been replaced by sulphur, thus opening up the wide prospect of the formation of a whole series of sulpho-silicates in which the oxygen of natural silicates is replaced by sulphur.

A WORK on the telephone, by Mr. W. H. Preece and Dr. Julius Maier, which has long been in preparation, will be published in the course of a week or two by Messrs. Whittaker and Co., as a volume in their "Specialists' Series." A work on manures will be published shortly in the same series. Its author is Dr. A. B. Griffiths, Principal of the School of Science, Lincoln. Mr. G. R. Bodmer has in the press a practical treatise on hydraulic motors, which will also form a volume of the "Specialists' Series."

THE Carpathian Club in Transylvania, which was founded in 1880, has now about 1600 members. Its object is to investigate the Transylvanian Alps, and to construct good roads and refuges. The Club intends to erect at Hermannstadt a Museum for its library, and for maps, plans, photographic views of the Carpathians, and ethnological and natural history objects.

THE Liverpool Science Students' Association has issued its Report for the session 1887-88. According to the Committee, the condition of the Society, as regards both the number of its members and the character of its work, is eminently satisfactory. The papers read at the evening meetings were, we are told, "of a high character, evidencing much careful observation and patient investigation."

A REPORT from Elba states that the whole of the island is infected by Phylloxera. In Toscana the efforts to check the plague have as yet proved unsuccessful. The insect has also made its appearance at Parmi, in Calabria, at Novara, and at Cervo in Liguria. Reports from the neighbourhood of San Remo and from Lombardy state that the infected areas are constantly increasing.

DURING the last summer, Washington and other eastern cities of the United States were exceptionally free from the attacks of "shade-tree pests." Elm-leaf beetles were not nearly so numerous as usual. In recording this fact in *Insect Life*—a useful publication lately started by the United States Department of Agriculture—the writer refers to "an occurrence which shows how careful one must be in drawing conclusions from experiments to destroy insects." "Counting," he says, "upon the ordinary appearance of the elm-leaf beetle, we sprayed the trees in our garden with London purple early in the summer, and as no damage was done, we were quite of the opinion that the spraying had been a success until, later, we noticed that unsprayed trees were quite free also. In the same way a gentleman came to us toward the end of the season and informed us that he had completely protected his trees, by spraying the grass under them with Paris green, his trees for the first time in several years having retained the verdure of their foliage."

AT a recent meeting of the Linnean Society of New South Wales, Mr. C. W. De Wis read a paper presenting "A Glimpse of the Post-Tertiary Avi-fauna of Queensland." He described such bird-remains as can with confidence be referred to known genera, from the Chinchilla deposits, Darling Downs, Queensland. The fossiliferous beds, which have been exposed by floods in the banks of the Condamine River, have yielded the remains of mollusks, fresh-water fishes, alligators, turtles, and terrestrial vertebrates; whence it may be inferred that the locality was once the site of a densely populated water-course or basin. In keeping with this it might have been anticipated that the birds whose remains have so far come to light would belong for the most part to tribes which haunt the margins or explore the waters of lakes and rivers. And this turns out to be the case, for, with one or two more or less doubtful exceptions, the nine species described are referable to birds of no higher grade than the old order Grallatores, the majority of them belonging chiefly to the Anseres and Rallidæ.

THE last issue of the *Mittheilungen* of the German Asiatic Society of Japan (Heft 39, Band iv.) contains a long account of the remote Chinese province of Kansu, by Herr von Kreitner, who is known to many readers as a member of the Szchenyi Expedition to the borders of Tibet about twelve years ago, and the author of an account of the explorations then made, entitled "Im Fernen Osten." The writer describes the geography of the province in three sections: (1) North-Western Kansu, belonging to the Gobi and Shamo deserts; (2) the central and north-eastern districts, which are drained by the Hoang-ho or Yellow River; and (3) Southern Kansu, belonging to the Yang-tsze drainage area. One feature of the paper is a picturesque account of the loess districts of the province. Prof. Fesca describes briefly two works of his on Japanese agriculture, and the agricultural capacities of the country, which have recently appeared. A third paper, also short, contains the results of an investigation into the chemical changes produced in tea by the process of "firing," a species of roasting which the leaf undergoes after it is picked, and before shipment to Europe.

IN the signature of the letter entitled "The Pasteur Institute," printed in NATURE last week, for Parky*er* read Parky*n*.

THE additions to the Zoological Society's Gardens during the past week include two Philippine Paradoxures (*Paradoxurus philippensis*) from the Philippine Islands, presented by Mr. G. P. Ogg; a Brazilian Tapir (*Tapirus americanus* ♂) from the Province of Parana, South America, presented by Mr. Anthony Taafe; a Long-fronted Gerbille (*Gerbillus longifrons*) from Western Asia, presented by Mr. Lionel Hanbury; a Meyer's Parrot (*Pæocephalus meyeri*) from East Africa, presented by Dr. Hugh Eaton, F.G.S.; two Common Quails (*Coturnix communis*), European, presented by Mr. W. H. St. Quintin, F.Z.S.; a Common Guillemot (*Lomvia troile*), British, presented by Mr. E. Hart, F.Z.S.; a Moorish Gecko (*Tarentola mauritanica*) from the South of France, presented by Mr. J. C. Warbury; an Indian Crocodile (*Crocodilus palustris*), a Hawks-billed Turtle (*Chelone imbricata*) from the Philippine Islands, presented by Captain J. Sommes; a Black Salamander (*Salamandra atra*), European, presented by Mr. G. A. Boulenger, F.Z.S.; a Bald-headed Chimpanzee (*Anthropopithecus calvus* ♀) from the Gaboon, three Dwarf Chameleons (*Chamæleon pumilus*) from South Africa, deposited; a Molucca Deer (*Cervus moluccensis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

COMET 1888 *e* (BARNARD, SEPTEMBER 2).—The following ephemeris for this comet for Greenwich midnight is in continuation of that given in NATURE, vol. xxxix. p. 114:—

1888.	R.A. h. m. s.	Decl. ° ′	Log Δ.	Log *r*.	Brightness.
Dec. 15	1 4 21	7 41·2 S.	0·1390	0·2810	8·7
17	0 56 52	7 43·1			
19	0 49 59	7 43·4	0·1654	0·2776	7·8
21	0 43 38	7 42·4			
23	0 37 47	7 40·1	0·1916	0·2744	7·0
25	0 32 24	7 36·9			
27	0 27 29	7 32·8 S.	0·2170	0·2715	6·3

The brightness on September 2 has been taken as unity.

The spectrum of the comet presents no very interesting feature, as it is mainly continuous, with but little evidence of the bright hydrocarbon bands.

Y CYGNI.—Although it is only just two years since Mr. Chandler discovered this variable, and but comparatively few minima have therefore yet been observed—the fewer that the star seems to have been unaccountably neglected by European observers—yet some strange and strongly-marked anomalies have already been observed in its period. Although the light-curve is of such a shape as to enable the minima to be determined with much precision, the variations in brightness proceeding with relatively great rapidity, and Mr. Chandler had therefore believed that the period he had deduced from his 1886 and 1887 observations could not be more than a second or two in error, he found, when the star came again under observation in the spring of 1888, that the ephemeris required a correction of four or five hours, and this difference increased until, in the October just past, it had amounted to nearly seven hours. Dividing all the observations into five groups, they give the following values for the period:—

d.	h.	m.	s.
1	11	56	31·8
		57	45·3
		58	19·5
		58	30·9

The observations from which these periods have been deduced include five minima determined by Mr. Sawyer, three by Mr. Yendell, and thirteen by Mr. Chandler himself. Using his own observations alone, he has roughly represented them by the following elements: 1889 January 25, 5h. 39·6m. (G.M.T.), + 1d. 12h. 0m. 0·12s. $(E - 519)$ + 0·24s. $(E - 519)^2$; the elements being referred to epoch 519 on account of the convenience of the period at that time being so nearly commensurate with a solar day. But these elements leave residuals far too large to be regarded as errors of observation, whilst the lengthening of the period, half a second between two successive recurrences, is entirely unparalleled in amount by the irregularities in other Algol variables. Mr. Chandler considers that this change is far more likely to be periodic, and of short period, than secular, and it is therefore specially to be desired that observers will follow it with all attention, for the complete knowledge of its changes must throw much light on the whole subject of variation of the Algol type. At present the minima occur soon after sunset, for England, on December 17, and every third following day, and shortly before sunrise on December 19, and every third following day. It is to be hoped that English observers will give a most persistent attention to this star, now so clearly a most important one.

RECENT SKETCHES OF JUPITER.—Vol. xlix. of the *Mémoires couronnés* of the Royal Academy of Sciences of Belgium, contains a valuable series of observations of Jupiter by Dr. F. Terby, of Louvain. This series, which contains his observations from 1882 to 1885, is in continuation of a former memoir, which appeared in vol. xlvii. of the same publication, and is soon to be followed by a third containing the results of his work in 1887. In this present memoir Dr. Terby has made some first efforts towards the identification of details on the surface of Jupiter in successive rotations, a work which he considers he has been able to carry on more successfully in his third series. The present memoir is illustrated by 100 sketches of the planet, which, if of no great beauty or minuteness of detail, are very creditable when the smallness of the telescope used—only 3½ inches aperture—is borne in mind, and are valuable from their number and the completeness of the series which they form. The 1887 observations were made with Dr. Terby's new telescope of 8 inches aperture, which has given excellent proof of its good quality.

On one occasion (1884 February 16) Dr. Terby was fortunate enough to watch the shadow of the first satellite pass over a bright white spot. The shadow lost none of its blackness in the transit, showing that the white spot was in no perceptible degree self-luminous.

85 PEGASI.—Some recent observations which Mr. Burnham has obtained of this close and difficult double, discovered by him in 1878, have enabled Mr. Schaeberle (*Gould's Astronomical Journal*, No. 185) to compute an approximate orbit, from which it appears that the star has a period of 22·3 years, and an eccentricity of 0·35, and that it passed through periastron at the beginning of 1884. The other elements are—

$$\pi - \Omega = 70°·3 \quad \ldots \quad i = 68°·6$$
$$\Omega = 306°·1 \quad \ldots \quad a = 0''·96$$

From observed position-angles and distances of the third star, C, the following result has been obtained for the relative proper motion of 85 Pegasi, viz. annual motion being 1″·305 and its direction 140° 20′·4, or in R.A. and Decl., $\Delta\alpha = + 0''·833$; $\Delta\delta = - 1''·005$. The comparison star, C, would appear to have a slight proper motion also, as these values differ somewhat from those obtained for 85 Pegasi by Argelander, Mädler, and Brünnow.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 DECEMBER 16–22.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 16

Sun rises, 8h. 3m.; souths, 11h. 56m. 7·7s.; sets, 15h. 50m.: right asc. on meridian, 17h. 38·3m.; decl. 23° 22′ S. Sidereal Time at Sunset. 21h. 33m.

Moon (Full on December 18, 11h.) rises, 15h. 3m.; souths, 22h. 48m.; sets, 6h. 43m.*: right asc. on meridian, 4h. 32·5m.; decl. 18° 23′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	7 34	11 26	15 18	17 8·0	23 34 S.
Venus	10 38	14 45	18 52	20 27·5	21 16 S.
Mars	10 50	15 14	19 38	20 56·5	18 36 S.
Jupiter	7 32	11 30	15 28	17 12·5	22 36 S.
Saturn	20 24*	3 51	11 18	9 31·5	15 44 N.
Uranus	2 14	7 38	13 2	13 19·4	7 43 S.
Neptune..	14 26	22 10	5 54*	3 53·8	18 32 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Dec. 19,	22 44	*m*
V Tauri	4 45·6	17 21 N.	,, 17,		*M*
R Canis Majoris	7 14·5	16 12 S.	,, 18,	21 24	*m*
		and at intervals of		27 16	
U Monocerotis	7 25·5	9 33 S.	Dec. 16,		*M*
R Crateris	10 55·1	17 43 S.	,, 18,		*M*
S Virginis	13 27·2	6 37 S.	,, 17,		*M*
U Coronæ	15 13·6	32 3 N.	,, 18,	6 9	*m*
β Lyræ	18 46·0	33 14 N.	,, 20,	7 0	m_2
S Vulpeculæ	19 43·8	27 1 N.	,, 17,		*m*
η Aquilæ	19 46·8	0 43 N.	,, 21,	6 0	*M*
S Sagittæ	19 50·9	16 20 N.	,, 20,	17 0	*m*
R Sagittæ	20 9·0	16 23 N.	,, 21,		*m*
T Vulpeculæ	20 46·7	27 50 N.	,, 22,	2 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 16,	5 40	*m*
		and at intervals of		36 0	
δ Cephei	22 25·0	57 51 N.	Dec. 22,	0 0	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
Near Pollux	115 ...	32 N. ...	Rather swift.
	195 ...	67 N. ...	Swift; streaks.

GEOGRAPHICAL NOTES.

GREAT activity is still being displayed by German explorers in the interior of Togo-land. Captain C. von François has brought to a successful termination his journey into the country confined within the great bend of the Niger. The route taken by him was as follows: viz. Kpandu, Salaga, Jendi, Gambaga, and then across the upper course of the Volta at Bupere (the river at this spot, though more than 80 yards broad, is no longer navigable). From this point he arrived on April 19 last at Surma (11° 28′ N. lat.) in the country of Mosi. He afterwards made an excursion from Gambaga to the south-west by way of Nantong to the River Volta, and returned to the coast through Adeli. In the latter place he met Dr. L. Wolf, who founded a station there on the Adado Mountains in the beginning of May, having travelled to Adeli through the eastern part of Togo-land. Dr. Wolf has, with the aid of a mercurial barometer, been able to determine numerous altitudes with greater accuracy than any measurements previously made in this part of Africa. It appears from his results that Dr. Henrici has considerably over-estimated the height of the Agome Mountains. Herr von Puttkamer, the Imperial Commissioner, made in March an excursion into the region of the French Protectorate as far as the lower course of the Mono, and then explored the country of Agotime up to the foot of the mountains. All these travellers agree in stating that the prospects of the interior of Togo-land are very favourable, both as regards agriculture and commerce. The climatic conditions of these uplands are much more favourable than those of the coast.

THE last issue of the *Bollettino* of the Italian Geographical Society, which is a double number for October and November, publishes an account by Dr. Leopoldo Travesi of an expedition he made last November to the almost unknown district of Jimma (Jimma-Kakka, Jimma Abba-Jifar) on the debatable borderland between Abyssinia and Kaffa. Jimma constitutes a petty Mohammedan State tributary to Menelek, King of Shoa, in the Upper Valley of the Ghibeh, which is an important head-stream of the Gugsa, and which had hitherto been crossed only by one European, Travesi's fellow-countryman, Cecchi. Through fear of "annexation" or "protection," Europeans are jealously excluded, and Travesi only succeeded in penetrating into the country by joining the suite of its ruler, Abba-Jafir, who was returning from a visit to his suzerain, King Menelek. Jimma forms a deep upland valley over 6000 feet above sea-level, about 40 miles long and 10 broad, and inhabited by an extremely mixed population, in which the Abyssinian, Galla, and Negro elements are intermingled in diverse proportions. Hence the great variety of types, and colours ranging from the relatively fair and regular Hamitic to the dark and nearly pure Negro, as well illustrated by the numerous photographs Travesi succeeded in obtaining, and eight of which are here reproduced. The penal code of Jimma is remarkable for its simplicity, most offences being punished by "banishment," which is here a euphuistic expression for "slavery." Hence to the traveller's question, "What do you do with your thieves and other criminals?" the prompt reply invariably was "We drive them from the country," meaning "We sell them." Had Travesi been allowed to penetrate a little further south, he would have solved the problem of the Juba and Sobat (White Nile) water-parting. As it was, he was able, from native report, almost to satisfy himself that the Gugsa, here called the Uma and elsewhere the Abula, flows, not west to the Nile basin, but south-east to Lake Abbala, which is known to be in the Juba basin. Consequently the Gugsa may be regarded as the chief head-stream of that river. He also heard of d'Abbadie's Mount Wosho, here however called *Woso*, which lay away to the south-east, and to describe the altitude of which the natives exhausted the language of hyperbole. They could not exactly say where it was, except that the country was called Am'ca (?), but they knew quite well it was the highest mountain in the world, lost in the skies, &c. The Wosho, whose existence was lately imperilled by certain vague reports, seems thus, at all events, "rehabilitated."

ELECTRICAL NOTES.

THE magnetic elements for Paris for 1888 are—

Declination	15° 52′ 1″
Dip	63° 14′ 7″
H	0·19480
V	0·42245
Total force	0·46520

SORET (*Arch. des Sciences*, April 1888) has reproduced Oliver Lodge's experiment on the dissipation of fog on a small scale by placing a platinum cup, containing water in a boiling condition, by a Bunsen flame in connection with one pole of an influence machine and a point above the water in connection with the other pole. When the machine is not at work, so-called steam ascends undisturbed, but when the machine is excited the clouds whirl and move about in a flame-like fashion, until the vapour disappears entirely. The experiment is made in a dark room, and the cup is illuminated by a beam of electric light.

C. L. WEBER (*Ann. Wiedemann*, xxxiv. p. 576) has made some interesting observations on the variation of the resistance of alloys of tin-lead and tin-bismuth at their period of fusion. A considerable and rapid increase of resistance is observed as the fusing-point is reached, and it is the more marked the simpler the composition of the alloys. Pb_2Sn behaves like pure tin. The tin-bismuth alloys are very irregular, for the specific resistance of bismuth falls as the point of fusion is reached.

VON OETTINGEN (*Ann. Wiedemann*, xxxiv. p. 570) has been repeating his old experiments on the oscillatory discharges of Leyden jars, and he has obtained some admirable photographs of sparks. They give periods of oscillations varying from 19 to 39 millionths of a second.

THE use of Gassner's dry cell is making great progress in Germany. In the latest form hydrated ferric oxide is used as the depolarizer. Ferric oxide is said to abandon all its oxygen in presence of sal ammoniac.

A NOVEL trial is about to commence in Philadelphia. It is to decide the question whether electricity is a condition or a thing, and whether it is something which is manufactured.

A CONGRESS OF ELECTRICIANS will be held in Paris, in 1889, under the presidency of M. Mascart, and under the auspices of the International Society of Electricians. It is proposed to hold the meeting in September, but the British Association meeting in Newcastle will prevent many English electricians from being present.

THE term "therm," in place of *calorie*, for the unit of heat in the C.G.S. system has not met with general approbation, as the other names applied to these units have done. It was, perhaps, hastily accepted; but has it occurred to the dissentients that it might be dispensed with altogether, and that the unit of work "Joule" answers all the purpose of a unit of heat? There are 4·2 *Joules* in a *therm*. They are of the same dimensions, and really indicate the same physical quantity, viz. the mechanical equivalent of heat. Calorie will, however, perhaps hold its own, now that the C.G.S. system is so generally adopted. The only reason that led to the acceptance of the therm was the confusion arising from the kilogramme-degree, as well as the gramme degree, being called a calorie, but the former is fast going out.

THE ANNIVERSARY OF THE ROYAL SOCIETY.[1]

IN the month which intervened between our last anniversary and the end of the year, the Society lost four of its Fellows. In addressing the Fellows last year, I referred to the loss which science had sustained through the death of the illustrious Kirchhoff, and before three weeks were out, one followed him to the grave whose researches on the connection between the emission and absorption of radiant heat and light were closely akin to those of Kirchhoff. I refer to Balfour Stewart, who, shortly after landing in Ireland, whither he had gone to spend the Christmas with his family, was suddenly carried off after only a few hours' illness, shortly after he had entered on his sixtieth year. His name is widely known on account of his scientific work in heat, magnetism, and solar physics. He has been a member of the Council, and the Rumford Medal of the Society was awarded to him for the particular research to which I alluded at the outset. The other three of our ordinary Fellows

[1] Address of the President, delivered at the Anniversary Meeting, on November 30.

who died before the month was out were all far advanced in years. Two of them were eminent in the medical world, Sir George Burrows and Dr. Arthur Farre, both of whom served on our Council. Early in the year we lost one of our Fellows, who, while not a man of science, was eminent in literature and jurisprudence. While our ranks are mainly recruited from men of science, we gladly welcome among us men who, like Sir Henry Sumner Maine, have proved their ability and earned their distinction in other branches of knowledge; whose connection with us we look on as honourable to the Society, while, as the very fact of their joining us shows, they regard the Fellowship as honourable to themselves. Admiral Sir Cooper Key, who was highly distinguished as a naval officer, and was at one time Director of the Royal Naval College at Greenwich, was another who served on the Council. Philip Henry Gosse, who died at an advanced age, is well known for his charming popular works on natural history. These are some of the Fellows on the home list who died since the last anniversary; but, besides these, we have lost no less than three of our foreign members. Prof. Anton de Bary, so well known for his researches on the Cryptogams, and the eminent naturalist, Prof. Asa Gray, who not very long ago was over in this country, both died in January. Comparatively recently we have lost Prof. Clausius, so eminent as a physicist, especially in the department of thermodynamics.

The year of the Society which terminates to-day has shown no flagging in scientific activity. Since the last anniversary, thirty-three memoirs have been published in the Philosophical Transactions, containing a total of 1010 pages and 91 plates. Of the Proceedings, nineteen numbers have been issued, containing 1008 pages and 17 plates. In addition to this, a monograph of the Horny Sponges of Australia, by Dr. R. von Lendenfeld, which was accepted for publication by the Council, and which when completed will extend to about 1000 pages, is now nearly through the press.

A large amount of work connected with the Library has been done since the last anniversary. A special effort has been made to complete imperfect series of scientific periodicals; and by means of exchange, or by the generosity of our corresponding Societies, some hundreds of deficient numbers have been obtained. The Lists of Institutions entitled to receive gratis the Philosophical Transactions and Proceedings have also been carefully revised by the Library Committee.

In December last, Mr. Arthur Soper was engaged as a special Assistant to continue the formation of the Shelf-Catalogue, and the revision of the Catalogue of MSS., and other work. The Shelf-Catalogue of the Upper Library is now completed—a work involving the rearrangement or removal to the lower stories of several thousands of volumes. Considerable progress has been made in collating and cataloguing the Archives and other manuscripts belonging to the Society, and an instalment of slips have been written towards a Catalogue of the Miscellaneous Literature in the Library.

In the course of this work many duplicate scientific books, and literary works of little value to the Society, have been thrown out, and these have been presented, by order of the Council, to the libraries of the Universities and some of the chief scientific Societies.

The cataloguing of the titles of scientific papers for the decade 1874 to 1883 is now complete, and the work is ready for the press. The amount of matter is estimated to require, if printed, three quarto volumes of the usual size. The extraction of the title, the preparation of the work for the press, and the correction of the proofs of this work, which is really of international importance, has all along been done at the sole charge of the Royal Society; but the printing of the volumes which have already been published has been done at the Stationery Office, by the authority of the Lords of the Treasury, and the proceeds of the sale have been paid in to the Treasury. The Council have applied to the Lords Commissioners of Her Majesty's Treasury to sanction the printing of the last decade in a similar manner, and it is hoped that the application may be favourably entertained.

In the year 1882 a change was made in the amount and mode of administration of the grant which for a considerable time before had been voted annually by Parliament for scientific research. Since that year the annual grant has been one of £4000, which has been administered by the former Government Grant Committee, with the addition of certain *ex-officio* members, mostly the Presidents of certain scientific Societies. Meetings of this large Committee, consisting usually of about fifty members, have been held twice a year, and the various applications for aid from the grant to enable the applicant to carry out investigations explained by him, have been previously discussed in meetings of three, or latterly two, Sub-Committees, into which the whole Committee was divided, and then submitted to the General Committee for confirmation or modification.

In the discussion of these grants, the Government received the benefit of the gratuitous services of a large number of men of the highest distinction in science. In the large Sub-Committees, however, it necessarily happened that of the members present only a fraction would be likely to be conversant with the particular branch of science to which any particular application belonged; and the Council thought that the time of the members might be economized, and at the same time a more efficient discussion of the grants secured, by arranging the applications under a number of subdivisions, and assigning the discussion of these to a corresponding number of Boards formed out of the General Committee. It was thought that a good deal of the discussion of the applications in the several branches might be carried on by correspondence among the members of the respective Boards, so that one or two meetings of each Board might suffice. If some trouble were thus saved to the members of the Committee in regard to personal attendance at long meetings, there would probably be more expenditure of time in the way of correspondence, and it was thought that one meeting of the General Committee in the year would in most cases suffice. To meet pressing cases in the interval, it was suggested that a limited sum might be placed by the General Committee at the disposal of the Council of the Royal Society. There are further provisions for forming a reserve fund of not more than £2000 to meet special objects involving unusual expenditure, and for holding in reserve out of the money available for any one year enough to meet annual grants of limited amount made for a period not exceeding three years, the future grants being contingent on the receipt by the Committee of satisfactory evidence of progress in the inquiry. The new regulations, of which I have merely given a slight sketch, have been communicated to the Treasury, and will come into operation next year.

The Krakatão Committee have now completed their work, and the volume which is the outcome of their labours is in the hands of the public. The Society is much indebted to those Fellows and other gentlemen who discussed and reported on the different subjects into which the whole inquiry was divided, and to Mr. Symons, who was the first to propose that the materials should be collected, and to whose unwearied labour as Chairman of the Committee, director of the correspondence, and editor of the volume the successful accomplishment of the undertaking is largely due. The work has been favourably noticed in more than one quarter. A comprehensive and digested account of that extraordinary volcanic explosion, remarkable both for its magnitude and the striking disturbances and other phenomena attending or following it, is now placed within easy reach of the ordinary reader, and will go down to posterity; whereas, had the various accounts remained in their isolated form, they would many of them have perished, and the remainder could not have been brought together without a most laborious search. It must be a great satisfaction to my predecessor in this chair to remember that he urged upon the Council the importance of collecting the facts before the materials should have become dissipated, and while the freshness of men's recollection of the event kept up a lively interest in all that belonged to it.

The Royal Society is in possession of some important standards for the safe keeping of which we are responsible. Parliamentary copies of the standard yard and standard pound have been intrusted to our custody; and we have also a standard measure of length known as Sir George Schuckburgh's scale, with reference to which the length of the seconds pendulum for Greenwich has been determined by Kater and Sabine. This length, as determined by experiment, has been defined with reference to the interval from the 0 to the 39- and 40-inch graduations on the scale; but no exact comparison has hitherto been made between the length of this portion of the scale and the national yard, and such a comparison is no easy matter. It happens that Commandant Deforges has been engaged in determining the length of the seconds pendulum at Greenwich with reference to the French standard metre, and just before his return to Paris he came to our meeting, and offered to take charge of the scale, bring it with him to Paris, and there determine the length of the part of the scale used by Kater and Sabine with reference to the metre, for doing which he has all the requisite appliances; and as we know the ratio of the metre to the yard,

the length of the seconds pendulum as determined by Kater and Sabine would thus be known accurately with reference to the standard yard. It seemed to me that so important a scale should hardly be sent away, even though in the care of so experienced a physicist, without the authority of the Council, and without an outer case being made for its box, which there was no time to get ready. The authority of the Council has since been obtained, and it fortunately happens that one of the assistants at the Greenwich Observatory is going to Paris, who will take charge of the scale. Thus by the kind proposal of Commandant Deforges, we may shortly hope to have an authentic comparison of the length of the seconds pendulum as measured by Kater and Sabine with the standard English yard.

At the time of the anniversary last year, some of the reports of the observers who went to Grenada to observe the total solar eclipse of August 1886 had been seen in, and I mentioned that it seemed desirable, for convenience of reference at a future time, that the different reports should come out together, instead of being published in a scattered form, provided at least that the waiting for the later reports should not cause too much delay. I regret to say that the completion of the reports has been delayed in part by the illness of one of the observers, but I have every hope that they will all be in by Christmas, and I do not anticipate that any long time will elapse before they will be in some form in the hands of the public.

The time is well within our recollection when the occurrence of the solar prominences seen in total eclipses first attracted the attention of astronomers, and when for observations bearing on their nature we had to wait for the rare and brief glimpses which, clouds permitting, were afforded by total eclipses. Now, however, thanks to the method of observation devised independently by Lockyer and Janssen, they may be studied at any time. It would obviously be a great advantage if a similar study could be made of the corona; for though we cannot expect to obtain a picture of it equal to that which may be got during a total eclipse, yet, if a fairly good picture could be obtained from time to time, we might thereby be enabled to learn more about the history of its changes than could be got by observations extending over a lifetime if restricted to total eclipses. Some observations were made during the partial phases of the last total eclipse with the view of throwing light on the prospect of success. Notwithstanding the unpromising nature of the results obtained, I have reason for hoping that the desired object may yet be accomplished.

In addressing you last year, that year which will be memorable as the Jubilee of the reign of our beloved Sovereign, I alluded briefly to the progress which science had made in the last half-century, and ventured to indicate one or two directions in which it seemed to me possible that a very great addition to our physical knowledge might some day be reached. I will not to-day venture to look so far ahead; but the mention of a total eclipse leads me to refer to some theories now before the scientific world which are likely to undergo full discussion and further examination in the near future, with the probable result of a pretty general agreement as to their acceptance or rejection.

It is now many years since Dr. Huggins discovered the peculiar character of the spectra of the nebulæ, spectra which he found to consist mainly of bright lines, indicating that what we see is an incandescent gas. The natural supposition to make at the time was that those distant masses of matter consisted of incandescent gas, of which the luminosity was in some way kept up, probably as a result of condensation. But the researches of Mr. Lockyer, as described by him in the Bakerian Lecture which he delivered last spring, and in part in a previous paper communicated shortly before the last anniversary, have led him to take a different view of the constitution of nebulæ. According to the theory advanced by him, the mass of a nebula consists mainly of meteorites, which are constantly coming into collision here and there; and the glowing gas the existence of which the spectroscope reveals, is merely a portion of the matter, volatilized by the heat of collision. According to the former view therefore, the nebula consists of glowing gas, not yet condensed into a solid or liquid form, possibly in a oondition even more elementary than that of the so-called elements that we know on earth; according to the latter it consists mainly of discrete portions of solid matter, and the glowing gas does not consist of the same matter permanently glowing, but is continually supplied afresh by fresh collisions.

A similar theory is applied to explain the self-luminosity of the nucleus, and sometimes the very root of the tail, of comets. A comet is regarded as a swarm of meteorites, moving in orbits not greatly differing from one another; and as the swarm approaches the sun collisions become more frequent, and individually more potent, from an increase in the velocities, differential as well as absolute; and a portion of matter is volatilized and rendered incandescent. As to the tail, the theory long ago suggested by Sir John Herschel has always seemed to me by far the most probable of those that have been advanced—namely, that it is due to the propulsion of excessively attenuated matter, owing to a repulsive force, probably of electrical origin, emanating from the sun. This view seems to be adopted both by Mr. Lockyer and Dr. Huggins; and the latter gentleman, in an earlier Bakerian Lecture, has suggested a new theory of the corona—the corona as distinguished from the prominences—namely, that it is not projected from the sun by molar forces due to the tremendous state of turmoil in which we have very strong reason for believing that the matter composing the sun exists, but of matter actually propelled from the sun by a repulsive force in the manner of the tails of comets.

Daring as some of these speculations may appear to be, there seems a great deal to recommend them, and the whole subject is one of extreme interest at the present day.

But I must not take up your time longer by dwelling on so special a subject; I proceed to matters more particularly connected with the occasion on which we are assembled.

The Council have awarded the Copley Medal of the year to my predecessor in this chair, Mr. Huxley, for his investigations on the morphology and histology of vertebrate and invertebrate animals, and for his services to biological science in general during many past years. These subjects lie so entirely out of the range of my own studies that I need hardly say that in attempting to give some idea of the more salient features of his investigations I am dependent upon the kindness of biological friends.

During the fifteen or twenty years which preceded the publication of Darwin's famous work, the "Origin of Species," the views and methods of comparative anatomists underwent a most marked change. Without that change, biologists would have been far less prepared to accept Mr. Darwin's work, and, what is even more important, would have been unprepared to make use of that work as a light enabling them to carry on the remarkable researches which have so brilliantly characterized the progress of biology during the last quarter of a century. That change was effected chiefly by the labours first of Johannes Müller, and subsequently of Huxley in this country, and of Gegenbaur in Germany. The labours of these men opened out the right road of morphological inquiry. It is not, perhaps, too much to say that Mr. Huxley's treatment of his subject in his "Morphology of Cephalous Mollusca" was to many young morphologists little short of a revelation, and all his other works of the same period, such as that on the Hydrozoa and on Tunicates, and, later still, his treatment of the Vertebrate skull and skeleton, and Arthropoda, produced in varying degree a like effect.

Closely allied to, or rather forming part of, his morphological labours, are his numerous palæontological researches, carried out for the most part while he was Palæontologist to the Geological Survey, researches characterized by the same clear morphological insight, researches which have been as profitable to animal morphology as useful to the geologist. The most important are perhaps those on the remarkable reptiles of the Elgin Sandstones and on the Dinosauria; but many others have great value, and his anniversary address to the Geological Society, in 1870, made its mark.

Though his career has been in the main that of a morphologist, he has through the common ground of histology given considerable help to physiology. An early paper by him "On the Cell-Theory," did much to clear away erroneous notions concerning the relations of structure to the actions of living beings. His article on "Tegumentary Organs" was a great step onward as regards both morphology and histology, and still remains a classical work; while, by other papers and in various ways, he has contributed to the progress of histology and physiology.

But, however important Mr. Huxley's original contributions to the advancement of our scientific knowledge have been, we should form a very inadequate idea of his benefits to the cause of science if we did not bear in mind also his singular ability and effectiveness as an expositor of science to the people, and the powerful influence he has exerted in the improvement of the teaching of

biology in its widest sense in this country. Indeed, it is not too much to say that the remarkable improvement which has taken place within the last few years must be ascribed either directly or indirectly to his influence, and has been in many cases due to his initiation.

The Rumford Medal has been awarded to Prof. Pietro Tacchini for important and long-continued investigations, which have largely advanced our knowledge of the physics of the sun.

Prof. Tacchini occupies a foremost place among those who have paid special attention to the physics of the sun. Since 1870 he has unceasingly observed, first at Palermo, and afterwards at Rome, the solar prominences. The information at our disposal at the present time, both as regards their distribution, their spectra, and the changes which take place in them, and their connection with other solar phenomena, rests to a large extent upon his individual efforts. His memoirs on this subject are very numerous. He has been engaged in the observation of four total solar eclipses, and from some of the phenomena therein observed has drawn the important conclusion that many of the so-called prominences are really descending currents.

A Royal Medal has been awarded to Sir Ferdinand von Mueller for his long services in Australian exploration, and for his investigations of the flora of the Australian continent.

For more than forty years von Mueller has been working, without intermission, at scientific botany and its practical illustrations. As a botanical traveller and collector, he has, to quote the words of Sir Joseph Hooker, "personally explored more of the Australian continent than any other botanist, except Allan Cunningham." No one has investigated the Australian flora and the geographical distribution of its components with so much perseverance and success, and no one has enriched our herbaria, laboratories, and gardens with materials for study to so great an extent. The eleven volumes of the "Fragmenta Phytographiæ Australiæ" contain the descriptions of a great series of new plants, and the unrestricted communication of his collections and observations to the late Mr. Bentham rendered possible the preparation of the "Flora Australiensis," in seven volumes, the only account of the vegetation of any large continental area which has at present been completed.

He has especially devoted himself to the elucidation of the most difficult though most characteristic groups of the Australian flora; and as a result of his labours in this direction, his "Eucalyptographia" may be more particularly mentioned, a work which will always be the standard of nomenclature for the intricate genus *Eucalyptus*. Of a similiar character are his descriptions and illustrations of the "Myoporineous Plants of Australia," and his "Iconography of the Genus *Acacia*." To him is also due the foundation of the Government Herbarium at Melbourne, the first great botanical collection formed in the southern hemisphere, and the future centre of all scientific work on the Australasian flora.

A Royal Medal has been awarded to Prof. Osborne Reynolds for his investigations in mathematical and experimental physics, and on the application of scientific theory to engineering.

Prof. Reynolds was among the first to refer the repulsion exhibited in that remarkable instrument of Mr. Crookes's, the radiometer, to a change in the molecular impact of the rarefied gas consequent upon the slight change of temperature of the movable body due to the radiation incident upon it; and in an important paper published in the Philosophical Transactions for 1879, he deduced from theoretical considerations the conclusion that similar phenomena might be expected to be observed in bodies surrounded by a gas of comparatively large density, provided their surfaces were very small. He verified this anticipation by producing on silk fibres, surrounded by hydrogen at the atmospheric pressure, impulsions similar to those which in a high vacuum affect the relatively large disks of the radiometer.

In an important paper published in the Philosophical Transactions for 1883, he has given an account of an investigation, both theoretical and experimental, of the circumstances which determine whether the motion of water shall be direct or sinuous, or, in other words, regular and stable, or else eddying and unstable. The dimensions of the terms in the equations of motion of a fluid when viscosity is taken into account involve, as had been pointed out, the conditions of dynamical similarity in geometrically similar systems in which the motion is regular; but when the motion becomes eddying it seemed no longer to be amenable to mathematical treatment. But Prof. Reynolds has shown that the same conditions of similarity hold good, as to the average effect, even when the motion is of the eddying kind; and moreover that if in one system the motion is on the border between steady and eddying, in another system it will also be on the border, provided the system satisfies the above conditions of dynamical as well as geometrical similarity. This is a matter of great practical importance, because the resistance to the flow of water in channels and conduits usually depends mainly on the formation of eddies; and though we cannot determine mathematically the actual resistance, yet the application of the above proposition leads to a formula for the flow, in which there is a most material reduction in the number of constants for the determination of which we are obliged to have recourse to experiment.

There are various other investigations of Prof. Reynolds's which time would not allow me to enter into, and I therefore merely mention his investigation of the relation between rolling friction and the distortion produced by the rolling body on the surface on which it rests, that of the effect of the change of temperature with height above the surface of the ground on the audibility of sounds, and his explanation of the effect of lubrication as depending on the viscosity of the lubricant.

The Davy Medal has been awarded to Mr. Crookes for his investigations on the behaviour of substances under the influence of the electric discharge in a high vacuum.

Mr. Crookes's remarkable series of researches which conducted him to the invention of the radiometer led him to work with excessively high vacua. In connection with this he found that an electric discharge in such vacua is capable of exciting effects of phosphorescence apparently quite different in their origin from those produced in the ordinary way by such discharges. The latter are clearly referable to the action of the ethereal undulations which are propagated from the seat of the discharge. But the former involve in some way the effect of the actual transference of the molecules of ponderable matter. These phenomena in the hands of Mr. Crookes opened up a new means of discrimination between different bodies, and he has applied them as a test for the discrimination of groups of rare earths, not yet fully investigated. The test went hand in hand with processes of chemical separation. But here a great difficulty presented itself. So very closely allied in their chemical properties are the members of the groups, that it was only by an excessively tedious and laborious system of fractional precipitation that Mr. Crookes was able to effect a pretty fair separation. Even still, the separate existence of some members of the groups is more or less problematical. It is for these most painstaking researches that the medal has been awarded.

The existence, or apparent existence, of so many earths of such close chemical relationship led Mr. Crookes to speculate on the possibility that after all the molecules of what is deemed a chemical element may not be absolutely alike, as chemists have almost universally believed, but only very approximately so, and that what is deemed the molecular weight of the substance may really be that of the average of its molecules. Should such groups exist, it is conceivable that by processes of very delicate chemical separation they might be split up again into sub-groups, the molecules of which still more nearly match one another; so that according to this view the number of groups into which an element, or what is deemed such, might be split up, not, be it observed, by any dissociation, but merely by a sorting of the molecules which are very nearly alike, may be somewhat indefinite.

Chemists will not probably be disposed to give up the idea of the perfect similarity of the individual molecules of elementary bodies; but it is surely legitimate for one who has worked so assiduously at these difficult separations to suggest, merely as a matter for chemists to think about, a possible view of the nature of elements different from that to which they have been accustomed.

MOTIONS OF THE SOLAR SYSTEM.[1]

NO other hypothesis has been suggested which offers such direct and complete answers to most of the questions which relate to the origin, structure, and unity of the universe, as Newton's law of gravity. It is but natural, therefore, that the

[1] Abstract of an Address before the Section of Mathematics and Astronomy of the American Association for the Advancement of Science, at Cleveland, O., August 15–22, 1888, by Ormond Stone, Vice-President of the Section.

majority of the problems which arise in regard to the motions of the solar system should have their origin in an effort to confirm that law.

The first attempt to apply Newton's law to all the motions of the solar system was made by Laplace. When, however, Lindenau and Bouvard undertook to compute their tables of the motions of the planets, a complete revision of Laplace's theory was found necessary. So enormous is the labour involved, that there exists, besides those mentioned, only one other complete set of theories and tables of the motions of the principal planets—that of Leverrier. Leverrier's tables of the inner planets are now nearly thirty years old. His tables of the outer planets are much later, having employed his attention almost to the day of his death. His tables of Jupiter and Saturn were published in 1876, and those of Uranus and Neptune in the year following. Newcomb's tables of Neptune were published in 1865; those of Uranus, in 1874. Hill's theory of Jupiter and Saturn, which has for years occupied his attention, has at last been completed, and he is now engaged in preparing tables therefrom. These are intended to form a part of a complete series of tables of the principal planets now being prepared under the direction of Prof. Newcomb at Washington. Another such series is a'so being prepared by Prof. Gyldén at Stockholm.

The values of the coefficients of the terms of short period in the motions of the principal planets are now pretty well known; and the same might be said of the secular variations, were it not for the difference between theory and observation which exists in regard to the motion of the perihelion of Mercury, which was discovered by Leverrier, and has been confirmed by Newcomb, in a discussion of the observations of the transits of Mercury extending over a period of more than two centuries. The cause of this difference still remains unknown. The completion and comparison with observations of the new theory of the four inner planets, now being prepared under the direction of Prof. Newcomb, will be awaited with interest, with the hope that it may throw new light on this interesting subject.

The only recent original tables of the moon's motions are those of Hansen. These, like Leverrier's tables of the inner planets, are now more than thirty years old. These tables have been compared with observations, and agree fairly well with those made during the century preceding their publication, but not with those made before or since that time. The theoretical value of the acceleration of the moon's longitude is 6″; that found by Hansen from accounts of ancient total eclipses of the sun, 12″. Newcomb, however, considers these accounts as unreliable, and, limiting himself to the Ptolemaic eclipses of the "Almagest" and the Arabian eclipses of the "Table Hakémitc," obtains the value 8″·3, or, from the Arabian eclipses alone, 7″—a value but little greater than the theoretical value. Dr. Ginzel, from an extended examination of accounts of ancient and mediæval total eclipses of the sun, concludes that Hansen's value requires a change of only a little over 1″. His solution, however, in reality depends upon the ancient eclipses alone. The only other theory of the moon comparable with Hansen's is that of Delaunay. This theory, however, is limited to a determination of the inequalities in the motion of the moon due to the action of the sun, on the hypothesis that the orbit of the earth is a pure ellipse; and differs from that of Hansen in that the inequalities determined are not expressed numerically, but only symbolically in terms of arbitrary constants.

While the coefficients of the inequalities upon which Hansen's tables are based seem to be pretty well known, I am not aware that the tables themselves have been sufficiently checked, except by comparison with observations. Apparently the great desideratum now is a set of tables computed from Delaunay's theory in a completed form, or computed in some other way entirely independently of Hansen's. Until Hansen's tables are thus checked, it is questionable whether it can be safely said that the motion of the moon cannot be completely accounted for by the law of gravity.

The detection of the two satellites of Mars by Prof. Hall may be considered the most interesting recent achievement in pure discovery. It was not till the discovery of these satellites that a means was offered for the accurate determination of the mass of that planet. No satellites of Venus and Mercury have as yet been detected, and the values at present assumed for the masses of those planets are very uncertain.

In 1788, just one hundred years ago, Laplace published his theory of Jupiter's satellites. This theory is still the basis of the tables now in use. Souillart's analytical theory of these satellites appeared in 1881. The numerical theory was completed only within the last year, and the tables therefrom remain still to be formed.

Bessel made a careful investigation of the orbit of Titan; but the general theory of the Saturnian system which he commenced, he did not live to finish. Our knowledge of the motions of Saturn's satellites, with the exception of Titan, was very meagre until the erection of the great equatorial at Washington. A difficulty in the determination of a correct theory of the motions of Saturn's satellites is the fact that there are a number of cases of approximate commen-urability in the ratios of their mean motions. The most interesting case is that of Hyperion, whose mean motion is very nearly three-fourths that of Titan. In this case there is the additional difficulty that their distance from one another is only about one-seventh as great at conjunction as at opposition.

Our knowledge of the motions of the satellites of Uranus and Neptune depends almost entirely on the observations made at Washington. Quite accurate determinations of the masses of these two planets have been obtained. The large secular motion of the plane of Neptune's satellite, to which Marth has called attention, needs confirmation.

The number of the asteroids is so great that they have been the frequent subject of statistical investigation. The systematic grouping of the nodes and perihelia which exists was shown by Newcomb to be the effect of perturbation. Glauser finds that the grouping of the nodes on the ecliptic is a result of a nearly uniform distribution on the orbit of Jupiter. Prof. Newton had previously found that the mean plane of the asteroid orbits lies nearer to the plane of Jupiter's orbit than to the orbit plane of any individual asteroid. Eighty-five per cent. of the asteroids have mean motions greater than twice and less than three times that of Jupiter; and the mean motions of none approximate closely either of these, the two simplest ratios possible. The next simplest ratios lie beyond the limits of the zone; that is, there are no asteroids having mean motions nearly equal to or less than one and a half times that of Jupiter, and none nearly equal to or greater than four times that of Jupiter. The labour of determining the general perturbations and computing tables of an asteroid is as great as in the case of a major planet. It is no wonder, therefore, that tables have been prepared for scarce a dozen of these small bodies, and that these are already out of date.

Of well-known comets of short period, Encke's, which has the shortest period of any, possesses the greatest interest to the student of celestial motions, since it was from a discussion of the orbit of this comet that Encke detected evidence of the existence of a resisting medium which produces an acceleration in the comet's mean motion. This acceleration has been confirmed by the investigations of Von Asten and Backlund. The investigations of Oppolzer and Haerdtl indicate that there is an acceleration also in the mean motion of Winnecke's comet.

We have thus glanced briefly at the present condition of our knowledge of the motions of the principal bodies of the solar system. Only four cases have been found in which we cannot fully explain these motions, so far as known, by Newton's law of gravity. The unexplained discordances are the motion of the perihelion of Mercury, and the accelerations of the mean motions of the moon and the two periodic comets just named.

If we go beyond the solar system, we cannot tell whether Newton's law does or does not apply without modification to all parts of the universe. It is principally in the hope of answering this question that double-star observations are carried on; and, in the case of the many binary systems already detected, Newton's law is satisfied within the errors of observation. Nevertheless, this evidence is purely negative, and its value, it seems to me, not at all commensurate with the labour expended upon it, unless it be in the case of such objects as Sirius, whose observation may assist in the solution of the problem of irregular so-called proper motion. The angles subtended are in general so small that relatively large personal errors are unavoidable; so that, even though their motions be controlled by a law or laws of gravity widely different from that of Newton, it is not likely that such differences can be proved with any degree of certainty. It is rather to the study of the proper motions of the fixed stars and of the nebulæ, and then only after a lapse of hundreds and perhaps thousands of years, that we must look for a solution of this question.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—It is proposed to alter the system of papers in the second part of the Natural Sciences Tripos, which has lasted for many years, according to which questions in each subject are set in every paper. Formerly one question in each subject was so set; latterly at least two questions in each have been set. It is considered that under the present system candidates with an extensive knowledge of one subject may not have time to show such a competent knowledge of a second as is required to gain a first class. It is now proposed to set four separate papers in each of the eight subjects, and to combine them in groups of two subjects, so as to get the examination over in eight days. Probably this may remedy the evil complained of, which can only affect a minimum of candidates; but it will re-introduce the evil which the present system was intended to obviate—namely, it will give an opportunity for taking a number of subjects by means of cramming. It is also proposed to make the change next June, an altogether insufficient length of notice.

Prof. W. G. Adams, F.R.S., has been approved for the degree of D.Sc.

Mr. Francis Darwin, M.A., F.R.S., Reader in Botany, has been elected to a Fellowship at Christ's College.

Dr. Hill, Master of Downing College, has been appointed University Lecturer in Advanced Human Anatomy, and Mr. Walter Gardiner, Fellow of Clare College, University Lecturer in Botany, for five years in each case.

Dr. Guillemard has resigned the University Lectureship in Geography owing to ill-health, and a fresh election will take place in January, for the remainder of the term of five years, ending midsummer 1893. The stipend is £200 per annum. Candidates must send their names, with brief statements of their qualifications, and the methods they propose to adopt, to the Master of Caius College on or before January 8 next.

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for October 1888 contains the following:—On the structure of three new species of earthworms, with remarks on certain points in the morphology of the Oligochæta, by Frank E. Beddard (Plates xii. and xiii.).—This paper contains an anatomical description of *Acanthodrilus annectens*, n. sp., and *Deinodrilus benhami*, nov. gen. et. sp., from New Zealand, and *Typhæus gammii*, n. sp., from Darjeeling. Among the more important anatomical facts detailed, are the independence of the vasa deferentia and atria in Acanthodrilus; the independence of the single vas deferens and its atrium in Typhæus; the occurrence of six pairs of setæ in each (setigerous) somite of Deinodrilus; the completely double dorsal blood-vessel of Deinodrilus in a separate cœlomic space; and the presence in *Moniligaster barwelli* of an atrium consisting of a thick glandular covering of peritoneum, of a layer of muscular fibres, and finally of a single layer of columnar epithelium; the atrium being similar to that of Rhynchelmis.—On the development of the fat-bodies in *Rana temporaria*; a contribution to the history of the pronephros, by Arthur E. Giles (Plate xiv.). The fat-bodies in the frog are formed by a fatty degeneration, not of the anterior end of the genitalia, but of the pronephros or head kidney; it seems highly probable that the structure described by Balfour in the Ganoids and Teleostei as lymphatic tissue is the persistent but structurally and functionally modified pronephros.—On two new types of Actinaria, by Dr. G. Herbert Fowler (Plate xv.). In a bottle of corals, which had been collected from the reefs off Papieté during the expedition of H.M.S. *Challenger*, three small Actinaria were found, which would seem to differ markedly from all hitherto described types; so much so as to possibly necessitate the formation of a new tribe, of equal value with the Edwardsiæ, &c. The name proposed is *Thaumactis medusoides*, gen. sp. nn. The animal is flattened in shape, and almost medusiform; it appears to be free-swimming, for the aboral is like the oral ectoderm, and there is no trace of any attachment. Fourteen true tentacles surround the stomodæum, and peripherally to them are the pseudo-tentacles; the true tentacles with the stomodæum are drawn downwards and outwards into the cœlenteron; in the largest specimen twenty-one pairs of mesenteries, and in the smallest eleven pairs were present; no generative organs were met with. The second form was found attached to a piece of Millepore and is called *Phialactis neglecta*, gen. sp. nn. In this new genus the tentacles are replaced not by stomidia—slight elevations of the oral disk, surrounding the large opening which is homologous with the pore at the tip of some normal Actinarian tentacles—but by what the author calls "sphæridia," *i.e.* ampullate diverticula of the inter- or intra-mesenterial chambers, devoid of an opening to the exterior, and homologous, therefore, with the imperforate tentacles of many genera.—Morphological studies, ii. the development of the peripheral nervous system of Vertebrates; Part I, Elasmobranchii and Aves, by Dr. J. Beard (Plates xvi.–xxi.). This important memoir has appended to it a *résumé* of its chief results.

Revue d'Anthropologie, troisième série, tome iii. fasc. 6 (Paris, 1888.)—On the conversion of the cephalic index into a cranial index, by M. P. Topinard. This paper gives the author's reply to the objections raised by M. Houzé, of Brussels, against his method of determining comparative cephalic and cranial measurements. He explains the various methods employed by Broca and others, and points out the sources of error dependent upon the varying length of time in which skulls have been preserved owing to the gradual drying up of the cranial substance after prolonged preservation in our museums. Thus the craniometric determinations made under the latter conditions must be different from those obtainable immediately after death, or on removal from a damp humus.—Continuation of M. Boule's essays on the stratigraphic palæontology of man. The relations between the Pliocene and the Glacial formations of North America are here considered at length. In concluding his summary of the results yielded by the valuable labours of American palæontologists, M. Boule expresses his assent to the opinion advanced by Mr. Putnam, that recent discoveries afford conclusive evidence that a portion of North America from the Mississippi to the Atlantic was occupied by man contemporaneously with the mastodon and the mammoth, at a period when all the north of the continent was covered by vast glaciers. The closing part of the paper treats of the French classical beds at Chelles, Saint-Acheul, &c., from which date the earliest researches regarding fossil man in France.—On the concurrence in certain crania of divergent characteristics as exemplified in a series of Burmese skulls, by M. Hovelacque.—Kashgar and the passes of the Tian-shan Range, by Dr. Seeland. This is the first of a series of papers communicated by the doctor appointed by the Russian Government to institute proper measures for preventing the advance into the provinces of Semiretche and Ferganah of the severe epidemic of cholera, which had broken out in the Kashgar dominions in 1886. These provinces, which remained in a savage and uncultivated condition till they were brought under Russian dominion in 1862, are necessarily almost a *terra incognita*; and hence Dr. Seeland's narrative of his travels from Vierny to Ak-su in Kashgar, by way of Naryne, which compelled him to cross the colossal range of the Tian-shan range, is a valuable addition to our geographic knowledge of this portion of the Russo-Chinese frontier-lands, while his descriptions of their natural products, and his remarks on the habits and character of the Kirghis hordes, now being thrust back by the Russians, supply much information that is new to science.—Palæontology in Switzerland, by Dr. Victor Gross. This is a useful summary of the large and important mass of materials accumulated by recent Swiss palæontologists. After treating of the various periods of cave and lacustrine habitations, and of the later pile-dwellings, or *crannoges*, he considers at length the character and importance of the various finds belonging to the several stations. Of these, the Lakes of Bienne and Neuchâtel are remarkable, as having already yielded more than 19,500 complete bronze objects, of which fully three-fourths were of a decorative or domestic character, rings and pins numbering 4000. Important investigations are at present being carried out at the La Tène station, where the finds have hitherto been so exclusively connected with weapons of offence and defence as to lead to the inference that its pile constructions marked the site of some primitive fort. The search for lacustrine graves, successfully begun in 1876 is also being vigorously prosecuted.

Memoirs of the St. Petersburg Society of Naturalists, vol. xix.—*Geology and Mineralogy*:—*Dactylodus rossicus*, a new species of fish from the Moscow Coal-measures, by A. Inostrantseff.—The diabase deposits of Olonets, by F. Levinson-Lessing, being an elaborate work which contains a general geological and geographical description of the region, and a detailed

petrographical description of the rocks and their metamorphoses. A sketch-map and five plates of microscopical sections accompany the paper, which is well summed up in German.—Geological observations on the Yug River, by B. Polyenoff.—On the *Spaniodon barbotii* deposits of Crimea and the Caucasus, by N. Andrusoff (also summed up in German).—*Zoology and Physiology*:—Notes on the ichthyology of the basin of the Amur, by N. Warpachowski and S. Hertzenstein, being an elaborate description of forty species, and their connection with kindred species in neighbouring regions (diagnoses in Latin, and the whole summed up in German).—On the Vertebrate fauna of the Balkhash depression, by M. Nikolsky. The most interesting conclusions of the author as to the recent geological history of the Balkhash depression have already been mentioned in NATURE. Now we have a full description of the fauna (39 mammals, 226 birds, 21 reptiles, 3 amphibians, and 16 fishes), together with an elaborate inquiry into the connection of this fauna with those of the neighbouring regions.—The *Percarina* and *Benthophilus* of the Sea of Azoff, by J. Kuznetsoff.—The minutes of proceedings contain several papers of great interest—namely, on a journey to Dutch India, by A. Korotneff; on the ornithology of Caucasus, by K. Rossikoff; on the fossils of the Nijne-udinsk cave, by I. Tcherski; on a journey to Turkestan and Bukhara, by S. Lidsky, &c.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 22.—"On the Magnetization of Iron and other Magnetic Metals in very Strong Fields." By J. A. Ewing, B.Sc., F.R.S., Professor of Engineering in University College, Dundee, and William Low.

The large magnet of the Edinburgh University, kindly lent by Prof. Tait, was used throughout the experiments, and allowed the authors to effect a high concentration of the magnetic force by using bobbins, the necks of which had a cross-sectional area of (in some cases) only $\frac{1}{1600}$ of the cross-sectional area of the magnet cores. By this means the induction 𝔅 was raised to the following extreme values:—

	C.G.S.
In wrought iron	45,350
,, cast iron	31,760
,, Bessemer steel	39,880
,, Vickers's tool steel	35,820
,, Hadfield's manganese steel	14,790
,, nickel	21,070
,, cobalt	30,210

The induction was measured by means of a coil consisting of a single layer of very fine wire wound upon the central neck of the bobbin. Outside of this coil, at a definite distance from it, a second coil was wound, and the magnetic force was determined in the annular space between the two. In a paper communicated to the Manchester meeting of the British Association, the authors showed that if the force so measured could be proved to have the same value as the magnetic force within the metal neck itself, it would follow that the intensity of magnetism ℑ had begun to diminish under the action of excessively strong fields, in the manner which Maxwell's extension of the Weber-Ampère theory of molecular magnets anticipates. In the present paper the authors discuss at some length the question of how far the magnetic force within the metal is fairly measurable by the magnetic force in the ring of surrounding air, and they show that, with the form of cones originally used, the force within the metal must have been less than the force outside, by an amount probably sufficient to explain the apparent decrease of ℑ. The form of cone suited to give a uniform field of force with sensibly the same value in the metal neck and round it is investigated; and experiments are described in which the condition necessary for a uniform field was satisfied. The results of these experiments are conclusive in showing that no considerable change takes place in the value of ℑ (in wrought-iron) when the magnetic force is varied from about 2000 to 20,000 C.G.S. units. Throughout this range of force, the intensity of magnetism has a sensibly constant value of about 1700 C.G.S. units, which is to be accepted as the saturation value for wrought iron. The term saturation may be properly applied in speaking of the intensity of magnetism, but there appears to be no limit to the degree to which the magnetic induction may be raised.

The following are probable values of the intensity of magnetism when saturation is reached in the particular metals examined:—

	Saturation value of ℑ.
Wrought-iron	1700
Cast-iron	1240
Nickel (with 0·75 per cent. of iron)	515
Nickel (with 0·56 per cent. of iron)	400
Cobalt (with 1·66 per cent. of iron)	1300

Experiments were also made with specimens of Vickers's tool steel, and other crucible steels, Whitworth's fluid-compressed steel, Bessemer steel, Siemens steel, and Hadfield's manganese steel. This last material, which is noted for its extraordinary impermeability to magnetic induction, was found to have a constant permeability of about 1·4 throughout the range of forces applied to it—namely, from 2000 to nearly 10,000 C.G.S.

Physical Society, November 24.—Prof. Reinold, President, in the chair.—Captain Abney read a paper on the measurement of the luminosity of coloured surfaces, which was illustrated by experiments. In a communication to the Royal Society, General Festing and the author have described a method of comparing the intensity of the light of different parts of the spectrum, reflected by various pigments, with that reflected from white, and luminosity-curves have been constructed, the areas of which give comparative measures of the total luminosities. This method of comparison is accurate, but requires considerable time, and the author has devised a more rapid process. The coloured surface whose luminosity is to be compared with white is placed beside a white patch within a dark box. A direct beam of light passes through an aperture in the box, and a black rod casts a shadow on the coloured patch; another beam from the same source is reflected at an angle, and forms a shadow of the same rod on the white patch, the junction of the two shadows coinciding with that of the two surfaces to be compared. In the path of the direct beam is placed a rotating disk with angular openings, adjustable whilst rotating by a simple lever, and by this means the white patch can be made to appear too light and too dark in rapid succession. By gradually diminishing the range of oscillation of the lever, a position of equal luminosities can be found. The coloured surface is now replaced by a white one, and the adjustment again made; and from the angular apertures required in the two cases the relative luminosities are determined. Comparisons made in this way (the numbers relating to which are given in the paper) with emerald green, vermilion, French ultramarine, &c., gave results in close agreement with those deduced from the luminosity-curves obtained by the spectrum method. In reply to questions, Captain Abney said the spectrum method was the more accurate, and could be relied on to 1 per cent. The new method gave results within 2 per cent., showing that the eye is very sensitive to small changes of luminosity when such changes take place in rapid succession.—Prof. Rücker made a communication on the suppressed dimensions of physical quantities. In arranging a system of dimensional equations for thermal quantities, the question arises as to what are the dimensions of temperature. A degree of temperature, as measured by the ordinary arbitrary method of the mercurial thermometer, is not affected by changes in the units of length, mass, and time; but the numerical values of thermal quantities (J, for instance) depend on the scale of temperature adopted, say Centigrade or Fahrenheit. Two courses seem open, either of which renders a complete system of thermal dimensional equations possible: (1) temperature may be considered as a measure of energy, as in the kinetic theory of gases, and may be expressed as the energy of translation of a standard number of molecules (say that number contained in 1 cubic centimetre of air at standard pressure and temperature); or (2) temperature may be considered as a secondary fundamental unit. If the first be adopted, the dimensions of specific heat become M^{-1}, and the temperature of 0° C. is expressed by $1·5207 \times 10^6$ ergs. If a practical unit corresponding to $\frac{10^6}{18}$ ergs be adopted, this new unit of temperature will coincide with the Centigrade degree to about 1 part in 3000. The chief objection to such a definition of temperature is that the above relation between temperature and energy is not yet proved to hold for liquids and solids. If the second course be adopted, the dimensions of all thermal quantities may be expressed in terms of M, L, T, and θ, where θ is the unit of

temperature. Attention was directed to the difficulties students generally experience on finding the dimensions of the same electrical quantities to be different, according as they are expressed in electro-static or electro-magnetic measure, and that different quantities may have the same dimensions. The anomalies are shown to be due to the suppression of the dimensions of specific inductive capacity and permeability, each being called unity in air. By retaining K and μ in the dimensional equations, the author thinks that many difficulties will be avoided, the methods of transformation of units will be generalized, and the limits of our knowledge kept more clearly in view. Though the dimensions of K and μ cannot be determined, it is easily shown that those of the product $K^{\frac{1}{2}} \mu^{\frac{1}{2}}$ are $L^{-1} T$. Mr. Blakesley, in commenting on thermal units, strongly protested against the use of the word "*therm*" as a name for the unit of heat. If used at all, it should be reserved for the unit of temperature. Referring to the choice of fundamental units, he reminded the Society that the dimensions of quantities expressed in the electro-static or electro-magnetic systems become identical if the unit velocity be the velocity of light, and by choosing the unit of time as a suitable decimal part of a day, the relations between electrical and practical mechanical units could be simplified. Prof. Carey Foster, after discussing the effect of defining specific heat as a ratio, or as a quantity of heat as the dimensions of temperature, pointed out that, as quantity of heat = temperature × entropy, the dimensions of temperature would be determinate if those of entropy were found. Prof. S. P. Thompson considered that part of the difficulties of dimensional equations arose from the fact that no distinction was made between *scalar* and *vector* quantities. Thus the dimensions of work and moment of a force are given as ML^2T^{-2}, whereas the true representation for the latter would be $ML^2T^{-2}\sqrt{-1}$, because the line by which the force is multiplied is at right angles to the force. By similar considerations, Ampère's rule for the force between two parallel current elements can be derived from the magnetic equation $\frac{m \,.\, m'}{r^2} = f$, for, replacing m and m' by equivalent current elements, $i \,.\, ds$ and $i \,.\, ds'$, the equation becomes—

$$f = \frac{\sqrt{-1}\; i \,.\, ds \,.\, \sqrt{-1}\; i \,.\, ds'}{r^2}$$

$$= \frac{-i^2 ds \,.\, ds'}{r^2}.$$

Referring to the use of "therm," Dr. Thompson concurred with the remarks of Mr. Blakesley, and thought the word "calorie" answered all requirements. He also considered that thermal equations were greatly simplified by always expressing specific heat in ergs. Prof. Ayrton was of opinion that students experienced much greater difficulty in dealing with electrical units than with thermal ones, and thought part of this was due to the vague way in which some of the standard text-books treated the subject. With reference to the force exerted by quantities of electricity, Prof. Perry and himself had pointed out that specific inductive capacity must be taken into account, for, contrary to Faraday, they had found it to be different in different gases. In reply, Prof. Rücker said he often explained the identity of the dimensions of work and moment of a force, by considering moment as measured by the work done in rotating through unit angle, the dimensions of angle being zero as regards L, M, and T. He also pointed out that, in Bayne's "Thermodynamics," specific heats are always expressed in ergs. In thanking Prof. Rücker for his interesting paper, the President expressed his conviction that, by paying attention to the points considered, the difficulties arising from the two systems of units would be considerably diminished.

Chemical Society, November 15.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—The principles of thermo-chemistry, by Mr. S. U. Pickering. The author rejects the thermo-chemical principles enunciated by Thomsen, Naumann, and Berthelot, not only on special grounds, but on the more general ground that they depend on an impossible distinction between chemical and physical actions. A satisfactory explanation of all known thermo-chemical facts is derived from the recognition of the laws of dissociation and the hydrate theory of dissolution. Every act of combination must be accompanied by the evolution of heat, and in interactions where heat is absorbed this absorption must be due to the fact that, one or more of the agents being partially dissociated at the temperature of the interaction, the removal of one of the products of the dissociation necessitates a further decomposition of the agent. The heat evolved must also be a direct measure of the affinities saturated; and, of two possible interactions, that which evolves more heat must occur to the exclusion of the other. The cases of endothermic changes which present difficulties are those in which liquids and solids are concerned. The heat absorbed when many solids are dissolved in liquids cannot be explained by the fusion, but only by the volatilization of the solid. A mass of water contains some fundamental molecules possessing an energy of 10,000 cal. greater than the average molecular aggregates constituting the mass. These can therefore combine with the salt, and effect its volatilization with an evolution of heat, even if the heat of volatilization be nearly 10,000 cal.; other water aggregates then dissociate to supply the place of the free molecules thus removed from the sphere of action. From theoretical considerations the author arrives at the conclusion that Berthollet's theory as to the division of a base between two acids is correct, and argues that the facts observed are in accordance with these conclusions, and are entirely opposed to the existence of the so-called "avidity" or "affinity" constants advocated by Ostwald and others. In the discussion which followed the reading of the paper, Prof. Ramsay, F.R.S., said that he did not believe in the universal presence of complex molecules in liquids and solids, nor did he exclude the existence of such; the researches of Prof. Young and himself, he thought, conclusively established the absence of a complex molecular structure in such liquids as ethyl alcohol and ether, whilst, on the other hand, Henry's arguments testified to the complexity of the molecules of certain oxides, such as silica. With regard to water, which specially formed the subject of Mr. Pickering's remarks, it was to be noted that, whilst the vapour-density pointed to molecular simplicity, other arguments drawn from its behaviour when examined by Raoult's method were in favour of moderate molecular complexity. Prof. Armstrong, F.R.S., remarked that by taking into account the action of water, Mr. Pickering had advanced what appeared to be a rational explanation of many facts which hitherto had appeared paradoxical.—Note on the mixture of propyl alcohol and water, by Prof. Ramsay, F.R.S., and Prof. Young. The authors have determined the vapour-pressures of a mixture of propyl alcohol and water in the proportions $C_3H_8O : H_2O$, and like Konowalow, arrive at results adverse to the conclusion that a definite hydrate exists. Chancel found that this mixture distils over to the last drop at 87°·5 under 738 mm. pressure, but the authors find that the composition of the mixture of constant boiling-point varies with the pressure under which distillation takes place.—Note on the action of nitric acid on ammonium chloride, by Dr. F. E. Matthews. The principal gaseous product of the action of nitric acid on ammonium chloride in solution is nitrous oxide, and not nitrogen, as has been previously stated; the gas is mixed with small quantities of chlorine and oxychloride of nitrogen.—Ethylic cinnamyldiethacetate, by the same.—The isomeric sulphonic acids of β-naphthylamine, by Mr. A. G. Green. Three acids—the α-, β-, and γ-acids—are known to be formed when β-naphthylamine is sulphonated with ordinary sulphuric acid at 100°, but the author finds, as was to be expected, that the δ-acid is also present. The analogous behaviour of hydroxy- and amido-compounds makes it probable that β-naphthol on sulphonation gives four isomeric sulphonic acids, although two only have hitherto been isolated, and the author's experiments confirm this view, inasmuch as he has succeeded in isolating a third acid—corresponding to the β-naphthylamine-δ-sulphonic acid—from the product formed on sulphonating β-naphthol at 100°. In the discussion which followed, Prof. Armstrong, F.R.S., and Mr. Wynne pointed out that the formula adopted by Mr. Green as representing the constitution of the β-naphthylamine-α-sulphonic acid was at variance with the views put forward by Cleve and others, and could not be accepted; Mr. Green, in reply, defending his view that the α-acid is an ortho-compound, mainly on the ground that it and the corresponding β-naphtholsulphonic acid differed so greatly in properties from their isomerides.—The constitution of the dichloronaphthalenes, especially the $\alpha\beta$-compounds, by Prof. Armstrong, F.R.S., and Mr. W. P. Wynne. The three possible $\alpha\alpha$- and the two possible heteronucleal $\beta\beta$-dichloronaphthalenes are known, and formulæ have been ascribed to them which almost certainly are correct expressions of their constitutions. The authors point out that the four possible $\alpha\beta$-dichloronaphthalenes are also known, and draw attention to the somewhat discrepant statements on record relating to the so-

called θ-modification, melting at about 61°. The authors have found (Brit. Assoc. Report, 1887, p. 231) that under this designation two distinct dichloronaphthalenes have been regarded as one, and now bring forward evidence showing that one of these, melting at 61°·5, is a homonucleal, and the second, melting at 64, is a heteronucleal $\alpha\beta$-derivative. They confirm Cleve's view that the dichloronaphthalene, melting at 34°, is a homonucleal $\alpha\beta$-compound, the dichloronaphthalene melting at 48° being the remaining heteronucleal $\alpha\beta$-derivative. With regard to the constitution of the two homonucleal $\alpha\beta$-dichloronaphthalenes, the authors show that that melting at 61°·5 must be the *meta*-compound (that melting at 34° being, by exclusion, the ortho-derivative), since their experiments prove that so-called α-dichloronaphthalene, melting at 38°, and obtained by treating naphthalene tetrachloride with alcoholic potash, is a mixture of two homonucleal dichloronaphthalenes—namely, the $\alpha\beta$-derivative melting at 61°·5, and the $\alpha\alpha$-derivative melting at 68°. Sufficient data have not yet been obtained to determine the constitution of the heteronucleal $\alpha\beta$-dichloronaphthalenes.—Piazine derivatives, by Dr. A. T. Mason. A continuation of the author's work on a class of compounds formerly known as ketines, and more recently as pyrazines.

Anthropological Institute, November 27.—Francis Galton, F.R.S., President, in the chair.—The President exhibited a gold breastplate from an ancient Peruvian grave.—Mr. F. W. Rudler exhibited a collection of ethnological objects from the Jivaros of the Upper Amazons, and the Arawaks and the Acaways of the interior of British Guiana.—Mr. G. F. Lawrence exhibited two Palæolithic implements from the valley of the Thames, near Erith.—Mr. Osbert H. Howarth read a paper on the survival of corporal penance, and exhibited specimens of the "*disciplinas*," or scourges, which are still used, in public penance, in the village of Fenães d'Ajuda, a remote community on the north coast of St. Michael's, Azores.—The Secretary read a paper by the Rev. Benjamin Danks on marriage customs of the New Britain Group.

PARIS.

Academy of Sciences, December 3.—M. Daubrée in the chair.—Observations of the minor planets made with the great meridian instrument of the Paris Observatory during the first half of the year 1888, by M. Mouchez. The right ascension and polar distance, with correction of ephemerides, are tabulated for Diana, Danaë, Athamantis, Astrea, Parthenope, Flora, Sappho, Hebe, Cyrene, Germania, and five other minor planets.—On the satellites of Mars, by M. H. Poincaré. The paper deals with M. Dubois's recent hypothesis (*Comptes rendus*, August 20, 1888), that Phobos and Deimos were originally small planets, which a few years ago passed within the attraction of Mars. This hypothesis, which is based on the fact that the two satellites were never observed before 1877, is shown to be inadmissible by a consideration of the eccentricity of the orbit of Mars, and on other grounds. Although the eccentricity of Mars is about six times greater than that of the earth, it can be demonstrated that the elements of its moons cannot have perceptibly varied during the last hundred years.—On the preparation of the phosphorescent sulphides of calcium and strontium, by M. Edmond Becquerel. The author, who has lately resumed the study of these sulphides, now finds that some of the added substances, when employed alone, fail to produce any appreciable effect, and that the simultaneous presence of several is sometimes necessary for the preparation of strongly luminous bodies. The modifications depend not only on the nature of the mixed substances, but also on that of the phosphorescent sulphide itself.—On the invariants of differential equations, by M. E. Goursat. Since M. Halphen's researches on linear differential equations, M. Appell and others have extended the notion of invariants to differential equations of a more or less general form for certain special categories of transformations, without, however, determining in a general way the existence of these invariants. The determination is here effected by a demonstration based on Herr Lie's *Theorie der Transformationsgruppen*, (Leipzig, 1888).—On the dark waters of the equatorial regions, by MM. A. Muntz and V. Marcano. For the purpose of ascertaining the cause of the dark colour so characteristic of numerous affluents of the Amazons and Orinoco, the authors have analyzed some specimens from the upper course of the latter river. They conclude that the discoloration is due to the free humic acids held in solution, and derived from the decomposition of vegetable matter on a granite soil free from lime. The liquid is thus in some respects of the nature of bog-water, and the colour persists because, in the absence of lime, the phenomena of nitrification, and consequently the combustion of the organic matter, cannot take place, as shown by the complete absence of nitrates. The waters themselves are perfectly limpid, wholesome, and palatable, for although the discoloration is primarily due to their chemical composition, its intensity must be attributed to phenomena of reflection produced by the great depth of the liquid masses.—On the benzoic acetals of mannite and its homologues; decomposing action of benzoic aldehyde, by M. J. Meunier. The acetal of mannite is readily obtained by dissolving it in hydrochloric or sulphuric acid, adding a corresponding quantity of benzoic aldehyde and shaking; the mixture is rapidly transformed to acetal and solidifies. When a benzoic acetal, and doubtless others also, is completely freed from the excess of aldehyde, it resists the action of the acids as well as of the alkalies, and is not decomposed by prolonged boiling with acidulated water. In the presence of the aldehyde, on the contrary, decomposition takes place very rapidly by boiling, and all the more rapidly the greater the excess of aldehyde, even if the liquid be but slightly acidulated, containing, for instance, not more than 1 per cent. of sulphuric acid. Benzoic aldehyde thus influences the hydration of the acetal and the consecutive formation of mannite.—Action of the sulphide of carbon on clays; production of the oxysulphide of carbon, by M. Armand Gautier. During his researches on the mineralizing elements of thermal waters, the author has been led to attempt the synthesis of the oxysulphide of carbon by causing the vapours of the sulphide of carbon to act at red heat on the natural silicates, and especially on the argillaceous earths. These essays have been successful, and a method is here described which alone can furnish the oxysulphide of carbon, COS, in a pure state and in large quantities.—Transformation of terpilene into a menthene, by MM. G. Bouchardat and J. Lafont. By exposing terpine, $C_{20}H_{16},2H_2O_2$, for fifteen hours at 100° C. to twenty times its weight of aqueous hydriodic acid saturated at 0°, the authors have produced a dihydriodate of crystallized terpilene, $C_{20}H_{16},2HI$, identical with the dihydriodate of the essence of terebinthine. From their further researches they conclude that natural menthol should perhaps be grouped with the terpilene series.—On a spermaceti whale taken in the Azore waters, by Prince Albert of Monaco. Photographs are given of the head of a large spermaceti whale harpooned last summer in the neighbourhood of the Azores. It measured from the eye to the upper extremity of the mouth 1·90 metre, and from the under jaw to the lip 1·16 metre.—Papers are contributed by M. G. Saint-Remy on the brain of the spider family; and by M. A. Giard on *Peroderma cylindricum*, Heller, a parasite of the sardine.

BERLIN.

Physical Society, November 16.—Prof. du Bois-Reymond, President, in the chair.—Prof. von Bezold made a further communication on the thermo-dynamics of the atmosphere, in continuation of a statement made to the Society earlier in the year. After briefly recapitulating the processes which occur during the adiabatic expansion of a mass of air as it rises, he introduced into thermo-dynamic considerations a new idea, brought forward by Helmholtz, and found to be extremely convenient. The idea is that of "potential temperature," or in other words the absolute temperature assumed by a mass of air when it comes adiabatically under normal pressure. The speaker then propounded the following as a general law: "Whenever a mass of air changes its condition adiabatically, the potential temperature is never diminished, it is usually increased, and sometimes is unchanged." This law was proved from a number of examples. During the adiabatic alterations of pressure and volume in the currents of air as they rise and fall, the temperature should fall, on the average, about 1° C. for a height of 100 metres; as a matter of fact, the fall is really less than 1° C. This is due to the fact that under natural conditions the processes do not occur adiabatically, since near the earth's surface and above the level of the clouds warming and cooling influences are brought to bear on the air. In an anticyclone the powerful radiation from the earth leads to a cooling of the lower strata of air, and to this is due the fall of temperature observed at all stations which are situated on a height, a phenomenon which, according to the speaker, must also make its appearance at lower levels during maximal pressures of the air in winter and during the night. In cyclones the fall of temperature with increasing altitude similarly differs from its theoretical value, since warm air from the neighbouring anti-

cyclone becomes mixed with the colder air as it is rising, owing to the whirling motion: as a result of this, the formation of clouds must be most dense in the centre of the cyclone, and thinner towards its periphery. The latent heat liberated during the condensation accompanying cloud-formation is only obvious in the anticyclone, since it merely slows the rate of cooling in the rising current of air; on the other hand, the cold rain-drops as they fall cool the lower layers of air in a cyclone, so that as a result of the above a mixed convection of heat takes place from the cylone in the direction of the anticyclone. These thermo-dynamic considerations explain in general a large number of meteorological phenomena of which the speaker was only able to enumerate a few.—Dr. Budde made some remarks in connection with Janssen's communication to the last meeting of the British Association on the double spectrum of oxygen, of which one is proportional to the density of the gas, the other to the square of that density. He showed that on the supposition that one of the spectra is due to separate free molecules, the other to molecules which are impacting, the result must follow which Janssen has found experimentally.

Physiological Society, November 23.—Prof. du Bois-Reymond, President, in the chair.—Prof. Moebius spoke on the nests which are constructed by the marine stickleback (*Gasterosteus*). As early as 1829 the fact that this animal constructs a nest was described by an English observer. The speaker had had frequent opportunities of examining these nests in the Baltic, and found that they are constituted not only out of Fuci, Algæ, and other marine plants, but also out of the leaves of terrestrial plants which have fallen into the water, and even sometimes out of bits of wool. The male, who is constantly circling round the nest, knows how to find it again, even if it is lifted and lowered again into the water at a distance of five hundred paces from its first position. In an aquarium the speaker was able to observe that the male is continually spinning new fibres round the nest which proceed from out of the urinary bladder. The fibres are, as shown by chemical reactions, composed of mucin, which is not, however, secreted in the bladder, but by the kidneys. Sections through a kidney, treated with osmic acid and stained with hæmatoxylin, showed that only a few of the cells lining the uriniferous tubes are concerned in the elaboration of mucin, the others undergoing no such change. Out of the breeding-season none of these mucigenous cells are to be found in the kidneys, which are then less swollen. A case analogous to the above, of nests constructed of mucin derived from temporarily modified gland-cells, is found in Salangane, which produce the edible nests; these birds make use of a glutinous material for the construction of their nests, which is at times secreted by a gland, in this case the salivary gland. A comparative physiological-chemical analysis of these two secretions would be very interesting.—Prof. Munk gave an account of his researches on the physiology of the thyroid gland. It has long been known that in cases of excision of this gland in man the patients suffer from severe cachexia, to which they speedily succumb, with symptoms indicative of serious disease of the central nervous system; this fact has led to a long series of physiological researches, from which it appears that this small organ is of the greatest importance to life. It was assumed, in accordance with Schiff's views, either that it produces some substance which, passing into the blood, upsets the normal function of the central nervous system, or that it is concerned in the destruction of some injurious products of cerebral activity. Two years ago, as the speaker began his researches on the physiology of this gland, with a view to the discovery of the above remarkable substance, he observed solitary cases in which the dogs were only slightly ill, and then completely recovered, notwithstanding that the thyroid was completely extirpated; one dog showed no signs of any illness at all. Similarly in the literature of this subject, solitary cases are mentioned in which extirpation had no effect on the dog's health. The speaker had next changed his method of operating, merely isolating the gland from the surrounding structures, ligaturing the hilus, and replacing the isolated lobes in their original position. Some of the dogs with the gland thus isolated lived on in perfect health; in these the gland was found to have degenerated and become completely converted into connective-tissue. Others of the dogs became ill and died, and in these the gland had healed and recovered its vascular supply. From these experiments it followed that the thyroid is not an organ of absolute importance for life, inasmuch as animals can live in perfect health without it. It thus remained to determine what is the cause of the serious pathological condition and ultimate death which ensues when the thyroid is excised in man and other animals. A careful study of the symptoms showed that the normal functions of respiration, cardiac activity and nutrition, and of the nervous system, are upset, resulting in dyspnœa with powerful expirations, palpitation of the heart, relaxation of the arteries, derangement of the movements of deglutition, accompanied by vomiting, clonic and tonic cramps, resulting in epileptic attacks. It was further found that the dyspnœa and palpitation are primary symptoms, the cramps are secondary, and that death ensues during the latter. The dyspnœic attacks with the resultant conditions are undoubtedly due to the stimulation of nerves lying in the inflamed tissues after the extirpation of the gland, viz. the superior laryngeal, recurrent laryngeal, vagus, and sympathetic nerves. This is clearly shown by the fact that when the gland is simply isolated by a ligature the dogs live in good health, the gland at the same time degenerating, whereas in cases where the surrounding tissues inflame and lead to a renewed adhesion and vascularity of this organ the dogs became ill and died. The speaker was obliged to defer to the next meeting, owing to the lateness of the hour, the further description of his experiments, and of the conclusions to be drawn from them.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Mining Manual for 1888: W. R. Skinner.—The Speaking Parrots, parts 7 and 8: Dr. K. Russ (L. U. Gill).—British Dogs, Nos. 25 and 26: H. Dalziel (L. U. Gill).—A Text-book of Elementary Metallurgy: A. H. Hiorns (Macmillan).—Mental Evolution in Man: G. J. Romanes (Kegan Paul).—Die Stämme des Thierreiches (Wirbellose Thiere), vol. ii.: M. Neumayr (Temsky, Wien).—Essai d'une Théorie Rationnelle des Sociétés de Secours Mutuels: P. de Lafitte (Paris, Gauthier-Villars).—Cours d'Astronomie Pratique; Application à la Géographie et à la Navigation, 2me partie: E. Caspari (Paris, Gauthier-Villars).—The Agreement of Colour Theories with Practical Experience: G. H. Morton (Liverpool).—The Proposed Chemical Laboratory at the University of Sydney: A. Liversidge (Sydney).—Journal of the Chemical Society, December (Gurney and Jackson).—Proceedings of the Linnean Society of New South Wales, vol. iii. part 1 (Trübner).

CONTENTS.

THURSDAY, DECEMBER 20, 1888.

THE "ENCYCLOPÆDIA BRITANNICA."

The Encyclopædia Britannica. Vol. XXIV. (Edinburgh: Adam and Charles Black, 1888.)

THE assertion attributed by the undergraduate to the Master of his College, "What I know not is not knowledge," might be made in sober earnest on behalf of the authors of the ninth edition of the "Encyclopædia Britannica" in their corporate capacity. Their task has been to compile a compendious summary of all that is best worth knowing; to set up a landmark which should indicate the point to which we have now attained, which should distinguish between the uncertain and the sure, between hypothesis and fact.

To affirm that they have in all respects succeeded would be to assume an omniscience from which even the boldest critic might shrink, but it is doing them bare justice to say that it is generally held by those most competent to judge that their work is worthy of themselves. Art and science, history and literature, everything from the cedar on Lebanon to the hyssop on the wall, are included, and article after article bears in the initials at its end the hall-mark which stamps it as a work of the highest authority.

The "Encyclopædia," therefore, is not a mere compilation. Many of the scientific articles, though avoiding the mistake of giving undue prominence to opinions specially associated with the authors' names, are evidently the product of minds capable of looking forward as well as around. They are not content with producing the stock evidence in favour of generally-accepted theories; they know their weak as well as their strong points. They tell the reader not only what has been done, but something of what there is yet to do.

As examples, and in choosing them we confine ourselves to writers who, though they shared in the work, have unhappily, and to the great loss of science, not lived to see its completion, we may refer first to Prof. Clerk Maxwell's contributions on molecular physics. His article on "Atom" has become famous. Under the head of "Capillary Attraction," he gave (in spite of a few slips which have been pointed out by Sir William Thomson) a fuller and more suggestive discussion of the theory of capillarity than is to be found in any other English treatise. Turning to other subjects, no higher authority on the microscope could have been found than the late Dr. Carpenter. The article on "Terrestrial Magnetism," by Prof. Balfour Stewart, is a masterly synopsis both of the present state of knowledge on this subject, and of the directions in which inquiry should be prosecuted.

It is not, however, our intention either to attempt to give a general outline of the scientific articles or to criticize those in the concluding volume. At the moment when the task is just completed, we would rather congratulate the editor, authors, and publishers, on a work in which they may fairly take an honest pride. The examples we have cited will suffice to prove to our readers that anyone who has access to a good public library may now find in the "Encyclopædia Britannica" a review of what is known on almost every scientific subject, together with references which are sufficient to direct him if he wishes to pursue it further for himself.

Of course we do not mean to assert that articles in the earlier volumes—some fourteen years old—are always up to date now. But in spite of this drawback it is no slight advantage to have a succinct account of the state of knowledge at a definite and not very distant epoch. No doubt, editor and publishers have gained much valuable experience during the progress of the work, and perhaps they will be able to pigeon-hole a scheme by which the tenth edition will be more rapidly issued. We live quickly now, and though fourteen years was at one time considered a not unreasonable probation for an expectant swain, it seems long to a modern subscriber who is looking for the colophon.

Now that the end has come, the work may be regarded by Englishmen—or rather, if our Scotch friends insist on regarding that word as excluding them, by Britons—with just pride. Its completion was celebrated, in accordance with our national custom, by a dinner, of which we give some account elsewhere. In the course of an admirable speech, which he then delivered, Mr. Adam Black referred to the circulation of the ninth, as compared with that of the eighth, edition. It appears that while five thousand copies of the eighth edition were sold, the circulation of the ninth has been ten times as great. No doubt this is due in part to the demand for the work in the United States, but we may also assume that there has been a largely increased demand in England. The fact deserves to be specially recorded as a very striking sign of the times. It affords remarkable proof that during the lifetime of a generation there has been a steady growth not only of general intelligence, but of an enlightened desire to seek for information on all important subjects at the best and most trustworthy sources.

In these days of specialism, it is well that those engaged in different pursuits should, in one task at all events, meet on common ground. In educational matters they are too often opponents, struggling for the prominence of their particular subjects, offering rival inducements to the ablest scholars. Round the table in Christ's College, last week, these differences disappeared. The old learning and the new shared a triumph together. Every man who could tell, better than they, something of real interest to his fellows was recognized as having a claim on their attention.

In the company of encyclopædists, however, though due attention is given to each, the amount due is measured with the most scrupulous care. If sometimes we despair of the future when we read the endless babble of the platform, we may take courage from the study of pages in which the description of fact and the expression of thought are reduced to their utmost concentration. There is still hope for a race which, though it is producing "Hansard," has produced also the ninth edition of the "Encyclopædia Britannica."

Memories of some of those who have been left by the way cast over such a meeting a sobering but not necessarily a saddening influence. The "Encyclopædia" is itself a proof that we are growing in knowledge which can be put to good account to make the lives of succeeding generations less toilsome and more elevated than they

would otherwise have been. Lives which have been spent in the effort to secure this knowledge have not been lived in vain. To aid in securing that the tenth edition of the "Encyclopædia Britannica" shall mark an advance in our mastery over Nature comparable with that which is chronicled in the ninth there are still those among us who "would even dare to die."

MEDIÆVAL RESEARCHES FROM EASTERN ASIATIC SOURCES.

Mediæval Researches from Eastern Asiatic Sources. Fragments towards the Knowledge of the Geography and History of Central and Western Asia from the Thirteenth to the Seventeenth Century. By E. Bretschneider, M.D. (London: Trübner and Co., 1888.)

FOR some years past, owing mainly to the labours of Colonel Yule, European students have been made acquainted with the travels of European explorers of the Middle Ages in Central Asia and China. In "Cathay and the Way Thither," published in 1866 by the Hakluyt Society, and especially in his monumental edition of 1875 of the travels of Marco Polo, Colonel Yule laid before the world a record of practically all that had been done by mediæval travellers from Europe in these regions. Dr. Bretschneider's work is of the same nature, inasmuch as it deals with explorations of the same period in the same regions, but with this exception—his travellers are Chinese and start from China, Colonel Yule's are European and start from Europe. The former goes to Chinese literature as his storehouse, the latter to European literature. Each is complementary to the other; and, indeed, Dr. Bretschneider acknowledges that it was Colonel Yule's works that led him to study and collect the materials supplied by Chinese literature regarding the mediæval history and geography of Central Asia. He found that such quotations from the works of Chinese travellers as had made their way to Europe were not always carefully or faithfully translated; and as his position of physician to the Russian Legation at Pekin gave him peculiar opportunities of study, and placed at his disposal the valuable and rare library of Chinese works collected over a long series of years, at the expense of the Russian Government, by the Russian Ecclesiastical Mission, he determined to investigate the subject at first hand for himself. The result was the publication, in the pages of the Transactions of the North China Branch of the Royal Asiatic Society between 1874 and 1876, of a series of papers dealing with Chinese knowledge of Central Asia, and Chinese travellers there from about 1200 to 1600. Three of these papers are collected in the volumes before us, and form, as it were, the backbone of the work, viz. "Notes on Chinese Mediæval Travellers to the West"; "Notices of the Mediæval Geography and History of Central and Western Asia"; and "Chinese Intercourse with the Countries of Central and Western Asia during the Fifteenth Century." The new edition is brought up to date by references to the results of recent researches and investigations of Russian and other travellers, and especially to the vast increase in our knowledge of these regions produced by the rapid extension of Russian territory in the direction of India and China.

Dr. Bretschneider tells us that Chinese literature contains very considerable accounts of the geography of Asia at different times, and of the nations which formerly inhabited that part of the ancient world. These are mostly to be found in the histories of the various dynasties which have successively ruled China. At the end of each of the twenty-four dynastic histories, a section is devoted to the foreign countries and nations which came in contact with the Chinese Empire. These were probably collected by Chinese envoys, or compiled from the reports of envoys or merchants coming from those countries. Another category is drawn up in the form of narratives of journeys undertaken by Chinese. They never travelled, it seems, for pleasure, or to enlarge their sphere of knowledge. We owe all their narratives of travel either to military expeditions, or official missions, or pilgrimages to places famed for their sanctity. The number of these reports is not inconsiderable; but the difficulty of searching them out is great, as they do not, as a rule, exist as separate publications, but lie concealed amongst collections of reprints; and many of them have been wholly lost, their existence at one time being known only from ancient catalogues, or quotations in books which have survived. The difficulties of elucidation also are very great, for even when translated they require a vast number of explanations. This will be understood when we mention that, besides prefaces, introductions, explanations in the text, &c., there are 1188 footnotes, some of them running over several pages, in these two volumes, containing altogether rather less than 700 pages.

The first paper, entitled "Notes on Chinese Mediæval Travellers to the West," is confined to the thirteenth century, "the period of the development and the zenith of the power of the Mongols in Asia," and the earliest journey recorded in it is the itinerary of Chinghiz Khan's army from Mongolia through Central Asia to Persia in 1219. This is followed by the record of the journey of an envoy of the Emperor of North China, sent in 1220 to Persia, and as far as the Hindu-Kush Mountains, to meet Chinghiz Khan. The third journey recorded is that of a monk, who travelled, by order of the great conqueror, from China to Samarkand. He left Shantung, in the extreme east of China in 1220, went by way of Pekin, crossed the eastern part of Mongolia, probably passed near the modern Uliassutai, traversed the Chinese Altai Mountains, near the present Urumtsi, and along the northern slope of the Thian-Shan Range to Lake Sairam, whence he descended into Ili, went through Tashkend, crossed the Syr-Daria into Samarkand, and thence went southwards to Balkh, and on to Cabul. He returned by the same route, except that he made a shorter cut across the Mongolian desert; and arrived at Pekin in 1224. Such a journey performed either way to-day would probably make the traveller the geographical hero of the year, and it is recorded that, when he entered Pekin on his return, "venerable old men, men and women, assembled from all sides, and accompanied the master (the traveller) with fragrant flowers, and bowing before him obstructed the road." The fourth traveller started from Mongolia, and going by Samarkand, went westward to the Elburz Range, and the country where the Mulahi or Assassins lived; and the fifth was a Mongol officer who wandered about Central Asia between 1260 and 1262. The records of these various journeys are full of the most

interesting details about the countries and people visited, told sometimes in a very quaint and amusing manner. The task of following their routes and identifying the places is appalling; but Dr. Bretschneider goes through it all, balancing theories, and comparing modern descriptions of the same places, with untiring patience and ever-ready learning.

The second paper is entitled "Notices of the Mediæval Geography and History of Central and Western Asia," drawn from Chinese and Mongol writings, and compared with the observations of Western authors in the Middle Ages. These also refer to the period of the Mongol supremacy in Asia, and are mainly drawn from records of warlike expeditions of the Mongols to the West in the first forty years of the thirteenth century. These are preceded by bibliographical notices of the Chinese, Mongol, Arabic, and other books used, an historical and ethnographical sketch of the Khitan, Karakhitai, and Uigur peoples, and, more interesting still, a discussion on the information of the Chinese at the same period about the Mohammedans.

The second volume opens with a curious specimen of mediæval cartography, a rude Mongol-Chinese map published in the first half of the fourteenth century; and about 140 pages of the volume are occupied with identifications of the places mentioned on the map. The last paper contains an account, also from Chinese sources, of their intercourse with the countries of Central and Western Asia during the fifteenth and sixteenth centuries. In this we are given the description by Chinese writers of over fifty tribes and peoples of the West, including Portuguese, Spaniards, and Dutch, as far as they were known at that period in China. A most interesting sketch of the early Jesuit missionaries in China is found under the head Italy, in which the struggles of the Jesuits to retain permission to reside at Pekin, the intrigues against them, and their success because of their scientific attainments, are all described. From this record it appears that in the sixteenth and beginning of the seventeenth century a considerable number of Jesuit fathers resided at Pekin, some of them holding office about the Emperor's Court, and that all died in China after a long residence. Ricci himself, the senior and predecessor of them all, lived in China twenty-eight years, Longobardi fifty-seven years, Emmanuel Diaz forty-nine years, and so on.

We cannot conscientiously say that the book is one for the general reader: its long notes, Chinese names in italics, and other outward and visible signs of learning will warn off all light-minded persons. But to the student of the geography and ethnology of Asia it is an indispensable aid, for it contains almost all that is at the disposal of those unacquainted with the Chinese language, of the observations and experience of Chinese travellers in Central Asia between the thirteenth and sixteenth centuries. We say "almost," because, since Dr. Bretschneider's papers were first published, Dr. Hirth has worked the same mine in his "China and the Roman Orient," published a few years ago, and the discussions which have arisen amongst Chinese scholars in consequence of this book have added much to our knowledge of Chinese literature relating to Central and Western Asia.

THE ORIGIN OF FLORAL STRUCTURES.

The Origin of Floral Structures. By the Rev. George Henslow, M.A., F.L.S. (London: Kegan Paul and Co., 1888.)

PROFESSOR HENSLOW'S book on the origin of floral structures tends to supply a want in botanical literature. It has the merit of being the first popular work which deals extensively with the morphology and development of the flower, and introduces to the English reader the work of Payer, Van Tieghem, and Baillon, besides further popularizing the exquisite researches of Darwin and Müller concerning the process of fertilization of plants.

The early chapters, which deal with the anatomy of the flower, though containing little original matter, present a good general view of floral anatomy and structure. The position of the various floral organs upon their axis having been deduced from the similar position and arrangement of leaves upon a vegetative shoot, we shortly come to the first of the author's main points, viz. the possibility of elucidating floral structures by an examination of the relative positions of the vascular bundles, or, as the author prefers to call them, "foliar cords." This idea is by no means new, and we venture to think that the author has not done sufficient justice to extant literature. It is, moreover, a great pity that the new expression cord has been substituted for the well-known vascular bundle, since there appears little or no need for it. In our opinion far too much stress is laid upon the position and distribution of the vascular bundles, as if the vascular bundles in every case determined the number and position of the various members of the flower and were not rather subservient to them, as certainly appears to be the case in many irregular flowers. In the discussion on the relative positions of the stamens and so-called petals of the Ranunculaceæ, Prof. Henslow has apparently not seen that Prantl has lately shown the so-called petals to be staminodes.

The second part of the work deals with the forms of flowers, and all the varied phenomena associated with fertilization. Prof. Henslow lays particular stress upon the theory that the shape of the flower as a whole, and also that of the various floral appendages, are definitely associated with, and bear relation to, the particular insects which fertilize them, and the further elaboration of this exceedingly probable hypothesis is the second main point to which he pays especial regard.

Nectaries—floral and extra-floral—he considers to have been brought into existence, equally with the rest of the floral appendages, through insect agency. Starting with a review of the cases of irritability and response to stimulus which so often occur in plant life, he further points out how frequently pathological growths, such as galls and the like, are formed by the irritation set up by insects, and argues that it is exceedingly probable that in the case of nectaries the perpetual irritation of particular localities by insects in search of the sweet juices which are present in the floral tissues, may have induced the formation of a definite glandular outgrowth, secreting nectar. This hypothesis is certainly ingenious, and even at the present time is not altogether without support. In the present state of the science it would, however, be premature to accept it without further and strong proof. Prof.

Henslow has no reference to Beccari's remarks which appeared in 1884 in the second volume of "Malesia" under the head of "Piante ospitatrice." In the preface to the descriptions of his exceedingly beautiful and well-known myrmecophilous plants, Beccari puts forward the very view taken by Prof. Henslow, both with regard to floral and extra-floral nectaries, so that Prof. Henslow has no need "to venture to go further" (*i.e.* than Beccari), and attribute the large honey-pits at the base of the leaf-stalk of *Acacia sphærocephala*—see p. 157—to the mechanical irritation of ants.

The book closes with some remarks on the origin of species and the origin of flowers. There is evidence that the author has not thoroughly acquainted himself with some of the literature to which he refers, and in certain instances important references are omitted altogether.

THE CORAL REEFS OF THE PENINSULA OF SINAI.

Die Korallenriffe der Sinaihalbinsel, geologische und biologische Beobachtungen. Von Johannes Walther, Dr. Phil., und Privat-docent an der Universität Jena. Des xix. Bandes der *Abhandlungen der Mathematisch-physischen Classe der Königl. Sächsischen Gesellschaft der Wissenschaften.* (Leipzig: bei S. Hirzel, 1888.)

MUCH has been written and said of late on the origin of coral reefs; yet the best authorities, when they have not theories of their own to uphold, are agreed in thinking that the matter is far from being finally settled. For this reason a thorough examination of all coral districts is much needed, and every work which adds to the general stock of knowledge on the subject deserves attention. The present memoir deals with the geology of the peninsula of Sinai, and the dependence of the coral reefs in the Gulfs of Suez and Akabah on the characters of the rocks forming the shores. Herr Walther has undertaken difficult and disagreeable, if not dangerous, journeys in the course of his research, and in point of thoroughness his observations leave little to be desired. Believing that a solution of the question in any given area can only be obtained by carefully studying the relations of the reefs to their basis, he has thoroughly examined the geological character of the western mountains of the peninsula, and gives in the first part of the book a full account of all that he observed. The results of his geological survey are most conveniently studied in the plate giving a series of sections through the peninsula. These show that south of Uâdi Firan there are two parallel lines of granite mountains, running north-west and south-east, and between them lies a basin filled in with sedimentary rocks. As far south as Gebêl Nakûs the granite forms the shore, and the author points out that in this region there is no fringing reef and no coral of any kind. Further south, where the sedimentary rocks form the sea-cliff, the fringing reef makes its appearance, sending out offsets from the shore from place to place, which form barrier reefs and even atolls. The shores of the Gulf of Akabah are granitic, and are devoid of coral reefs. Commenting on this, the author explains that the granite is rapidly weathered out, and that its surface thus constantly undergoing destruction does not afford a sufficiently firm basis for coral growth.

The coral reefs are divided into living reefs, sub-fossil reefs, and ancient reefs. The first are the fringing and barrier reefs or atolls actually being formed beneath the sea-level; the second are upheaved reefs, lying just above the sea-level, and consisting of coral heads cemented together; the third are infrequent, and consist of masses of dolomitic limestone, the structure of which betrays its coral origin, lying 230 metres above the sea. The thicknesses of these reefs were accurately determined, and were found to be, for the ancient reef, 15–17 metres; for the sub-fossil reef, 3–5 metres; and for the fringing reef, 3 metres. These facts are by far the most important part of the author's work; they prove that considerable changes of level have taken place since the coral reefs were first formed, and that these changes have been in the direction of elevation. Thus another instance is added to the many now accumulating of barrier reefs and atolls being formed in an area of elevation. The slight thickness of the reefs also deserves attention. At the end of the book the author speaks of a reflux of the sea having occurred rather than an upheaval of the land. As he does not explain what he means by a reflux of the sea, his statements are rather puzzling. Does he hold the view that considerable changes of sea-level have occurred as consequences of glaciation at either pole? In any case, the phenomenon which he seeks to account for by an alternating level of the sea, viz. the existence of a dead reef below the sea-level and beneath the living fringing-reef at Râs Muhámmed, requires for its explanation nothing more than a period of subsidence following on a period of elevation; and several of the geological facts seem to point to a recent though slight subsidence at the southern end of the peninsula.

The author adds nothing to our knowledge of the biological conditions and the composition of coral reefs. His accounts of the living coral and its mode of growth, of the filling up of the interstices of dead coral blocks with detritus, and the formation of oolitic granules, are familiar to all visitors to coral lands, and have been fully described by previous authors. Although an unnecessary amount of space is devoted to the description of these well-known phenomena, the whole work demands the attention of geologists and of students of coral formations. The numerous plates and woodcuts render the text light and easily comprehensible, and the map showing the condition of the coral reefs at different geological periods is of especial interest.

G. C. B.

OUR BOOK SHELF.

The Book of the Lantern. By T. C. Hepworth, F.C.S. (London: Wyman and Sons, 1888).

THE lantern has of late years become such an important aid to almost every branch of education, even in theological and political matters, that no apology is needed for the publication of a thoroughly practical treatise on everything connected with it. As a former lecturer at the Royal Polytechnic Institution, and present lecturer at the Birkbeck, Mr. Hepworth has gained the practical experience the benefits of which he now places at the disposal of others.

After brief reference to the history of the lantern, the optical arrangements are considered, and these are

followed by instructions for the preparation and storage of oxygen, which is now so commonly employed in conjunction with hydrogen, or ordinary gas, for illuminating purposes. The preparation of slides of every description, micro-photography, and the process of making lantern enlargements, are all fully considered. One chapter is also devoted to the description of a few simple scientific experiments, which can be easily performed whilst projected on the screen. Finally, a few valuable hints are given to aspiring lecturers or entertainers who wish to avail themselves of the powers of the lantern.

The necessary references to firms which supply particular appliances have been made without any partiality. The addresses of such firms might have been given with advantage.

The book is full of practical hints from beginning to end. It is very readable, and we can confidently recommend it to all who are concerned with lantern matters in any shape or form.

Chemical Problems. By J. P. Grabfield, Ph.D., and P. S. Burns, B.S. (Boston: D. C. Heath and Co., 1888).

THE systematic part of this book occupies the first forty-six pages, the remaining forty pages containing reprints of examination papers. The first part contains general information as to chemical calculations and such matters, with some problems worked out which are likely to be of service to the elementary student; but there are a few points that appear open to improvement. The word reaction is used in its ordinary sense, and also to indicate an equation without the figures that indicate the numbers of the several molecules; to adopt the words of the authors, an equation is a balanced reaction. This appears to be a needless perversion of the meaning of a useful word. The student is told to "balance the reaction" "by repeated trials" of numbers, a method that is certainly very common, but entirely unscientific and unnecessary—in short, a method of cramming, and not a method of teaching. At p. 12 we read, without qualification, "that the weights of all gases are to each other as their molecular weights": it would be very inconvenient to the commercial maker of gases if the weights of his productions were so restricted. At p. 5 the word weight is used in yet another sense: "If we divide the *weight* of any element in the molecule, multiplied by 100, by the per cent. of that element, we will have the molecular weight." This looseness of language would, we fear, be confusing to most students and to many teachers. The volume will be chiefly useful to those who are preparing for the examinations indicated in the second part.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Recent Eruption at Vulcano.

IN a letter recently received here from Mr. Narlian at Lipari, that gentleman mentions some interesting facts relating to the recent eruption of Vulcano. Amongst others, he says that some weeks since some fishermen crossing between Sicily and Vulcano "found themselves in a boiling sea, the water bubbling up," "pieces of pumice coming up through the water." It appears also that the cable between Capo Milazzo and Lipari has been broken at about the same place, which is marked by "large rocks." He then says he has been unable to examine any of the pieces of pumice said to have come from the bottom of the sea.

Whether we have evidence here of a submarine eruption or of a fumarole it is difficult to decide, but further information we must hope will be forthcoming. It is possible that it is simply a submarine fumarole or spring, similar to the Sorgente Le Caldaje, which is met with in the sea at a point forming a triangle with the islets of Lisca Bianca and Bottaro between Lipari and Stromboli.

The following is the latest information in regard to the eruption of Vulcano (the letter is dated Lipari, December 12):—

"Some weeks since, the crater itself showed signs of diminishing activity. The eruptions are often at intervals of hours from each other, and never of such violence. Some days ago, a gentleman, Mr. Salino, says he had gone to the top of the mountain and seen the interior. This gentleman assured me that two-thirds of the old crater is filled up (he had visited the place eighteen years ago!); there was an opening of about 100 metres in diameter in the north-east side of the crater. No lava was seen."

From this it appears that the eruption is diminishing; that probably this filling up of the crater is due to the construction of a cone of eruption, or that it is being choked by the materials ejected from the new opening in the north-east side. Of course without examining the locality one can only conjecture what has really taken place. H. J. JOHNSTON-LAVIS.

Naples, December 15.

Natural Selection and the Origin of Species.

FOR the third time Mr. Thiselton Dyer announces that his judgment is opposed to the theory of physiological selection. But this is not the point that I am debating. I am not discussing the merits of my theory, or endeavouring to influence the opinion of a critic who, after having shown that he had not read my answer to the criticisms which he triumphantly reproduced as never having been answered, now tells us that he has "devoted a good deal of time to the study of" my "views." From the first I have restricted myself to meeting his specific allegations. Still restricting myself to the same ground, I find that there are but two points in his last letter which it is necessary for me to consider.

First, touching the inutility of some, as distinguished from the alleged utility of all, specific characters, Mr. Thiselton Dyer expresses impatience with me for putting what he regards as a "strained interpretation" on Mr. Darwin's writings.[1] Now, of all things in the world, this is exactly what I should most wish to avoid. But, rightly or wrongly, I am profoundly convinced that such strained interpretation as there is, here lies on the other side. Over and over again—and more and more emphatically the later the editions of his works—Mr. Darwin insists that he does not regard natural selection as the only agent which has been concerned in the origination of species, and therefore concludes—to quote only one additional passage from among many to the same effect: "No doubt the definite action of changed conditions, and the various causes of modification, lately specified, have all produced an effect, probably a great effect, independently of any advantage thus gained" ("Origin," p. 160). Moreover, towards the close of the last edition, he complains most bitterly of "steady misrepresentation" with regard to this very point (p. 421)—a complaint all the more forcible from its presenting the only note of bitterness that is to be met with through the whole range of his writings. Now, since his death, this "steady misrepresentation" has continued, until the post-Darwinian school have come to designate by the term "pure Darwinism" the very doctrine which he here so vehemently repudiates.

This doctrine of utility as universal was very clearly enunciated in his own life-time by Mr. Wallace, as "a necessary deduction from the theory of natural selection" ("Contributions," &c., p. 47), but, as just remarked, he expressly renounced it in his section headed "Utilitarian Doctrine, how far true." Therefore I say that, quite apart from all questions of biological theory, or merely as a matter of historical fact, any charge of "strained interpretation" must here lie at the door of those who seek to attribute to Mr. Darwin the opinions which have always been held by Mr. Wallace, and which have now been adopted by the school of Prof Weismann. Moreover, not only is there no sense or reason in speaking of the passages in

[1] He says: "What, however, I view with less patience than his unsustained generalizations, is his persistent attempt to place them on the shoulders of the Darwinian theory." Elsewhere, however, the crown and front of his charge is that I have sought to shrivel up the Darwinian theory to very small dimensions—nay, that I have roundly denied it altogether, if my words have any meaning. How these contradictory criticisms are to be reconciled I must leave their author to explain.

question as "admissions wrung from a hostile witness," or as due to Mr. Darwin "admitting the possibility of explanations to which he really, however, did not attach much importance;" but by thus endeavouring to belittle his judgment on these points, the post-Darwinians are merely showing the weakness of their own. These passages are due to Mr. Darwin having clearly perceived that the doctrine of Mr. Wallace was neither sound in logic nor true in fact. Not sound in logic, because it does not follow as a "necessary deduction from the theory of natural selection," that all specific characters must be adaptive—it being sufficient for the theory if only some such characters are adaptive in each case, as Mr. Huxley has recently shown; not true in fact, because any number of cases (such as those given by Mr. Mivart) can be quoted to the contrary. Therefore, as I said two years ago in the *Nineteenth Century*, those who seek to encumber the theory with this illogical deduction are merely giving occasion to the enemy; it is a gratuitous dogma, standing like the feet of clay in a figure of iron.[1]

But to pass on to the second point. In my last letter I challenged Mr. Dyer to justify his statements that I had roundly denied the agency of natural selection as "the mechanical means by which evolution has been brought about," and that the theory of physiological selection "shrivels up the part played by natural selection to very small dimensions." In answer, he quotes a passage from my paper, and agrees with me that what it says is substantially the same as what the *Times* said. But he still fails to see that this is totally different from what he himself said. In other words, unlike the *Times*, he does not perceive the validity of my distinction between natural selection as a theory of species and as a theory of adaptations. Physiological selection, he thinks, shrivels up natural selection, because, if a true principle in Nature, it must play an important part in the origination of species, and, in whatever measure it does this, it must in a corresponding measure detract from the importance of natural selection. Having at last got him to show that this is his way of regarding the matter, I must first of all repeat that natural selection, besides being a theory of the origin of species, is also something very much more: it is a theory of the cumulative development of adaptations, wherever these occur. In fact, it is, as I have before said, *primarily* a theory of adaptations in all cases, and only becomes *incidentally* a theory of species in those cases where the adaptations happen to be of merely specific value. It is now perfectly evident that Mr. Dyer fails to perceive this distinction; hence his misunderstanding of my views, and hence also the present correspondence. He regards the "origin of species" as synonymous with, and therefore as covering the whole field of, "organic evolution": therefore he accuses me of roundly denying natural selection as "the mechanical means by which that evolution has been brought about," on the ground that I have suggested a supplementary theory of the origin of species. Such being manifestly the impression under which he has read my paper, it is no wonder that in the process he has been, as he says, "completely befogged." I will now endeavour to clarify the matter by explaining at length what I had supposed the readers of my paper would have recognized for themselves.

It is quite true that the evolution of adaptations depends upon the evolution of species, the serial succession of which, in any given line of descent, is the necessary means (through the struggle for existence) to the gradual development of adaptations in that line. But it is of no consequence how many "indifferent" characters these successive species display, provided that they also display, in ever-improving degrees, the particular adaptive characters which are in course of evolution. A bird's wing, for example, is an adaptive structure which cannot be evolved as a merely specific character; it requires to be slowly built up through the lives of an enormous number of successive species, which ramify into genera, families, &c., as the process goes on. Now, throughout this process it is a matter of no consequence how many *other* features of a *non-adaptive* kind arise among all these innumerable species: it is enough, as regards the evolution of a wing, that at each stage of the process *some* of the species should present slight improvements on their predecessors in respect of this adaptive structure. Physiological selection, sexual selection, geographical isolation, "changed conditions" as to climate, &c., or any other "factor," may all the while have been originating any number of *species*, without reference to their wings, though, at the same time, natural selection was continuously promoting the development of wings in *genera*, *families*, and *orders*. In short, species are like leaves, successive and transient crops of which are necessary to the gradual building up of adaptations, while these, like the woody and permanent branches, grow continuously in importance and efficiency through all the tree of life. Now, it is the office of natural selection to see to the growth of these permanent branches: physiological selection has to do only with the deciduous leaves. Hence, although natural selection has likewise an immensely large share in the origination of species (*i.e.* has to do with all species which are distinguished by adaptive characters *peculiar to themselves*), this, in my view, is really much the least important part of its work. Not as discovering *an* agent in the differentiation of species, but as revealing *the* agent in the genesis of adaptations, do I regard Mr. Darwin's theory as the greatest generalization in the history of science. If this view of the matter betrays on my part, as Mr. Dyer says, a fundamental misunderstanding of that theory, I shall be greatly obliged to him for showing me wherein the misunderstanding consists. In the event of his doing so, I will cheerfully renounce the inquiry on which I am engaged, for then, no doubt, my theory would be found in opposition to—and not, as I suppose, in co-operation with—the theory of natural selection. On the other hand, should he fail to meet this request, I shall have "reluctantly to arrive at the conclusion" that the "fundamental misunderstanding" in this matter, like the "strained interpretation" previously considered, lies the other way.

It will now, I trust, be sufficiently evident why I differ *toto cœlo* from Mr. Dyer where he concludes that the theory of physiological selection shrivels up the theory of natural selection. In point of fact, the former theory stands to the latter in precisely the same relation as does the theory of sexual selection. In both these supplementary theories, it is the origin of species that is concerned, and so concerned with reference to characters that are non-adaptive. The cases being thus precisely parallel, I should like to know whether my present critic regards one of Mr. Darwin's own theories as shrivelling up the other.[1] Assuredly, Mr. Darwin himself did not think so, because he clearly perceived that the "origin of species" constituted but a small part of the whole field of "organic evolution." It is true that he entitled his work "The Origin of Species by means of Natural Selection," and therefore in my paper I was careful to point out that, "if it appeared somewhat presumptuous to have insinuated that Mr. Darwin's great work on the 'origin of species' has been mis-named," there were passages in the work itself which fully justified me in my definition of his theory. My critic now quotes this explanation as justifying his statement that I intended to deny the agency of natural selection altogether! I do not quite know how to meet an opponent who resorts to such strange devices; but I may at any rate assure him that in my opinion no more appropriate title could have been chosen by Mr. Darwin for his great work than the one which he did choose; and if I spoke of that work as having been mis-named, I thought I had made it clear enough that I was "strictly speaking," or speaking to a point of logical definition. Moreover, at the time when the work in question appeared, the problem as to the origin of species was, as its author says, "the mystery of mysteries." But

[1] In connection with this point I have to express regret for a verbal error which Mr. Dyer has already pointed out in my last letter. But, from what he says of Prof. Nägeli's essay, it appears to me that he cannot have read it. At any rate, it is in no way confined to "the inutility of family characters," and in considering it Mr. Darwin supplies more than two pages of instances to illustrate his argument therewith, ending with the general conclusion:—"We thus see that with plants many morphological changes may be attributed to the laws of growth, and the interaction of parts, independently of natural selection." To ignore all such passages, or to regard them as admissions," is assuredly—once more to quote my critic's words against himself—"entirely to misapprehend their significance, or the spirit in which they were made"

[1] In this connection it is interesting to note that Mr. Wallace has always been the principal opponent of the theory of sexual selection, as he now is of the theory of physiological selection. Moreover, the reason of his opposition in both cases is that he thinks such subordinate theories of the origin of species must fail to find a *locus standi* in the presence of the greater theory of natural selection: the latter, in his view, must necessarily "shrivel up" the former. Now, his arguments against sexual selection are incomparably stronger than those which he has advanced against physiological selection (compare "Tropical Nature," pp. 192–211, with the *Fortnightly Review* for September 1886, and the *Nineteenth Century* for January 1887); yet they failed to influence the judgment of Mr. Darwin, whose very last words to science—read a few hours before his death to a meeting of the Zoological Society—were: "I may perhaps be here permitted to say that, after having carefully weighed, to the best of my ability, the various arguments which have been advanced against the principle of sexual selection, I remain firmly convinced of its truth."

now that we have all come to recognize species as nothing more than pronounced varieties, it appears to me a curiously interesting example of the "survival" of traditional modes of thought, that so many systematists still continue to regard the value of Mr. Darwin's theory to consist in what is really its least important function. The result has been clearly displayed in the present correspondence:—

There's such divinity doth hedge a species,
That science can but peep to what it would.

London, December 8. GEORGE J. ROMANES.

Engineers *versus* "Professors and College Men."

PROF. TAIT in his recent letter says that the only meaning the expression $\frac{Mv^2}{2g}$ conveys to his mind is the product of a mass by a length. But how does he measure his mass and his length? Is the mass to be measured in pounds or kilogrammes, or to be measured in units of g pounds or g kilogrammes? And is the unit of length the foot, or the metre or centimetre? so that g is variously 32, 9·81, or 981.

These are the points which are slurred over by "Professors and College men," but are of fundamental importance to practical engineers, who dare not trust to a formula till they have verified it numerically.

Let me conclude by giving Prof. Tait a question, selected from College text-books:—

"What is the meaning of $\frac{Mv^2}{2g}$ when the unit of area is one-tenth of an acre, the numerical value of g is 2, and the unit of weight is the weight of unit volume of the standard substance (the substance of which the specific gravity is unity)?"

I think he would be amused by the variety of answers he would receive, although the answers might individually be all correct.

December 17. A. G. GREENHILL.

Mr. Dodgson on Parallels.

MR. DODGSON has written to me thanking me heartily for my "interesting and helpful review" of his "New Theory of Parallels." He admits his slip in the corollary on p. 11, and supposes, as I had myself thought, he took ADC to be the triangle required instead of ABF. "But there is *one* criticism of yours which, if true, would vitiate the whole treatise. May I ask you to reconsider the point, and, should you see reason so to do, to notify to the readers of NATURE that you withdraw it? You say that, in Props. viii., xi., I tacitly assume that the 'amounts' of triangles are either *all* greater than two right angles, or else *all* less. . . . Such an assumption would indeed be monstrous." I willingly accede to Mr. Dodgson's request, as the following form of his argument, supplied in his letter to me, does away with my difficulty. "Either (α) there is a triangle whose 'amount' = two right angles, or (β) there is none. If (β) be true, then either (β1) *all* triangles have *greater* 'amounts,' or (β2) *all* have *less* amounts, or (β3) some have *greater* amounts and all others *less*. Now (β1) is proved impossible, in Prop. viii.; (β2) is proved impossible in Prop. xi.; (β3) may easily be proved impossible, by means of Prop. vii. Hence (β) is impossible. Hence (α) is true." It will be well, if, in a future edition, the missing link of (β3) be supplied. One other point puzzles Mr. Dodgson. It is my remark on Prop. vi.: "How are the figures to be constructed if $n>2$?" Mr. Dodgson says: "It surely does not need pointing out that the operation of bisecting an angle may be repeated *ad libitum*." Certainly not. But what I meant was the effect of these bisections upon the resultant chords. The figures to the proposition are incorrectly drawn: in the one figure BD, DC, and in the other BE, ED, DF, FC are *not drawn* greater than the radius, and my point was not the bisections but the enlargement of the figure: thus if $n = 3$, we should have eight triangles, vertices at the centre A, with the sum of their angles greater than 480°. My apology for thus trespassing upon valuable space is my desire to meet Mr. Dodgson's natural wish, and by pointing out what I thought were faults in his "interesting" *brochure* to enable him to make it more perfect in after editions. R. TUCKER.

University College School.

The *Porcupine* Echinoidea.

THE researches lately published by the Drs. Sarasin upon the anatomy of the Echinothuridæ, render a careful reconsideration of the types of Asthenosoma collected by the late Sir Wyville Thomson, during the voyage of H.M.S *Porcupine*, absolutely necessary in my opinion. The species were described in the Philosophical Transactions for 1874. I shall be much obliged if information can be sent me regarding the whereabouts of the specimens which were figured by Wild, *i.e.* the type-specimens of *Calveria* (*Asthenosoma*) *hystrix*, Wy. Th.; *C.* (*Asthenosoma*) *fenestrata*, Wy. Th.; and *Phormosoma placenta*, Wy. Th.

P. MARTIN DUNCAN.

Angry Birds.

IN reference to the notice of a fierce pheasant mentioned by Mr. Maw in the number of NATURE for December 13, I would refer him to my "Observations in Natural History" (p. 172), in which I have spoken of a daring cock pheasant, which I saw myself, while walking in the grounds of a friend in Cambridgeshire. This bird was in the habit of attacking any persons that approached near the spot where he was. Some woodcutters at work on the grounds had to protect their legs with strong leather gaiters. L. BLOMEFIELD.

Bath, December 18.

TWO years ago, whilst walking across a fallow field here, I heard a fluttering of wings, and received a violent blow on the back of the neck from a partridge: before I could recover myself she struck the back of my head and knocked my hat off. Although I had a heavy stick, I could not drive off the bird, who made a loud noise, and now attacked me in front. As I walked rapidly off, the bird followed and struck at me many times, attacking my head and shoulders with the greatest determination and violence. W. G. SMITH.

Dunstable.

PRESENTATION OF A PORTRAIT OF PROFESSOR A. W. WILLIAMSON, F.R.S., TO UNIVERSITY COLLEGE.

ON Wednesday (the 12th inst.) a portrait of Prof. A.W. Williamson, late Professor of Chemistry to University College, London, was presented to the College by Sir Henry E. Roscoe, M.P., F.R.S., on behalf of the committee of subscribers. The portrait is painted in oil by the Hon. John Collier. The presentation took place in one of the lecture-rooms, the chair being taken by the President of the College (Mr. John Erichsen, F.R.S.); and amongst those present were Sir F. A. Abel, Prof. Bonney, Prof. H. Morley, Dr. J. H. Gladstone, Prof. George Carey Foster, Dr. Atkinson, Prof. Ramsay, Prof. Thorpe, Prof. Marks, Prof. Russell Reynolds, and other Professors, and a large number of the past and present students of the College.

Dr. W. J. Russell, on behalf of the Committee, for whom he had acted as Treasurer, said that judging from the subscription list there was a large number of the former colleagues of Prof. Williamson who had subscribed to this portrait; and it would no doubt be very pleasant to him to know that members of all the Chemical Societies in England had liberally subscribed towards the portrait; and further, that many of the subscribers had not satisfied themselves by sending formal contributions, but had written to him (Dr. Russell) expressing their great esteem and regard for Dr. Williamson. The subscriptions did not come only from various parts of Great Britain, but from France, Germany, Switzerland, Italy, Russia, and even so far afield as the United States, Jamaica, India, and Japan. He thought this was all that it was necessary for him to say in order to indicate the high value which the subscribers attached to the great scientific attainments and labours of Dr. Williamson, whose intimate friends and old pupils, those who knew him best, now came forward to pay him this mark of their esteem and regard.

Sir Henry Roscoe, M.P., said:—I consider it, sir, a privilege that, as an old pupil and an attached friend of Dr. Williamson, I should have been chosen, on this occasion, to present his portrait—which I think you will

all consider as a life-like one—by Collier, to the College in which he laboured so faithfully and so successfully for nearly forty years. The first appointment of Dr. Williamson dates, as you, sir, are aware, from the year 1848, when, following Fownes, he was, as Professor of Practical Chemistry, placed in charge of the first teaching scientific laboratory established in England, and in a few years afterwards, on the resignation of Graham, he assumed the responsibilities of the two Chairs of Chemistry. A favourite pupil of Liebig's, Williamson had at Giessen imbibed the scientific spirit of that great master, and had, at the early age of nineteen, published his first original investigation. Afterwards carrying on his studies in Paris, and becoming intimate with Laurent and Gerhardt, he brought to London the best traditions of the French as well as of the German schools of chemistry, uniting in his person the attributes of both. Entering upon his duties in this College with the enthusiasm for his science characteristic of his nature, was it to be wondered at that he should have imparted to the young men who were fortunate enough to come under his influence some sparks of that fire which burnt so brightly in his own breast? I well remember the vivid interest, the keen appreciation, with which all those who studied in the Birkbeck Laboratory at that now distant time followed step by step the unfolding of his views on etherification, and on the constitution of salts, which may be truly said to have laid the foundations of modern organic chemistry. All those of his pupils who then made up their minds to devote their lives to chemistry, whether in the walks of the pure science or in those of its applications, must willingly own that much of the success which they may have met with in after years is due to his teaching and example; and admit that in the receptive period of a man's life the influence exerted upon them by a teacher whose years were not far removed from their own, of high aims and of ardent temperament, could not fail to be inspiring. This is not the occasion to inquire into the position which Williamson holds as one of the great chemists of our time and country. Rather is it our object now to express the feelings of gratitude and, if I may be allowed to say so, of affection, which we, who have been his pupils and are his friends, as well as those of us who can only claim the latter but perhaps no less intimate relationship, entertain towards him; to assure him that we look back upon the times spent in the laboratory with him as some of the pleasantest as well as some of the most fruitful of our lives. And both pupils and friends here join to show their appreciation of his labours and of his character, and to acknowledge the debt which they and their science owe to him. This portrait, sir, of our friend and master, finds a fitting resting-place within the walls of the College in which his working years were spent. It will remain as a memorial of a teacher, an investigator, and a colleague, whose main interest was to uphold and increase the renown of University College as a centre of intellectual progress, and of one whose character, both as a man and as a chemist, future generations, like our own, will delight to honour. It is now my pleasing duty to unveil the portrait, and to ask you, sir, as the President of this College, to accept it on behalf of the subscribers.

The portrait was then exposed to view.

The President said:—In the name of and on behalf of the Council and members of this College, I accept with gratitude this admirable likeness of our dear colleague and friend, Dr. Williamson. Sir Henry Roscoe has truly said that this is not the place to dilate on Dr. Williamson's great scientific merits, and the great claims which he has as a scientific man to any honour that could be bestowed upon him. I shall not venture on this subject, but I may say this: that looking at Dr. Williamson's career, as I can do, for the last forty years (thirty years of which he has been connected with this College), there never was a man more loyal to this institution, and more devoted to its best interests, than Dr. Williamson. The business of a Professor here is not only to teach, but to take part in the management of the College, which, as you know, devolves individually and collectively upon them. It is in the meetings of the Council and the Senate that the devotion of the Professors to the interests of the College is shown quite as much as in the teaching of classes. The College could not be worked without the business aptitude of the professorial staff; and in this duty of management none showed more zeal, loyalty, and devotion, during thirty years, than Dr. Williamson. We as a governing body must feel deeply indebted to him for the interest which he has shown in the welfare of this institution. But I should not be doing my duty if I were not to couple with his name that of his wife. Mrs. Williamson was as devoted as her husband, and did very much to raise the character of the School. She showed the greatest interest and enthusiasm in all the work which as a woman she was able to perform, and to bring about harmony within the walls of this institution. Mrs. Williamson worked side by side with her husband with unwearied devotion in, as I have already said, the best interests of University College. Ladies and gentlemen, I can only add in my own name, and in the name of this institution, the hope that Dr. Williamson and Mrs. Williamson will be followed with all health and happiness in their comparative retirement from further active work.

Sir Frederick Abel said:—Allow me to move a vote of thanks to the President of the College for his kindness in being the mouthpiece of many old friends in expressing as he has done the high respect and great affection entertained for Dr. Williamson by all his old colleagues. I desire to add that it is a great pleasure to me to be able to assist at this ceremony; and I, for one, am highly gratified at the life-like portrait presented to the College and accepted by you, sir, as President, on its behalf.

Dr. T. Anderson seconded the vote of thanks proposed by Sir Frederick Abel.

The President briefly acknowledged the compliment which had been paid him.

Dr. Williamson (who was received with cheers) said:—"I believe, sir, that the reward which upon the whole is most satisfactory (and which perhaps I may call the highest) which can be given to the man who has endeavoured to do his duty, is the expression of approbation from men of high authority on the subject-matter on which he has worked. The compliments which have been paid to me to-day have been enhanced greatly by some words which Sir Henry Roscoe let fall, and which could not have come with greater weight from anyone than from my old friend. It is to me a proud feeling—one which gives me great satisfaction—that in the decline of my life, and the end of my career, I should, from such a man—a man of such high character and position—have received so cordial and friendly an expression of approbation and personal esteem. I must ask leave, sir, to thank the Council, and you as their head, for the honour which you have done me in allowing my portrait to be placed within these walls; for although I have been associated with other colleagues, and have performed duties of other kinds, there is no place that I have felt it so great an honour to be connected with as University College. Here I have been associated with many men who have made noble self-sacrificing efforts in the best interests of this institution. I look back with pride on my connection with my colleagues of this College, though I have often bitterly regretted that my intercourse with the students has not been more personal. Sometimes a man comes up to me, shakes me by the hand, and calls me by my name, and I am obliged, to my shame, to confess that I do not know his name, which I am obliged to ask, and then I find he was an old student who knew me perfectly, remembered my lecturing in a long dark room, in which I was visible

to him, though he was not visible to me. I have often very much regretted that I have not been brought into closer relations with this large body of earnest men and students. Still, among those whom I have known I have found many esteemed friends. I do not think it desirable for me to make further remarks, beyond expressing to Mr. Collier my appreciation of his success in making what is not an ugly portrait out of such an ugly face as mine.

The proceedings then terminated.

In the evening Dr. Williamson was entertained at dinner at the Freemasons' Tavern by a goodly number of his friends and old pupils. Sir Henry Roscoe presided. After the toast of the Queen had been given and duly honoured, Mr. Carteighe, one of the honorary secretaries, announced that a considerable number of letters from subscribers had been received, expressing their regret at not being able to be present. The one from Prof. Michael Foster, F.R.S., referred humorously to Dr. Williamson as the "Ether Meister."

Sir Henry Roscoe, in proposing the toast of the evening, "Our Guest" (Dr. Williamson), alluded in kindly and affectionate terms to his early association with him, to his enthusiasm as a teacher, and to the respect in which he was held by men of science all over the world.

Dr. Williamson, in replying, expressed the gratification which their hospitality and kindness had afforded him, and referred with pride and satisfaction to the great honour which had been conferred upon him in the presentation of his portrait to University College. In conclusion, he invited any of his old pupils, present and absent, when in the neighbourhood of Hindhead to call and see him in his "nest."

Mr. Norman Lockyer, F.R.S., proposed "University College and its President," and Mr. J. Eric Erichsen, F.R.S., the President, replied.

Prof. W. H. Flower, F.R.S., submitted "The Professors of University College." Prof. Henry Morley responded for the Arts Faculty, and Prof. G. C. Foster for that of Science.

Prof. Ramsay, F.R.S., proposed "The Chairman," and Sir Henry Roscoe, M.P., responded.

Prof. T. E. Thorpe, F.R.S., proposed "The Committee of the Williamson Testimonial," to which Mr. Michael Carteighe, President of the Pharmaceutical Society, and Dr. H. Forster Morley, the honorary secretaries, replied.

THE MORPHOLOGY OF BIRDS.[1]

II.

THE second part of vol. ii. is taxonomic and systematic. The author criticizes and tests the taxonomic value of the numerous characters of all the organic systems; each paragraph forms therefore a condensed *résumé* of our present knowledge of the various organs, with especial reference to those parts which proved to be of more than ordinary taxonomic importance. The question if an organ is of taxonomic value at all does not depend upon the presence or absence of the organ itself, but upon what it is like. Hence the weakness of those systems which have been based upon positive and negative characters only; even Garrod failed, since he took for his guidance not quality, but merely quantity. Those organic characters are preferable which exhibit a certain amount of differentiation, but which at the same time do not vary much within the limits of smaller groups of birds. Through combination of a considerable number of such characters, to be taken from organs between which there can be but little correlation, we have the best chance of arriving at a sound system. But of such characters there are, unfortunately, few.

However, on pp. 1580–91, Fuerbringer has selected forty-eight characters, not all, of course, of equal value, and has arranged them in tabular form, together with the ninety families into which he divides the birds. Especial attention may be drawn to the second column, which contains the first known occurrence of fossil members of each of the families. This column, together with the remarks on pp. 1107–10, and the discussions under the heading of each family in the special systematic part of the book, contains the only complete and critical essay on fossil birds that has yet been published.

But it is impossible to give here anything besides occasional hints about the vast amount of thought which the author has bestowed upon nearly all the organic systems, always on the look out for characters which might perhaps prove constant enough to act as guides amongst the chaos of the natural affinities of birds, always awake where great adaptiveness or convergence of forms might easily lead us astray.

Bill and feet proved to be of comparatively little value, in spite of their historical significance; the same applies to the oil-gland; whilst pterylosis is never to be neglected, especially that of the embryo.

Oology.—The size of the eggs depends upon the terrestrial, aquatic, or aërial life of the birds. Those which make their nests in high trees lay, as a rule, smaller eggs, and are "altrices"; whilst those which lay the eggs on the ground, and are "precoces," have more and larger eggs. Thickness of the shell, or the weight of the egg, often depends upon the smaller or greater liability of the eggs to external injury. The colour of the eggs stands, like that of the female bird, in correlation with the configuration of the nest, and affords good characters for classification. The best character, however, is formed by the finer structure or texture of the shell, since this remains unchanged in the species, and can also successfully be used for the recognition of wider relationship.

Skeletal System.—The importance of relative measurements has induced the author to look for a unit applicable to all birds. This he finds ingeniously in the average length of the dorsal vertebræ, because of the constancy of these parts. The numerous tables, which contain (pp. 794–800) an enormous number of measurements, have shown, however, that their taxonomic value is but very limited. The total number of vertebræ is inconstant even in the individual, and varies in larger groups to such an extent (Limicolæ 43–50, Anseres 50–63) that it can hardly be used in determining the systematic position of a given bird. Better results are yielded by the numbers of the cervical, thoracic, and sacral vertebræ alone, and their proportionate quantity, cf. Table xxii. pp. 778–79.

In the configuration of the sternum, the anterior margin, with its spine, is the most noteworthy point.

Of greater value is the configuration of the maxillo-palatine apparatus, as was first pointed out by Nitzsch, J. Mueller, and especially by Cornay in 1847. Huxley's classification, based upon these characters, in 1867, marked an epoch in the systematics of birds; but it is artificial, not natural, as the numerous exceptions and intermediate stages show, which have been discovered by later anatomists. The basipterygoid processes likewise afford gradual differences only. The whole maxillo-palatine apparatus is far too adaptive to permit of its use as a safe guide in classification.

The hyoid bones afford a rather good generic, and occasionally even a family character.

The size of the coraco-scapular angle depends in inversed ratio upon the development of the shoulder-muscles. This, with the various dimensions of the scapula, the processes and foramina of the coracoid, &c., receive special attention in the tabular lists, pp. 738–57,

[1] "Untersuchungen zur Morphologie und Systematik der Voegel. zugleich ein Beitrag zur Anatomie der Stuetz- und Bewegungsorgane." Von Max Fuerbringer, Professor der Anatomie, und Direktor des anatomischen Institutes und des Museum Vrolik der Universitaet zu Amsterdam. Mit 30 Tafeln. (Amsterdam: T. van Holkema, 1888.) Continued from p. 152.

and in the text of the osteological part of the book. Table xxxvii. contains the length of the humerus in units of dorsal vertebræ.

Pp. 1042–47 form a condensed essay on the pelvis. The difficulties of homologizing its constituent parts with those of other Vertebrata are pointed out, but they are not solved. The pelvis, as a whole, has never been tested sufficiently as to its taxonomic value, and the adaptability of the limbs, both anterior and posterior, warns us not to lay too much stress upon these parts either.

Pp. 1053–66.—Fuerbringer points out which muscles are of systematic importance, also how far and in which groups of birds he found them to be so.

The results yielded by the most extensive examination of the brachial plexus (pp. 232–80, Plates 8–10) are morphological only, but of no taxonomic value.

In his treatment of the sense-organs, the digestive, vascular, excretory, and reproductive systems, he gives only a more or less cursory review of the work of other anatomists. The organs of voice and respiration receive more attention. The author distinguishes between (1) syrinx trachealis, possessed by the Passeres tracheophonæ, and in a less finished degree by certain Pelargi; (2) s. tracheobronchialis (Psittaci, Passeres, Pseudoscines = Menura and Atrichia); (3) s. bronchialis, many Cuculidæ, Caprimulgidæ, Strigidæ, &c.

Concerning the ontogenetic development of birds, Fuerbringer has been struck with the extraordinary resemblance which the embryos of certain families exhibit to each other before the divergence of the final formation of beak and feet has been fixed. Thus, Laridæ and Limicolæ, Pici and Passeres, Striges and Caprimulgidæ, indicate in these stages close relationship.

Remarkable, although rather short (pp. 1107–19) are the chapters on palæontological development and on geographical distribution. The hypothetical division of the world into Arcto- and Neogæa is not favoured, whilst Lemuria is justly re-established. Explanations of the present distribution of the Ratitæ, Spheniscidæ, Rasores, Passeres, and other principal orders are attempted, and if not always successfully solved, are at least partly cleared up by the allusion to fossil intermediate forms.

The cradle of the Passeres is very old, of Cretaceous age, and existed probably in the Oriental region; the Eurylæmidæ still exist as the last and least modified descendants of the primæval Passeres. Thence they spread all over the globe. About the beginning of the Miocene age one stock branched off, likewise in the Oriental region, as the Oscine type, the numbers of which conquered the world, with the exception of the Neotropical region, which they reached last, and found already fully occupied by their older but highly developed relatives the Oligomyodi and Tracheophones.

The outcome of all this work is a most elaborate systematic arrangement of birds, recent and extinct. This occupies pp. 1136–1591.

Family after family is discussed as to its characters, affinities, distribution, first fossil occurrence, and the position it held in the opinion of previous ornithologists and anatomists.

Fuerbringer's system of birds is almost entirely new, less striking in the arrangement of the families and the placement of odd or solitary genera than in the disposal of the whole host of birds into a few large orders. Such a grouping together has been a long-felt desideratum, because the close adherence to the principle "*Divide et impera*" has led to a splitting up of the birds into an ever-increasing number of groups, whilst their combination into greater phyla was in danger of being lost sight of.

This want of generalization made us hail the terms Schizo-, Desmo-, Ægitho-gnathæ; but they were hardly established as household words amongst ornithologists before Schizorhinæ and Holorhinæ, Homalogonatæ and Anomalogonatæ, went through their short-lived existence, and in their turn gave way to other principles of classification by Garrod and Forbes, which will easily be detected in the system now before us. The class Aves is divided into two sub-classes, eight orders, twenty-four sub-orders, forty-five gentes, and ninety families. The orders, especially the four into which the Carinatæ are divided, represent such centres or phyla as we have been longing for, and around them are arranged other, mostly aberrant or much specialized, groups as "intermediary sub-orders." The orders end each in *-ornithes*, the sub-orders throughout in *-formes* (see table on next page).

This system of birds is graphically illustrated by two side views of an elaborate "ancestral tree," on Plates 27*c*, 28, and by three more plates which represent three horizontal sections through this ideal tree. The author justly insists upon the necessity of constructing such ancestral pedigrees in the three dimensions, and he has himself taken care to indicate isomorphism, *e.g.* Gypogeranus and Cariama, Procellariidæ and Steganopodes, by the convergence of the branches.

It is, of course, beyond the scope of this review to enter into many of Fuerbringer's ideas on the affinities of all the families of birds. Only those of general interest can here be dealt with.

The old group of the *Odontornithes* has properly been discarded; their constituent members have been distributed amongst the other birds. Probably all birds possessed teeth during the Cretaceous epoch.

Archæopteryx belongs to the primitive Carinate flying birds or Proto-Ptenornithes. It cannot be decided whether it is a direct ancestor of living Carinate birds; but there are no valid reasons why it should be looked upon as an intercalary type between reptiles and birds.

We learn more about the Ratitæ. They are Deuter-Aptenornithes, *i.e.* they are descendants of Ptenornithes, but have lost their power of flight. The differences between the various forms which are generally recognized under the name of Ratitæ are so great, that these birds cannot collectively be opposed to the Carinatæ. Struthio, Rhea, and Dromæus Casuarius are each representatives of separate orders. Fuerbringer approaches the views of Sir Richard Owen, who more than twenty years ago suggested that the various Ratite birds are the descendants of several groups of the Carinatæ, but that they have become modified in similar directions: their Ratite characters are cases of analogy, and do not indicate near relationship. The separation from the common Carinate stock took place very early, certainly as early as the Cretaceous epoch. The root of the Struthiornithes perhaps contains fibres of the later Pelargornithes, whilst the Rheornithes and Hippalectryornithes have also some traces in common with the primitive or dawning Charadriornithes and Alectorornithes. Lastly, the New Zealand Ratitæ, Apteryx, and Dinornis resemble the Carinatæ in so many features that they form only the sub-order Apterygiformes of the order Alectorornithes. The affinities of Apteryx with the Crypturi and Fulicariæ are even greater than those with the other Ratitæ

For Carinatæ the synonym Acrocoracoideæ has been invented, but the author does not see his way to accepting them as a separate sub-class, since he had to break up the Ratitæ.

The most primitive forms amongst the Ornithuræ are the American Cretaceous Ichthyornis and Apatornis. They differ from recent Carinate birds in degree only, viz. by their tormodont teeth and amphicœlous vertebræ. They stand nearest to the Laridæ, with touches of the Procellariidæ and Ciconiiformes.

Hesperornis has most probably lost the keel of its sternum, and in correlation with this loss has also acquired platy-

CLASSIS AVES.

I.—Subclassis Saururæ.

Order.	Sub-order.	Gens.
ARCHORNITHES	Archæopterygiformes	Archæopteryges.

II.—Subclassis Ornithuræ.

Order.	Sub-order.	Gens.
STRUTHIORNITHES	Struthioniformes	Struthiones.
RHEORNITHES	Rheiformes	Rheæ.
HIPPALECTRYORNITHES	Casuariiformes	Casuarii = Dromæus + Casuarius + Dromornis.
	Intermed. S.O. Æpyornithiformes	Æpyornithes.
	Intermed. S.O. Palamedeiformes	Palamedeæ.
PELARGORNITHES	Anseriformes	Gastornithes. Anseres s. Lamellirostres.
	Podicipitiformes	Enaliornithes. Hesperornithes. Colymbo-Podicipites.
	Ciconiiformes	Phœnicopteri. Pelargo-Herodii. Accipitres. Steganopodes.
	Intermed. S.O. Procellariiformes	Procellariæ s. Tubinares.
	Intermed. S.O. Aptenodytiformes	Aptenodytes s. Impennes.
	Intermed. S.O. Ichthyornithiformes	Ichthyornithes.
CHARADRIORNITHES	Charadriiformes	Laro-Limicolæ. Parræ. Otides.
	Intermed. S.O. Gruiformes	Eurypygæ, incl. Rhinochetus, Aptornis. Grues = Grus + Psophia + Cariama.
	Intermed. S.O. Ralliformes	Fulicariæ = Heliornis + Rallidæ. Hemipodii = Mesites + Hemipodiidæ.
ALECTORORNITHES	Apterygiformes	Apteryges = Apteryx + Dinornis.
	Crypturiformes	Crypturi.
	Galliformes	Galli = Gallidæ + Opisthocomidæ.
	Intermed. S.O. Columbiformes	Pterocletes. Columbæ.
	Intermed. S.O. Psittaciformes	Psittaci.
CORACORNITHES	Coccygiformes	Coccyges = Musophagidæ + Cuculidæ.
		Intermed. G. Galbulæ, incl. Bucconidæ?
	Pico-Passeriformes	Pico-Passeres. { Pici. / Passeres. Makrochires. Colii.
		Intermed. G. Trogones.
	Halcyoniformes	Halcyones. Bucerotes, incl. Upupa. Meropes.
		Intermed. G. Todi.
	Coraciiformes	Coraciæ. Caprimulgi. Striges.

coracoidal features; it would therefore have to be grouped with the Ratitæ if we wanted to degrade this expression to a collective term for cases of converging analogies or isomorphism, and thus deprive it of any phylogenetic meaning. The characters which mark Hesperornis as an Aptenornith are secondarily acquired, whilst all the rest of the skeletal characters indicate its close affinity with the European Enaliornis, and amongst recent birds with the Colymbidæ and Podicipedidæ. This relationship receives its final expression by the establishment of the order Podicipitiformes.

These Podicipitiformes, with the Anseriformes and with the Ciconiiformes, are combined in one big order, *Pelargornithes*. In proportion as the first two of these orders appear circumscribed and natural the Ciconiiformes appear heterogeneous They are made to contain the Phœnicopteri, Pelargo-Herodii, Steganopodes, and the Accipitres or diurnal birds of prey. The close affinity of the Phœnicopteri with the Pelargi is beyond doubt, and so is that of the Storks and Herons, and that of the latter with the Steganopodes. But how the Accipitres should be related to the other three or four gentes seems less clear. However, we must not forget that already Garrod had arrived at similar conclusions. Fuerbringer holds that the Cathartidæ are a very old and now declining Raptorial family, and that they have many structural points in common with the Ciconiidæ, whilst the Gypofalconidæ exhibit genetic relations with the Steganopodes (Fregata) and with the Ardeidæ. Gypogeranus had formerly (Miocene of France) a much wider distribution than now, and it is the last remnant of a group which branched off from the common Accipitrine Stork before the division into Cathartidæ and Gypofalconidæ took place.

Steganopodes are known to have existed in the earliest Eocene period, and are now on the decline; lowest amongst them stand now the Phætontidæ, highest the Fregatidæ. Their rather striking affinities with the Accipitres have already been mentioned, perhaps they are as distantly connected with the Pelargo-Herodii.

Pelargo-Herodii.—Plataleidæ form the lowest type, and afford some points of connection with the Limicolæ; Ardeidæ, the highest and most flourishing family, exhibit various characters by which we might trace their pedigree towards the roots of Colymbus, Haliæus, Falco, and others. This diversity of connections indicates either

that the Pelargo-Herodii are an extremely old group, which has preserved features common to all the other Pelargornithes, or that the division into the various much specialized gentes took place rather recently. Fossil material seems to favour the latter view, and this circumstance probably explains why the Ciconiæ have more in common with the Cathartidæ, whilst the Ardeæ approach the Steganopodes and Falconidæ. Why the Flamingoes should be elevated to the rank of a gens does not appear clear, considering their close genetic connection with the Pelargi, especially through the Miocene Palælodus.

Anseriformes.—Probably an old and small pre-Miocene group, which has marked its broader development more recently. The Eocene Gastornis seems to have been a gigantic type, which had lost its power of flight, like the diluvial Cnemiornis of New Zealand. Amongst recent Lamellirostres, Mergus is the lowest, Cygnus the highest type; they are distantly related to the Podicipitiformes.

Palamedeiformes show many connective points with the Anseres, Steganopodes, and Pelargo-Herodii, but their reception into the Pelargornithes is rendered impossible by various fundamental and primitive peculiarities. Through their intestines and pterylosis they somewhat resemble Rhea. Whether we place them nearer to the Anseres than to the Pelargi and Steganopodes depends upon the taxonomic value which we happen to attribute to their skeletal, muscular, intestinal, or external features.

The Antarctic *Aptenodytiformes*, s. Spheniscidæ, are a very old family, because the genus Palæeudyptes shows that they had become specialized into diving and swimming birds with total loss of the power of flight in the Eocene period, or probably even earlier. Fuerbringer calls the Penguins Trit-Aptenornithes, indicating that they, like the Great Auk, the Dodo, Ocydromus, and others, have lost their power of flight later than the Ratitæ. A sharp line between Deutero- and Trit-Aptenornithes cannot, however, be drawn, since Cnemiornis, Gastornis, &c., are intermediate forms, just as Stringops is now on the way to become Aptenornithic.

Many of the characters of the Penguins generally considered as primitive are partly "pseudo-primitive," *i.e.* phylogenetically reduced and ontogenetically retarded; *e.g.* the structure and distribution of the feathers, the fin-like anterior extremities, the broad scapula, and, according to Fuerbringer, even the metatarsus. The resemblances with Podiceps and Colymbus are superficial only, but he cannot tell to which of recent birds the Penguins approach nearest. All that the author contends against is the removal of the Penguins into a sub-class, equivalent to the rest of the Carinatæ. On Plate 29*a* they are represented as a lonely group.

The *Procellariiformes*, or Tubinares, have likewise the rank of a sub-order, intermediate between Steganopodes, Ichthyornis, Spheniscidæ, and Charadriiformes. They are certainly a very old and now isolated group.

The large order of the CHARADRIORNITHES has split into aquatic and gralline types. The Alcidæ are closely allied to the Laridæ, and are probably the most recent of those birds which have assumed a pre-eminently aquatic and diving life, with correlated reduction of the wings. They are restricted to the periarctic zones of the northern hemisphere, whilst their relatives, the Gulls, enjoy a cosmopolitan range. There can be but little doubt that the oldest *Charadriiformes* were gralline, so that the Otides, with Œdicnemus, Parra, and the Thinocoridæ, stand nearer the common stock than the more specialized aquatic members.

The *Gruiformes* are connected with the Charadriiformes by Eurypyga, with the Ralliformes by Aramus. They seem to have reached their culminating period in the Miocene age. Dicholophus is the most highly-specialized form, and has assumed peculiar Raptorial characters isomorphic with those of Gypogeranus, which is a true bird of prey.

The *Ralliformes* flourished as early as the Eocene period. The Fulicariæ, consisting of the Rallidæ and Heliornis, are more nearly related to the Hemipodii than to the Crypturi. The sub-order of the Ralliformes takes, therefore, a position intermediate between Gruiformes, Crypturiformes, and Apterygiformes.

The latter two sub-orders, together with the Galliformes, constitute the order ALECTORORNITHES.

The relationship of the Crypturi with the Apteryges is real, and bridges over the gulf between Carinate and Ratite birds, especially through cranial and pelvic structures.

The *Galliformes* proper consist of three families: Megapodii, of Austro-Malayan distribution; Neotropical Cracidæ; and universal Gallidæ. The two former exhibit so many important differences in their soft parts that, in spite of their numerous skeletal resemblances, they cannot be opposed to the rest of the Fowls as Peristeropodes. Closely allied to the Galli is Opisthocomus, an old type now dying out; the last solitary species has reached a high degree of one-sided specialization, which elevates this bird above its nearest allies to the level of low arboreal birds.

Columbiformes stand between Charadriiformes and Peristeropodes, perhaps nearer the former through the Pterocletes, which are undoubtedly the more primitive group, whilst Columbæ, beginning with the Miocene only, are still on the ascending scale, and are birds of the future. Didus and Pezophaps are degenerate Columbæ, not necessarily very old forms.

Psittaciformes.—The affinities of the Parrots have puzzled Fuerbringer as much as other ornithologists. He places them as an intermediate sub-order, like the Columbiformes, between the Alectorornithes and Coracornithes. Our knowledge of fossil Parrots is very defective. They existed in the Lower Miocene of France, typically developed; now they are a large, numerous group of birds, with more than intertropical range, and with no living members through which they approach other groups.

The last great order is that of the CORACORNITHES.

The *Cuculiformes* = Musophagidæ and Cuculidæ, are connected with the ancestral Limicolæ and Galli; however, their roots meet so distantly, certainly not later than the earliest Eocene period, that these birds have gone along parallel lines of development since those remote times, and that the Cuculiformes cannot be classed with either Galliformes or Charadriiformes. Their original centre was probably the Oriental region, whence they spread chiefly in Western directions.

The *Coraciiformes* are relatively least removed from the Charadriiformes. The Coraciæ represent the lowest group of arboreal birds, and are related to the Caprimulgi, more remotely to the Owls, Trogons, and Bee-eaters. The Caprimulgi include necessarily the Podargidæ and Steatornithidæ, whilst their apparent similarity with the Cypseli rests chiefly upon secondary analogies. The same applies to the Striges with reference to the Accipitres. Owls have so many important points in common with the Coraciæ (Leptosomus), and especially with the Podargidæ, that they have to be looked upon as Raptorial Coraciiformes or "Podargoharpages."

Halcyoniformes.—The Halcyones, Meropes, and Bucerotes—the latter of course including Upupa—form a pre-eminently palæogæic group of syndactylous birds. The Todi, including the Motmots, connect them with the previous and with the next following sub-order. The same applies to the Trogons.

Pico-Passeriformes.—This large sub-order contains the Pico-Passeres, Makrochires, and the Colii. The Colii have frequently been classed with or near the Musophagidæ, Fuerbringer thinks owing to superficial analogies only. They are now a very lonely little group in the Ethiopian region, without any known history, or without

satisfactory indications of their pedigree, their affinities with the Cypseli being perhaps the least far-fetched.

The *Makrochires* = Cypselidæ + Trochilidæ, prove to be far more closely related to the Passeres than to the Caprimulgi. The old group of the "Cypselomorphæ" had therefore to be broken up.

Pico-Passeres.—Very intimate relationship connects the Indicatoridæ, Capitonidæ, Rhamphastidæ, and the Picidæ to one group—*Pici.* Primitive Pici existed in the Eocene age; many threads bind them to the Galbulæ and to the Halcyones, still more to the Pseudoscines.

Passeres.—They represent the highest types which the avian stock has as yet developed. In spite of their enormous number of genera and species, which surpasses that of all the rest of the birds, they agree so closely with each other in all their principal and primary characters that the Passeres proper are morphologically only of the value of one family. This uniformity has naturally always rendered their further classification very difficult.

Fuerbringer divides them as follows, in close conformity with the views held by most English ornithologists.

I. Family *Pseudoscines* = Atrichia + Menura.—They are types which are now dying out, and which differ from all other Passeres through those characters which they have in common with the Pici.

II. Family *Passeridæ*, with four sub-families.

(1) Desmodactyli = Eurylæmidæ.—They differ fundamentally from the Coraciæ, and are the last remnants of the oldest Passerine forms.

(2) *Oligomyodi.*—Their wide distribution—*e.g.* Pitta in the Oriental and Ethiopian regions, Xenicus in New Zealand, the overwhelming majority in the Neotropical region—sufficiently indicates the extreme age of the Oligomyodi, and sufficiently accounts for the great diversity in the development of the syrinx, podotheca, and femoral artery, &c., which makes these birds appear a rather heterogeneous group.

(3) *Tracheophones.*—The tracheophonous syrinx, and the entirely Neotropical distribution of the Conopophaginæ, Pteroptochinæ, Formicariinæ, Furnariinæ, and Dendrocolaptinæ, suggest a monophyletic origin of these birds from lower American Oligomyodi.

(4) *Oscines* s. *Acromyodi.*—This family forms what may be called the topmost branches of the avian tree, with the Corvinæ as its culmination. It is characterized by the diacromyodean syrinx, and by the bilaminate covering of the tarsus. The latter feature occurs, however, also in the tracheophonous genus Heterocnemis, and is absent in the Alaudinæ.

Regarding the development of these four sub-families of the Passeridæ, the reader may be referred to a previous page (p. 178) of this summary.

Most probably all birds are the descendants of one reptilian form, though of which we do not know. The first lizard-like birds were small, and very likely terrestrial. They diverged into climbers on rocks and trees, and into inhabitants of swampy regions. The latter stock gave rise to swimming birds. The first birds were not vegetable feeders, as is generally supposed, but lived on insects and other small Invertebrata.

Lastly, there arises the question: What are the reasons for the natural extinction of large birds? Not predestination or catastrophes.

High differentiation, possible only through the one-sided development of certain organic systems and correlated regressive metamorphosis of the others, has, in the older groups of birds, frequently led to increased size of the body. This size, although securing a predominant position to the birds for the time being, inevitably implies the turning-point in the height of their development Large or highly specialized animals will be least able to adapt themselves to further changes of their never-stationary, ever-changing surroundings, because, through their very one-sidedness, the retrograded as well as the most specialized organs have rendered the whole organism more fixed than is the case with lower or less differentiated and therefore still plastic contemporaries. Amongst the younger groups of birds such a large size as is common amongst old and isolated types has not yet been reached, and probably will always be avoided. Small, but equally developed, will be the birds of the future.

So far so good. But with all this praise, are there no faults in Prof. Fuerbringer's work? Certainly, there are some. Its greatest fault may be indicated and at the same time explained in one sentence. If the author had been able to devote another year's labour to his "Epoche machenden Untersuchungen," he probably would have written a smaller book. H. GADOW.

MUSINGS ON A MEADOW.

TO the general observer nothing in the way of vegetation would appear to present so few aspects, so limited a scope to the imagination and the associative faculties, as an expanse of herbage; and yet, perhaps, nothing that bountiful Nature has provided for the use and service of men so teems with the variety of associations that it presents to each different mind.

The farmer, whether he be the farmer of England, the wandering Bedouin, or the ranch-man of the New World, looks at the broad pastures and far-stretching plains, but not to admire the mingled masses of gorgeous colours, not to speculate upon the battle that may have been fought upon this spot or the scenes that have happened there in former times, not to separate the numerous varieties of grasses into their many botanical genera and species, but to calculate how many sheep he can feed to the acre upon it, whether there is enough of white clover to fatten his camels upon, or whether his horses will have a sufficiency of suitable food to graze upon. The wide wild waste of endless lines of pale yellow, red, and gray, conveys no pleasure, but merely the indication of a good soil; and the buttercups and daisies he sees in the pasture meadows of England, hallowed by songs and memories, are to his economic eye positively offensive; knowing, as he does, that the older these buttercups grow, the more distasteful they become to stock, but never stopping to discover that it is because they become more acrid. To him it would seem a species of legerdemain if a botanist were to say to him, pointing to a buttercup, "Dig that up, and you will find a tuber at the root," and were then to select another, apparently similar in appearance to the former, and were to tell him that it had no tuber at the root; for from his eyes are completely hidden those minute differences so easily seen by the specialist between *Ranunculus bulbosus* and *Ranunculus acris.*

The botanist, on the other hand, as his eye rests on the same spreading plains of green, is utterly regardless of the feeding value of the plants that he sees before him. As he wanders from country to country, his eager eye detects the diminution or increase of particular species in different latitudes and altitudes, searching out the truths of Nature, or watching with a view to the confirmation of some pet theory. His mind ranges over the different prairies, plains, and meadows of the world. Again the battle of plant life is waging for him. His delight is unbounded. Every plant has its own history, so evident to him, so abstruse to the mere superficial observer; and, involuntarily, associations crowd upon his mind, of some musty tome perchance, or some ancient and not very accurate plate, or some amusing anecdote. For example, the cactus in the plains of Arizona or Texas reminds him of the many times he has seen this genus portrayed in pictures of the Holy Land at the time of the Founder of Christianity, and how, even in books pretending to be learned, he has met with it in the description of the plants of Syria of 2000 years ago, although, as a matter of sober history, this

plant was only introduced into the Eastern Hemisphere after the discovery of America, in which continent the cactus is indigenous. Even if he be colour-blind, and the petals of the blazing poppy show to him the same tone as its sober sepals, yet he is still able to admire the beauty of form, which conveys to him the history of the development of the plant from its seed, and he would still be in the position to give a discourse interesting to the uninitiated though intelligent observer; and at the end of an hour's conversation, in common with others, he would be compelled to exclaim that there was still much in the life of the plant of which he was utterly ignorant—so limitless are the subtle workings of Nature!

But what does the artist care for either of these views? He cares not for the fattening of stock. He feasts with rapture on the different grays, greens, yellows, blues, and reds, that are spread out before him, and on the delicate tints and shadows cast by every passing cloud. He cares not to know of the buttercups and daisies that grow there; it would even distress him to tell him that the yellow-greens are groups of fescues, and the blue-greens patches of cock's-foot and fox-tail. What is that to him? He was wondering what colour in his box would reproduce those delicate tones. He does not seek to know how much corn to the acre that meadow would grow if ploughed up, nor how much it might once have grown. To him it is merely one endless feast of colour.

Perchance more sordid ideas of another kind may occur to him. Apprehensions as to the hanging of his picture may dispel his rapture in Nature's handiwork, and his mind may be occupied with a fear lest the red ground of his neighbour's picture will kill his own more delicate tones. Pounds, shillings, and pence, the cares of a wife and family, are apt to destroy for a time the beauties of Nature.

How happy is the man who sketches and sketches only to reproduce for himself these works of Nature; whether they be the meadows of England, dotted with short-horns and Hampshire Downs, and bounded by the rook-sheltering elm; or the plain of Megiddo; or the boundless prairie of Manitoba.

How differently again would the antiquary survey the self-same scene! His mind would revert to the people who trod these plains in days of yore. Their history, manners, customs, dress, and social habits, would open out to him a wide field of speculation. On this very pasture contending armies may once have trod, and the ebbing life-blood of patriotic heroes once have flowed. His eyes may be resting on a Bannockburn, or a Worcester, or a spot where the Carthaginians of old strove against the might of Rome, and were worsted in the fight. Here the chariots of the Egyptians may have rolled, or the devoted Aztecs have struggled hopelessly against their Spanish conquerors.

The contemplation of the plains produces in him yet another train of thought. He conjures up an historical novel or poem, but finds he has merely repeated the ideas he has read before: he casts them aside and starts afresh; and still he envies the artist the ever-changing phases, and the botanist the unsearchable workings, of Nature. To him there are limits fixed and defined: his speculations are restricted to the period of man's existence on this globe, but for the painter and botanist the range and variety of subject are illimitable.

Thus the pasture and the prairie grow up and die away, containing, like most things in this world, their quota of good and bad. Some weeds go unregarded; the pernicious effects of others become so prominent that they are recognized at once and hated accordingly, like the grass *Cenchrus tribuloides*, which bears a prickly fruit that winds itself into the wool of the sheep, and renders the rearing of sheep where it exists impossible; while other plants, such as clover, and the blue grass, arrest the attention of even the careless agriculturist by their manifest merits.

Now, to all, these herb-clothed portions of the earth offer themselves in various phases according as the mind is prepared to receive them; and happy is he who can so adjust his mind, and concentrate his thoughts upon the phase required; though so devious are the paths of Nature that he will often travel far, and then, as a man traversing a labyrinth, be checked by some such knotty question as: "How does a plant obtain its nitrogen?" and he will have to return to the post from which he started. And such are the difficulties which have deterred those who have written on the formation of pastures from going further on their course. They have rested content with a description of the peculiarities of each plant.

But to him who tries, both as botanist and agriculturist, to fathom the mysteries inseparable from a meadow, whether in the New or the Old World, difficulties present themselves "not in single spies but in battalions." Nature alone supplies enough subjects for the closest study and investigation: depth of soil, worms, showers, dews, periods of drought, periods of wet, grubs, birds,—each and all arrest the mind, and claim due consideration. And when to these are added difficulties of man's own providing, necessary though they be, the solution seems to become a hopeless problem. Now no longer is the battle of plants waged merely with the weather and their other natural antagonists, as they may be fitly called; no longer is the struggle modified into the simple solution of the survival of the fittest; for the farmer produces new enemies to pasture in the shape of stock and the scythe, for cattle select the plants they like best and leave the worst to seed, and the ruthless scythe exposes the delicate stem to the heat of the sun.

An opportune shower has preserved a field from the pernicious effect of soft oat grass (*Bromus mollis*), by thus rendering it palatable to stock, and so preventing its seeding, while the want of rain has caused a meadow, almost contiguous, to be impregnated with this obnoxious weed.

The struggle of plant life is always waging in a pasture, and unless the issue of the battle is directed by animals or men, the most vigorous get the upper hand.

Nature, with her customary and marvellous counterbalancing characteristics, has foreseen this possibility and provided against it, for in a wet season stoloniferous grasses (whose travelling shoots have then the power of sending innumerable roots into the ground, each to become a parent plant) cover the ground to such an extent that the superficial observer is tempted to declare that the meadow which he views is entirely composed of creeping grasses. On the other hand, in a dry season, deep-rooted plants such as tall fescue (*Festuca elatior*) gain the mastery and apparently oust their opponents. The vigorous grasses characterized by an underground growth, such as fox-tail (*Alopecurus pratensis*), and which are amply provided for by Nature in respect of hardihood when growing, are scanty seed-bearers; and even when they do perfect their seeds, so small is the store of food contained in them, compared with their immediate neighbours, that a large percentage of them germinate only to wither away. Rye-grass and the smaller dog's-tail (*Cynosurus cristatus*), deprived of other means of reproducing their species and fostering them, bear seeds that are eminently qualified to reproduce themselves.

Let him, therefore, who essays to unravel the mysteries of our green meadows remember to cultivate to the acutest degree the faculty of ocular observation, for

> "Segnius irritant animos demissa per aurem
> Quam quæ sunt oculis subjecta fidelibus, et quæ
> Ipse sibi tradit spectator."

Let him also learn to employ and utilize the intelligence of others; and above all let him not be surprised if, after much patient study and investigation, his heart sinks

within him, appalled before the host of difficulties; as when, for example, the shadow of a passing cloud on a sunny day reveals to him the individuality of the 400 plants in each square foot of pasture, which have before escaped his attention, and the thought flashes across his bewildered mind of the 400 parts played by each plant, and the 400 times 400 causes that effect them. Almost hopelessly he perceives that the knowledge which he is in search of, and which he fancied he had in his hand, has eluded his grasp like a fire-fly, and left only a flash behind; and for a moment his mind is enveloped in darkness, overpowered by the infinity of Nature. The searcher after truth, however, recovers, recognizes his difficulties, recalls the object of his pursuit, returns to it with renewed energy, and, with unbiassed mind, records his observations day by day.

ALPINE HAZE.

THAT no letter has appeared on the common occurrence of this phenomenon in the British Isles is one of a thousand tokens of the small amount of interest in atmospheric phenomena taken by the public in this country. In bright days when the atmosphere at the earth's surface is nearly calm and moderately dry, the sky being nearly devoid of clouds, horizontal layers of buff-coloured haze may be frequently seen near the horizon in almost every locality in the British Isles where the air is free from the smoke of our large towns. I do not happen to have seen it nor have I heard of its occurrence on the west coasts of Scotland or of Norway. I have frequently noticed it when out of sight of land. It would be interesting to know whether it is seen at a distance of 400 or 500 miles from the nearest shore. At St. Aubin's, Jersey, at an elevation at which the French coast near St. Malo is rarely visible, a stria of this haze is often the accompaniment of mirage, the inverted image of the white-tipped rocks appearing to hang from the layer of haze, and the mirage disappearing with a change of the observer's elevation, but the line of haze remaining visible. The mirage was, when first seen, mistaken for a few minutes by some members of my family for a series of water-spouts.

I have long ago given the specific name of *nebula arida*, "dust-haze," to the phenomenon dealt with in this letter; and I still think that the English title "dust-haze" is preferable to "earth-haze," or any equivalent of the former to any of the latter in foreign languages, the latter term being too general, and capable of including the haze or mist produced by the presence of water particles. The word "dust" does not exclude organic matter, although ordinary dust-haze consists in most cases, as I think, of inorganic particles. On the other hand, any name which appears to localize the phenomenon is scarcely admissible. Dust-haze may be somewhat easily distinguished from ordinary mist or water-haze by its colour appearing of a reddish-buff tint in reflected light; and unless in complete shade from direct light, rarely becoming neutral; whereas mist usually appears gray, neutral, or bluish in reflected light, and yellow, orange, or red in transmitted. Patches—or, as they appear at a distance, bands—of dust-haze often become beds of the under surface of *cumuli*, to the formation of which clouds they then seem to bear a causal relation. As frequently in our islands, the haze is replaced during the night by stratiform clouds at its own level. In these cases there can be, I think, little doubt that the solid particles cause the precipitation of vapour. This fact somewhat militates against M. Antoine d'Abbadie's theory that the haze is really dry air, a supposition which for other reasons I regard as untenable.

Seen near the sun at sunset, bands of dust-haze are mistaken by many observers for threads of *cirrus* (what I term *cirro-filum*), and moreover the two phenomena are not uncommonly seen at the same time. As observations at sunset are useful in forecasting weather, the observers should take care to remember that lines of *cirrus* are always more illuminated in transmitted light than any species either of haze or of stratus cloud; secondly, that the *cirrus* threads appear slightly arched when viewed across the line of sight, and radiating when more coincident with it; and lastly, that a slight curl here or there very often betrays the existence of the lofty ice-cloud. I have frequently given rough sketches of these latter sunsets to observers, and these have borne a singular likeness to Antoine d'Abbadie's sketch of the dust-haze itself (NATURE, November 22, p. 79). But I do not understand this sketch, or in what sense it represents two *horizontal* bands.

In conclusion, I may perhaps be permitted to refer to two phenomena slightly related to the subject of this letter. The first is smoke. This is much more "accumulated" or less amorphous than water-dust, although its optical characteristics resemble somewhat closely those of water-dust. On the other hand, it is much more amorphous than dust-haze; and even the line left in the atmosphere by a steamer on the sea horizon on a calm day will scarcely be mistaken for dust-haze. Smoke when at a considerable elevation undoubtedly produces *cumuli* under favourable conditions. A string of ill-defined *cumuli* may be seen in the smoke of a burning forest or heath when carried a long way from land by a wind from the shore. This condensation is probably principally caused by the presence of solid particles, as in the case of dust-haze, but it may be borne in mind that a large quantity of vapour is carried up with smoke.

Lastly, the keen eye will soon learn to distinguish a peculiar haze often noticeable in England, especially over the wheat-producing districts, in fair calm weather in the month of September. This haze is also visible in some, and probably in many, parts of continental Europe. It is caused by flying Aphides, ten or more of which may be often captured in a cubic yard of air at about 10 feet from the earth's surface. In reflected light the haze has a tint of tender ultra-marine ash.

W. CLEMENT LEY.

Lutterworth, December 8.

NOTES.

MR. RALPH COPELAND, Ph. Doc., F.R.A.S. has been appointed Astronomer Royal for Scotland, and Professor of Practical Astronomy in the University of Edinburgh, in the room of Prof. Piazzi Smyth, resigned. Prof. Copeland has proved himself to be among the most skilful of modern observers.

M. DITTE has been elected to succeed the late Prof. Debray at the Sorbonne.

THE Paris Municipal Council will shortly be asked to grant the funds required by Prof. Giard to provide a laboratory and to secure the necessary assistants.

WE refer elsewhere to-day to the dinner given on Tuesday, the 11th inst., at Christ's College, Cambridge, to celebrate the completion of the ninth edition of the "Encyclopædia Britannica." The chair was of course occupied by Dr. Robertson Smith, the editor. Among the scientific contributors present were Sir Frederick Abel, Dr. Affleck, Sir Nathaniel Barnaby, Dr. Buchan, Prof. Cayley, Prof. Darwin, Mr. F. Darwin, Prof. Dittmar, Prof. Michael Foster, Dr. A. Geikie, of the Geological Survey, Dr. Glaisher, Sir F. Goldsmid, Prof. Cleland, Prof. Marshall Ward, Dr. Creighton, Prof. Greenhill, Dr. Günther, Sir Charles Hartley, Baron von Hugel, Prof. Keane,

Mr. Keltie, Prof. Ray Lankester, Mr. Norman Lockyer, Prof. MacAlister, Col. Maurice, Captain Moriarty, Dr. John Murray, of the *Challenger* Commission, Prof. Newton, Prof. Roberts-Austen, Prof. Vines, General Walker. At the close of the banquet the Chairman read letters which had been received from scholars and men of science at home and abroad. In asking the company to pledge each other in good fellowship in the loving cup, he said he could not deny himself the pleasure of saying with how much cordiality and heartfelt gratitude he and his fellow-editor, Mr. J. S. Black, and the publishers greeted the contributors, who, by their hearty assistance and their constant readiness to do far more than one had a right to expect from contributors, had made it possible for them to carry to its completion a work the difficulties of which could not be appreciated other than by those who had to edit it. Having explained the difficulty of recognizing in one toast more than a thousand English and foreign scholars, of whom not more than one-tenth were present, Dr. Robertson Smith said he would follow the example of the title-page of the "Encyclopædia Britannica" and propose three toasts—Literature, Science, and Art. In submitting the first of these toasts, he referred to the great judgment and skill of his predecessor, Prof. Spencer Baynes. Dr. Garnett responded for Literature, Dr. A. Geikie for Science, and M. Yriarte for Art. Prof. Michael Foster, amid cheers, gave "The Health of the Editor." With regard to the first editor, they would all agree with him that the qualities which Prof. Baynes had for carrying on this great work were in a certain way unexampled. His great knowledge of men and things and the fascinating way in which he made one contribute an article were beyond compare. Concerning the present editor he would prefer to fall back upon the letter received from Prof. Huxley, in which he said:—"The influence for good of the spirit of sound criticism which permeates all the theological articles cannot be over-estimated; and in all other respects, so far as I can judge, the work is wonderfully well carried out." The toast was drunk with musical honours. Dr. Robertson Smith said it was hardly possible for him to thank them sufficiently for the way in which they had drunk his health. He would repeat how very much he had felt the constant kindness and support of all his contributors, without which a work of the kind was impossible. He feared that the editor must sometimes be more or less exacting in such cases. It certainly pained him more to be so than, according to his experience, it pained the contributor to meet the wishes that he expressed. Although all good English work had been due to the spirit of accommodation and of friendly help that pervaded our national life, from his own experience he did not think that any monument of English work had shown more of this good quality, which characterized literary men of every nationality, than had the "Encyclopædia Britannica." Prof. Newton proposed "The Publishers," and Mr. Adam W. Black responded. Mr. Sutherland Black gave "The Health of the Provost and Fellows of Christ's College," and with the reply of Dr. Peile the proceedings closed.

THE year 1889 being the fiftieth anniversary of the Royal Botanic Society, it is proposed that the occasion shall be celebrated by a special *fête*. An announcement on the subject will be made in due course.

PROF. JUDD informs us that he has communicated to the Geological Society a paper on the Tertiary volcanoes of the Highlands, dealing with some of the questions referred to in the article on this subject which appeared in NATURE two weeks ago.

ABOUT four years ago King Oscar of Sweden and Norway offered a prize, consisting of a gold medal valued at 1000 francs, and 2500 kr. (£140) in money, for any one great discovery within the sphere of higher mathematical analysis. The prize is to be awarded on His Majesty's sixtieth birthday, on January 21, 1889. Twelve papers have been sent in, seven of which are in French, four in German, and one in Italian.

VESUVIUS has lately been very active. It has been rapidly throwing up a new cone of eruption about 30 to 40 yards to the south-west of the original one, and the fissure across the crater plane towards the west-south-west is increasing in size and is richer in acid emanations. It is possible, therefore, that an eruption may take place soon on that side of the cone, since the vent tends to shift along the fissure pointing in that direction.

A SEVERE earthquake occurred in the Drant Valley on December 3 at 1.40 a.m. The shocks were from east to west, and were accompanied by subterranean noises.

IN addition to Prof. Milne's paper on the effects of earthquakes on the lower animals, to which we have already referred in these columns, the last number (vol. xii.) of the Transactions of the Seismologicul Society of Japan contains several other papers of interest, some of which, however, were noticed at the time they were read before the Society. Amongst other papers by Prof. Milne are: modern forms of pendulum seismometers; the Gray-Milne seismograph and other instruments in the Seismological Laboratory at the Imperial College of Engineering, Tokio; and on certain seismic phenomena demanding solution. A few of these latter are: sound-phenomena at the extremities of earthquakes; in soft ground the large horizontal motions are preceded by a series of vertical surface ripples; near to an origin the amplitude of normal motion is greater than the amplitude of transverse motion, but as the disturbance radiates they rapidly approximate to each other. Mr. W. G. Aston writes on earthquakes in Corea, giving a list of earthquakes prepared from the standard histories of that country, the "Tongkuk thong-kam" and the "Kuk-cho pong-kam," and showing that there were not more than twenty-seven earthquakes in all in that country in 1800 years. We observe also that the issue in Japanese of the Transactions of the Society is proceeding apace, and has now reached the fifth volume. This particular volume contains translations into Japanese of the following papers: earthquake effects, emotional and moral, by Prof. Milne; a model showing the motion of an earthquake particle during an earthquake, by S. Sekiya; earthquake frequency, by Dr. C. G. Knott.

ACCORDING to a private letter written by a resident at Godthaab, and sent to Denmark by the steamer *Fox*, Nansen encountered nothing but land covered with ice and snow in the interior of Greenland. The members of the Expedition, the writer says, were often able to "sail" along the smooth snowy surface on the *ski*. *Apropos* of the return of the Expedition, the first vessel leaving for Greenland next spring is the steamer *Hvielbjörnen*, belonging to the Greenland Company of Commerce. It departs late in March, so that the Expedition is due in Copenhagen at the end of May. A national subscription is now being raised in Norway to defray the cost of the Expedition, estimated at about £1000, part of this sum only having been as yet contributed. Some sort of national recognition of Dr. Nansen's achievements is also contemplated.

ON November 27, about 9 p.m., a brilliant meteor was seen at Christiansand, in Norway. It went from east to west in the southern sky, emitting a bright white light, finally bursting without any report.

FOGS of great density have prevailed recently in London, and have frequently spread over almost the whole of Great Britain and France. "It will probably not have escaped notice by those residing in the suburbs," says the *Times*, "that on many occasions lately, while the fog has lasted, moisture has poured down from the leafless branches of the trees as though they had

been exposed to a fall of rain, and the various hygrometers have shown the air to be completely saturated with moisture. Under such circumstances the fogs in London are always less injurious to life than those of a drier nature, and it will be observed that we have had no reports this year of cattle being suffocated at the Cattle Show by London fog, as they were a few years ago. What the difference may be between the two conditions would be an interesting subject for inquiry. On both occasions the fogs were anticyclonic, and it cannot be said that the number of fires in London has decreased during the past ten years."

THE Report on the Administration of the Meteorological Department of the Government of India in 1887-88, which has just been published, is divided into two parts. The first deals with the more important administrative questions that have arisen during the year; the second describes the actual working of the Department, and the condition of the observatories; it contains also extracts from the reports of the inspection of the stations. These reports show that in many cases the paid observers take but little interest in their work. Mr. Eliot has introduced various changes, among the chief of which are: (1) the discontinuance of solar and terrestrial radiation observations, except at a few selected stations, on the ground that these observations are open to various objections, and that the instruments are unsuited for exact measurement,—two instruments, apparently identical in construction, frequently giving different readings under the same circumstances; (2) the tabulation of all observations in a form admitting of easy reference, and of the calculation of daily averages,—at present, although the monthly means have been obtained, the average conditions of each day or week are not yet known; (3) the extension and improvement of the methods of collecting rainfall statistics. Rainfall is registered at all the meteorological stations, but each province has established its own method of taking the observations, the result being an utter want of uniformity in the hours of measurement, the times varying from sunrise to midnight. An observatory has been opened at Bagdad; and the question of the establishment of one at Perim, at the entrance of the Red Sea, is under consideration, at the suggestion of the English Meteorological Council.

ON November 1, a bird very rare in Europe was shot in the Island of Möen, in Denmark, viz. a specimen of the "isabel-coloured runner" (*Cursorius isabellinus*). The home of this bird is the Desert of Sahara. Only one or two specimens have hitherto been seen in Europe.

WE notice in the *Izvestia* of the East Siberian branch of the Russian Geographical Society, a very interesting paper by M. Khangaloff, on the customs of the Buriates some three or four centuries ago, when they did not yet carry on agriculture, and lived only by hunting and cattle-breeding. The author's attention has been devoted chiefly to the *zegheteaba*, *i.e.* the periodical hunting by whole tribes gathering together from places as distant from one another as Verkholensk, Tunea, and Transbaikalia. Several hundreds, and sometimes thousands, of men gathered for these communal huntings, which lasted for forty days, and the author gives interesting details as to the customs in use on such occasions. They are still maintained, although only few tribes come together, and in order to keep up the old associations, fines have been imposed on those who do not assist at the huntings. Several "gentes" take part in the communal huntings, and must send one man out of each ten men of each "gens." The poor are freed from the obligation. The cleverness of the Buriates in killing wolves with their arrows, while riding at full speed, is really astonishing. The best archers kill a wolf at a distance of 100 yards.

THE Superintendent of the Anting Missionary Hospital, in a report on that institution, says that the Chinese believe that various animals, principally the hedgehog, weasel, fox, snake, and rat, take up their abode in man and control his fortunes. The reason given by the Chinese for the selection of these animals is that they have discovered the secret of long life possessing which every other good thing is certain to follow. One patient insists that a man inside him holds interminable conversations with him. A strong cathartic removes this delusion for a few days, but the intruder is certain to return at the end of that time. With regard to the insane in China, they have a pitiable lot there. A plea of insanity is of no avail in a trial for murder, and the culprit is decapitated just as if he were sane. Usually, however, insanity takes a very harmless shape: holding conversations with imaginary persons is the commonest form; refusing to eat or drink, and insisting on sitting continually in one place, are also common.

A REPORT from Hirschberg states that while a reservoir was being made a subterranean river was lately discovered in the Riesengebirge. It is 2 metres below the surface of the earth. The river is said to be, at one spot, 150 metres broad.

PROF. KIKUCHI has published the geometry for Japanese students upon which, as we stated some few months since, he has been engaged. The English equivalent of the title-page is, "A Text-book of Elementary Geometry, vol. i., Plane Geometry, Books i., ii., iii. (corresponding to the Books i., ii., iii., of the Association Geometry)." The work is brought out by the Educational Department of Japan.

PROF. MATTHIAS DUVAL, of the Paris Medical Faculty, has just published a quarto atlas of embryology. It contains forty double plates, with over five hundred figures, concerning the embryological evolution of the chick.

A NEW alkaloid has been isolated from the poisonous plant *Fritillaria imperialis*, a member of the order Liliaceæ, by Dr. Fragner, of Prague. All parts of this plant, and particularly the bulbs, have long been known to be violently poisonous; the action of the poisonous principle being very similar to that contained in the *Scilla maritima*, which is so much used in medicine. In order to investigate the nature of this noxious substance, a large number of the crushed bulbs were triturated with lime, and the mixture evaporated to complete dryness upon a water-bath. The residue was then repeatedly treated with hot chloroform, and the solution so obtained agitated with water acidified with tartaric acid. On the addition of sodium carbonate to the concentrated solution, the alkaloid itself was obtained in the form of a voluminous yellow precipitate. After removal of the mother-liquor as completely as possible by means of the filter-pump, the substance was dissolved in hot alcohol. From this solution in alcohol the alkaloid crystallized in short needles, which, after several recrystallizations were obtained perfectly colourless. The crystals are very sparingly soluble in water, but readily in ether, chloroform, and alcohol, imparting to these liquids an extremely bitter taste. They melt at 254° C., and furnish, on analysis, numbers pointing to the formula $C_{35}H_{60}NO_4$. Dr. Fragner, in consideration of its source, has endowed the new alkaloid with the name "imperialine." On allowing a solution of imperialine in alcohol saturated with hydrochloric acid gas to stand for a short time, large translucent crystals of the hydrochloride, $C_{35}H_{60}NO_4HCl$, separate out; this salt is very soluble in alcohol or water, and these solutions also possess the bitter taste. On the addition of ether to a mixture of platinum or gold chloride and the alcoholic solution of the hydrochloride, a yellow oil is obtained which eventually becomes semi-solid. If this pasty substance be dissolved in hot dilute hydrochloric acid and allowed to stand, the platinum or gold salt is obtained, the former in orange and the latter in yellow-coloured crystals. The platinum salt, on analysis, was found to possess the composition $2C_{35}H_{60}NO_4HCl + PtCl_4$, and the gold salt gave numbers agreeing with the formula $C_{35}H_{60}NO_4HCl + AuCl_3$.

Thus the new alkaloid has been thoroughly investigated, and its composition determined with tolerable certainty. It is a significant sign of the progress of the times that these poisonous principles of the vegetable kingdom are being gradually isolated, and their nature determined. Of the wonderful processes by which they are built up within the vegetable cells, we are as yet almost completely in the dark, and can only hope for light and knowledge from the persevering attempts of chemists to understand their constitution and to synthesize them.

THE additions to the Zoological Society's Gardens during the past week include a Hawk-Owl (*Surnia funerea*) from Russian Finland, presented by Lord Lilford, F.Z.S.; a Nankeen Kestrel (*Tinnunculus cenchroides*) from Australia, presented by Mr. A. J. Wilkins; eight Wild Geese (*Anser cinereus*), a White-fronted Goose (*Anser albifrons*), a Herring Gull (*Larus argentatus*), British, presented by Mr. E. S. Cameron; two Galeated Pentonyces (*Pelomedusa galeata*), six Robben Island Snakes (*Coronella phocarum*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; three Black Tortoises (*Testudo carbonaria*) from South Brazil, presented by Mr. Leonard Cooper; five Moorish Geckos (*Tarentola mauritanica*) from the South of France, presented by Mr. J. C. Warburg; an Australian Cassowary (*Casuarius australis*) from Queensland, three Barbary Turtle Doves (*Turtur risorius*) from North Africa, a White Stork (*Ciconia alba*), European, deposited.

OUR ASTRONOMICAL COLUMN.

THE UNITED STATES NAVAL OBSERVATORY.—Captain R. L. Phythian, the Superintendent of the Naval Observatory, Washington, has just published his Report for the year ending June 30, 1888. The great 26 inch equatorial, which is in the charge of Prof. Asaph Hall, is in good order, and has been in constant use for micrometric measurement of the satellites of Saturn and Mars, and the regular list of double stars. The surfaces of both Saturn and Mars were constantly and carefully examined, and drawings made from time to time. In the case of the latter planet, the "canals" of Prof. Schiaparelli, though specially looked for, both during and after the opposition, could not be made out. The reduction and discussion of these observations are well advanced, in particular the computations with respect to the theory of Hyperion, which are now nearly ready for a complete discussion. The transit circle was dismounted and cleaned after April 4, 1888, and remounted in July, but notwithstanding this interruption 1970 observations were obtained with it from October 1, 1887, to October 14, 1888. The reductions are, however, in a backward state, the computing staff being too weak in number. The results for 1883 have been printed, and those for 1884 are in the press. The programme for future work includes the observation of the stars of the zones S. Decl. 14°–18° for the Astronomische Gesellschaft. The 9·6-inch equatorial has been chiefly used in the continued revision of Yarnall's Catalogue, and the observation of minor planets, comets, and of occultations.

The appendices to the Report contain the results of chronometer trials, and of the examination of sextants, binoculars, and other instruments, chiefly for naval use; the report of the Transit of Venus Commission, and that of Lieut. Winterhalter on his visit to Europe, in which he strongly urges the desirability of the Naval Observatory being empowered to join in the scheme of the Paris Astrophysical Congress of 1887 for charting the heavens by means of photography.

THE TOTAL SOLAR ECLIPSE OF JANUARY 1, 1889.—As the track of this eclipse passes through California, San Francisco, lying but a few miles south of the shadow line, it is probable that it will be watched by a number of persons who might make useful observations, and would gladly do so, if suitably directed. With a view of securing the services of such volunteers, and of employing them to the best advantage, Prof. Holden has published a little pamphlet containing "suggestions for observers." The two principal points on which he lays stress are the determination of the exact limits of the shadow, by noting the duration of totality at places just within it, and the photographing of the corona. The pamphlet is not in the least intended for trained astronomers, but will probably prove very useful for the purpose for which it has been written. No English astronomers are going out to observe the eclipse, but, should the weather prove favourable, it will be well watched by American observers, for a strong party from Mount Hamilton itself are to occupy a station immediately on the central line, and Mr. Chas. Burckhalter, of Chabot Observatory, has organized a party of twenty amateur photographers for the purpose of obtaining pictures of the corona. Messrs. George and Thomas Davidson also, sons of Prof. Davidson, of the U.S. Coast Survey, will photograph the corona at the elevated station of Winnemucca, whilst five observers from the Harvard College Observatory, under the direction of Mr. W. H. Pickering, are to take up a very full programme of photographic, photometric, and spectroscopic observations. Mr. C. H. Rockwell, also, of Tarrytown, N.Y., who was one of the Caroline Island party in 1883, will observe the eclipse.

COMETS FAYE AND BARNARD, OCTOBER 30.—The following ephemerides for Berlin midnight are in continuation of those given in NATURE for 1888 November 29 (p. 114):—

1888.	Comet 1888 *d* (Faye). R.A. h. m. s	Decl. ° ′	Comet 1888 *f* (Barnard, Oct. 30). R.A. h. m. s.	Decl. ° ′
Dec. 21	8 8 53	0 28·8 N.	10 26 53	2 0·2 S.
23	8 7 39	0 22·7	10 26 53	1 10·0 S.
25	8 6 20	0 17·8	10 26 45	0 18·0 S.
27	8 4 54	0 14·1	10 25 26	0 35·7 N.
29	8 3 25	0 11·6	10 26 0	1 31·1 N.
31	8 1 53	0 10·3 N.	10 25 24	2 28·6 N.

The brightness of both comets remains practically unchanged from their brightness on December 19.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *DECEMBER* 23–29.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

***At Greenwich on December* 23**

Sun rises, 8h. 7m.; souths, 11h. 59m. 36·1s.; sets. 15h. 53m.: right asc. on meridian, 18h. 9·4m.; decl. 23° 26′ S. Sidereal Time at Sunset, 22h. 3m.

Moon (at Last Quarter December 26, 6h.) rises, 19h. 59m.*; souths 3h. 43m.; sets, 11h. 14m.: right asc. on meridian, 9h. 51·1m.; decl. 16° 6′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 3	11 46	15 29	17 56·0	24 46 S.
Venus.....	10 30	14 52	19 13	21 2·0	18 58 S.
Mars	10 34	15 8	19 42	21 18·3	16 57 S.
Jupiter....	7 13	11 10	15 7	17 19·3	22 44 S.
Saturn....	19 55*	3 22	10 49	9 30·6	15 50 N.
Uranus ...	1 47	7 11	12 35	13 20·3	7 48 S.
Neptune..	13 59	21 42	5 25*	3 53·2	18 30 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Dec h.
28 ... 19 ... Mercury in superior conjunction with the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Dec. 24,	22 23	*m*
			,, 29,	22 3	*m*
Algol	3 0·9	40 31 N.	,, 27,	4 46	*m*
R Canis Majoris ...	7 14·5	16 12 S.	,, 25,	17 0	*m*
		and at intervals of		27 16	
S Cancri	8 37·6	19 26 N.	Dec. 25,	21 29	*m*
δ Libræ	14 55·0	8 4 S.	,, 25,	6 54	*m*
R Ophiuchi	17 1·3	15 57 S.	,, 27,		*M*
T Vulpeculæ ...	20 46·7	27 50 N.	,, 23,	3 0	*m*
Y Cygni	20 47·6	34 14 N.	,, 23,	17 40	*m*
		and at intervals of		36 0	
R Vulpeculæ ...	20 59·4	23 23 N.	Dec. 29,		*M*
T Capricorni ...	21 15·8	15 38 S.	,, 29,		*M*
δ Cephei	22 25·0	57 51 N.	,, 25,	17 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near ι Ursæ Majoris ...	130	49 N.	
,, κ Draconis	193	68 N.	Swift; streaks.

THE BRITISH ASSOCIATION AND LOCAL SCIENTIFIC SOCIETIES.

THE fourth Annual Conference of Delegates of Corresponding Societies was held during the Bath meeting of the Association, thirty-eight local Societies having nominated representatives. The following is an abstract of the Report which has recently been issued by the Corresponding Societies Committee :—

At the first meeting the chair was taken by Dr. John Evans, Treasurer R.S., the Corresponding Societies Committee being represented by General Pitt-Rivers, Sir Douglas Galton, Prof. Boyd Dawkins, Prof. T. G. Bonney, Mr. W. Whitaker, Mr. G. J. Symons, Mr. W. Topley, Dr. Garson, Mr. J. Hopkinson, Mr. W. White, and Prof. R. Meldola, Secretary.

The delegates were invited to make any statements respecting the work done by the Committees appointed last year, or in connection with other subjects referred to in the Report which had been presented to the General Committee.

A discussion took place with reference to the working of the Ancient Monuments Act, in which the Deemster Gill, Prof. Boyd Dawkins, Dr. Evans, General Pitt-Rivers, Sir John Lubbock, and many of the delegates took part. The issue of greatest importance, so far as concerns the local Societies, is that these bodies should take upon themselves the responsibility of protecting, as far as possible, the ancient remains in their own districts.

Earth Tremors Committee.—Prof. Lebour reported that the Committee was about to apply for reappointment, with the object of, in the first place, prosecuting inquiries as to the best form of instruments, and the best conditions with respect to locality, foundation, &c., for fixing up such instruments. Several Societies and individuals had expressed their willingness to co-operate as soon as these conditions had been determined, and the Birmingham Philosophical Society had made a grant towards the expenses of these preliminary trials.

Prof. Lebour stated also that the North of England Institute of Mining and Mechanical Engineers had recently appointed a Committee, armed with a substantial grant, to make a series of experiments on so-called "flameless explosives." This Committee was now at work, and would gladly receive assistance in any way from kindred Societies. The same Institute had joined with the Mining Institutes of South Wales and Scotland in forming another Committee to conduct a series of experiments on fan-ventilation. He thought that these were examples of the kind of co-operation which the Conference of Delegates of Corresponding Societies was likely to bring about.

At the second meeting of the Conference, the chair was first taken by the Secretary, Prof. R. Meldola, and afterwards by the Vice-Chairman, Mr. W. Whitaker, the Committee being further represented by Mr. J. Hopkinson and Mr. W. White, and towards the close of the meeting by Dr. Evans, who had been detained at the Committee of Recommendations.

The Chairman, in opening the proceedings, said that it would be best to adopt their usual plan, and consider the suggestions and recommendations from the different Sections in their proper sequence.

SECTION A.

Temperature Variation in Lakes, Rivers, and Estuaries.—Dr. Mill said that he wished to point out some of the results that had been obtained by the Committee appointed to make the investigations in conjunction with the local Societies represented in the Association. He had a diagram which showed the work done more precisely than he could explain in a short time. The Committee had twenty observers working at various rivers; most of these rivers were in Scotland, only one or two being in England, while no observations had been started in Ireland. Their investigations showed that while in some rivers, particularly the Aray, the temperature was increased by rainfall, in others this condition was reversed, the temperature being found to suddenly fall during rain. He wished to impress upon the delegates the advisability of extending their observations throughout Scotland and England, and also of extending them to Ireland. Prof. Fitzgerald, the President of Section A, who was a member of the Committee, took a great interest in the subject, and had expressed an opinion that Mr. Symons's rain-gauge observers might make personal observations. Dr. Mill advised all observers to use the thermometer which he exhibited, and which he said was durable and cheap. He trusted that delegates on returning home would lay the subject before their Societies, give them some idea of the work of the Committee, and induce them to co-operate and make observations in their respective localities. Circulars, he added, would be sent to the Societies and to Mr. Symons's rain-gauge observers, and it was hoped that this would bring the question well before them. It would give local Societies an opportunity of doing what they professed to do, and he was perfectly certain they were anxious to promote real scientific work. The observations could be made with very little training, and the investigations of conscientious observers would lead to interesting results, as they would be considered by the Committee in connection with the temperature and rainfall of the districts in which they were made.

In reply to questions by Mr. Cushing and the Rev. E. P. Knubley, Dr. Mill said that the thermometer readings were taken at a depth of 6 inches below the surface of the water, and that the fullest particulars would be supplied by the Committee to any Society wishing to take part in the observations.

SECTION C.

Prof. Lebour, who had been nominated as the representative of the Committee of this Section, said that the Committees on (1) Sea-coast Erosion, (2) Underground Waters, (3) Erratic Blocks, and (4) Earth Tremors, the working of which had been explained to the delegates on former occasions, had been recommended for reappointment.

Geological Photography.—Prof. Lebour further informed the delegates that, in consequence of a paper read before Section C by Mr. O. W. Jeffs on local geological photographs, it was proposed by the Committee of the Section that a Committee should be appointed to collect and register such photographs. The proposal at present was so indefinite that there was no chance of the Committee of Recommendations dealing with it this year, but they gave the suggestion their cordial sympathy, and it was formally passed on to the meeting of delegates. It was hoped that delegates of Corresponding Societies, by discussing the matter among themselves, would have it so organized and ready to place before the Committee of the Section next year, and ultimately before the Committee of Recommendations, in such a form that a Committee of the Association might be appointed, with a small grant, to work the scheme satisfactorily. It was thought by the Committee of the Section that too many restrictions as to the uniformity of the photographs should not be enforced in the early stages of the scheme. The simple collection and registration of photographs was all that was at present aimed at.

The following suggestions with reference to this subject were forwarded by the Committee of the Section to the Secretary of the Conference :—

"(1) That a Committee be formed, having representatives for each county, charged with the arrangement of a local photographic survey for geological purposes in each district.

"(2) The Committee will gather together—

"(*a*) Names of Societies and individuals who have already assisted in this object, or who are willing to do so;

"(*b*) Copies of geological photographs already taken;

"(*c*) List of localities, sections of rocks, boulders, and other features desirable to be photographed;

and will arrange with local Societies for the work to be done as may be convenient or possible.

"(3) Each photograph to be accompanied by the following particulars :—

"(*a*) Name and position of locality or section;[1]

"(*b*) Details of features shown (with illustrative diagram or sketch whenever necessary for such explanation);

"(*c*) Scale of height and length, or figure introduced to indicate size in Nature;

"(*d*) Name of photographer and Society under whose direction the view is taken;

"(*e*) Date when photographed.

"(4) Size of photograph recommended: 12 × 10 inches (whole plate), but this is not compulsory.

"(5) Original negative to be the property of the Society or individual under whose direction it is taken, and who shall also fix a price at which copies may be sold.

"(6) One copy of each photograph to be the property of the British Association, and one other copy to be given to the Geological Society of London.

[1] Including Compass Direction.—*Sec. Corr. Soc. Comm.*

"(7) Each photograph officially received to be numbered and recorded in a reference-book, and a list published and circulated showing price at which members and others may purchase them.

"(8) A circular to be issued to all geological Societies, inviting their co-operation."

Mr. Jeffs said that a large number of Societies in different parts of the Kingdom had taken, from time to time, photographs of various geological sections and features as they came under their notice, but there had been no systematic way adopted either of collecting the photographs or of recording them, so that geologists interested might really know what had been taken. He thought that, if some arrangement could be made, a great deal of good might be done not only for the benefit of geological science, but also for educational purposes. Regarding regulations, he was not desirous of laying down any strict rules, but he thought that if the scheme were to be carried out at all satisfactorily, and at a minimum expenditure, some few regulations would be necessary.

Mr. Whitaker thought it a very fit subject for the Conference, and trusted that delegates would get their Societies to think it over. The object was to interest all the Societies, and to have a harmonious result.

Some further discussion took place with reference to the requirements of the proposed Committee and the mode of procedure in the field, in the course of which it was pointed out that the chief object was to secure photographs of typical and especially of *temporary* sections. The details of manipulation, the size of the photographs, method of mounting, registration of scale, &c., could only be settled when the Corresponding Societies had taken action in the matter, and the Committee had been formally appointed.

Section D.

The Committee of this Section was represented by Prof. Hillhouse.

Life-histories of Native Plants.—Prof. Meldola said that since their last meeting at Manchester, Prof. Bayley Balfour had received several applications for further particulars with reference to the suggestion which he communicated to last year's Conference. Prof. Balfour was unable to be present at Bath, but had forwarded the following:—

"*Suggestions for those studying the Life-histories of British Flowering Plants*:—

"(1) Seeds should be collected, and opportunity may be taken at the time of collection to note how they are disseminated in Nature—whether the fruit opens or not, whether they have appendages for promoting transport by animals or otherwise, whether they have colour or other features of attraction, &c.

"(2) The seeds being sown, their germination should be watched; its rapidity and manner noted. The variations and differences between albuminous and exalbuminous seeds are worthy of special note. The movements of the parts of the embryo in germination until it acquires its fixed position are also deserving of study. Further, the form of the parts of the embryo is various and instructive.

"(3) The development of the seedling into the adult can be readily watched in annuals and biennials, and smaller perennials. The succession of leaves after the cotyledons should be noted, and the forms which the leaves assume, and their positions and spread. The relative succession of buds in or adjacent to the axils of the later leaves and of the cotyledons should be observed, as also the ultimate fate of the buds developed. This will give a clue to the branching of the main axis of the plant, upon which its whole form and habit depend.

"(4) An important point to look at in the development is the amount, character, and position of any clothing of hairs the seedling may possess.

"(5) The development of the underground part of the seedling must not be neglected. The continuance of the primary root and its branching or its replacement by adventitious roots are points for particular attention, and also the formation upon it of any excrescence or buds. A sufficient number of seedlings must be grown to allow of proper study of these features.

"(6) The form of branching of the stem and leaves may be studied in the mature plant, which may be gathered wild. The formation of false axes should be specially looked for, and the complex relations often resulting from branching may be worked out upon the young top of a mature plant. It is not necessary to wait for the maturing of the seedling, but reference back to the seedling will show whether any observed relations are of late or early development in the life-history.

"(7) In the case of perennials, the mode of perennation is an interesting feature for observation, as well as the methods of vegetative propagation. In some cases the two processes are merged in one. Properly to understand perennation the perennating portions must be examined at all periods of the resting season as well as when they are starting anew into vegetative activity. Seedlings of perennating plants watched during two or three seasons will give a clue towards elucidation of the development.

"(8) When the seedlings begin to form flowers, the relation of the flower-shoots to the vegetative organs should be noted, and especially their sequence with reference to vegetative shoots. The succession of the flowers should be noted, as of course should be their structure and their adaptations to proper pollination. Many seedlings will not, of course, flower for years, and the sequence of flowers in such plants, and, indeed, in all cases, may be well traced in the mature plant growing wild.

"(9) After flowering and pollination the development of fruit must be studied. The parts concerned in forming fruit, the adaptations to scattering of the fruit or seed are points to be precisely noted.

"(10) The presence and position of any nectar-secreting structures outside as well as inside the flower are of much significance, and they should be carefully studied.

"(11) In connection with every point observed of structure and development the observer should ask himself, Why is this? What is this for? and endeavour to obtain some answer to the query.

"(12) A series of observations upon a specific plant made by a careful observer will enable him or her to draw up a complete history of its life, such as is hardly to be found recorded at the present day.

"I may add as a corollary that an interesting field for observation which local Societies might do good work in is that of the relation of plants to animals as food-plants. Some are discarded by browsing animals, others are preferred, and there are degrees of favouritism. Is there any principle of selection?"

Prof. F. O. Bower, the delegate from the Natural History Society of Glasgow, who was unable to be present at the meeting, forwarded a communication with reference to this subject, in which he pointed out that the chief precaution which would have to be observed in the carrying out of observations in accordance with Prof. Balfour's scheme, would be the correct identification of the species being worked upon.

Disappearances of Native Plants.—Prof. Hillhouse said that he was in charge of a Committee appointed two years ago for the purpose of collecting information as to the disappearance of native plants from their local habitats. Their report for 1887 said the Committee intended presenting a report in 1888 concerning its inquiries in Scotland. He came to that meeting prepared with a report, and learnt to his surprise that the Committee had lapsed, but an application had been made to the Committee of Section D to have it reappointed. He would give some brief account of their work in the past year. The report for Scotland covered eighty-five flowers which were extinct, or were "practically extinct," and they were of the most varied kinds. It had been discovered that *Nymphæa alba* (the white water-lily) had been almost exterminated in the lochs about Dumfries; the name of the person who had committed the ravages upon it was brought before the local Natural History Society, an appeal was made to the proprietors of the lochs, and the individual was warned off estates in the neighbourhood on pain of prosecution for trespass. There was one plant that had only a single station in Scotland, *Scheuchzeria palustris*, which was found in the Bog of Methven, and it had been destroyed in all probability by 300 or 400 black gulls settling in the bog and devouring everything in the shape of vegetation. Another plant which had been completely exterminated was one known as *Mertensia maritima*, which grew in shingle on the Bay of Nigg, and which had been destroyed by the shingle having been used to make concrete blocks to be used in the construction of a pier near at hand. Then a grass which grew in a patch near the Moray Frith had been destroyed by the overturning of a tree, which caused a large hole into which all the moisture of the patch drained; this grass was *Melica uniflora*. The Committee found that the disappearance of plants was caused in a great measure by the injudiciousness of individual botanists, and also by botanical exchanging clubs, who held out inducements

for the collection of eighty or a hundred specimens of extremely rare varieties. The Committee hoped to present a report next year.

At the conclusion of the Conference, votes of thanks were passed to the Chairman and Secretary.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—The Sheepshanks Astronomical Exhibition at Trinity College, open to the competition of any undergraduate of the University, has been awarded to Frank Watson Dyson and Gilbert Thomas Walker, brother scholars of the College, who are declared to be equal in merit. The value of the Exhibition is 50*l*. per annum, tenable for three years, and it will be divided between the successful candidates.

SCIENTIFIC SERIALS.

American Journal of Science, December.—The invisible solar and lunar spectrum, by S. P. Langley. This paper, which is an abstract of a memoir about to appear in the publications of the United States Academy of Sciences, summarizes the result of investigations carried on at the Allegheny Observatory in continuation of the author's previous researches on the infra-red of the solar spectrum to the extent of about three microns. By means of the improved apparatus here described, the extreme infra-red solar spectrum has now been searched from three to over eighteen microns; and it is shown that in this region the ratios between solar and lunar heat are completely changed from what they are in the visible spectrum. While the solar *light* in the latter is about 500,000 times that of moonlight, the solar heat received in the invisible part of the spectrum is probably less than 500 times the lunar. These studies also promise important results for meteorology, by opening to observation the hitherto unknown region of the spectrum, in which are to be found the nocturnal and diurnal radiations, not only from the moon towards the earth, but from the soil of the earth towards space.—A brief history of Taconic ideas, by James D. Dana. The Taconic question is here treated in chronological order from 1818 till the present year, in which the controversy may be regarded as practically closed. The conclusion is now firmly established that this system is not pre-Silurian, but merely another name for the older term "Lower Silurian."—Certain generic electrical relations of alloys of platinum, by C. Barus. In this paper are given the chief results of the investigations on the measurement of high temperatures already described in vol. xxxv. p. 407, of the *Journal*. The results generally point to a limit below which, in the case of solid metals and at ordinary temperatures, neither electrical conductivity nor temperature-coefficient can be reduced. It thus appears that a lower limit of both conductivity and temperature-coefficient is among the conditions of metallic conduction, not to say of the metallic state absolutely.—On the Puget group of Washington Territory, by Charles A. White. A careful study of some fossil Mollusca from the coal-bearing formation in the Puget Sound basin, shows that they belong to a hitherto unknown brackish-water fauna, characterizing a deposit of unusual interest. A section of this formation measured at the town of Wilkeson gives a minimum thickness of no less than 13,200 feet, with a probable maximum of 14,500 feet. The surprise caused by the discovery of such an extraordinary thickness in an estuary deposit is increased by the fact that its Molluscan fauna appears to range vertically throughout the whole formation. The fauna itself seems to be of the same age, but distinct from, the Laramie, which flourished, not in an estuary, but in a land-locked basin. The area of the Puget group includes the Cascade Range, but is not otherwise yet clearly defined eastwards from the Pacific seaboard.—Papers are contributed by L. G. Eakins, on some sulphantimonites from Colorado; by A. E. Kennelly, on the voltametric measurement of alternating Currents; by Dr. C. Hart Merriman, on the fauna of the Great Smoky Mountains, with description of a new species of red-backed mouse; by W. E. Hidden and J. B. Mackintosh, on anerlite, a new thorium mineral; and by O. C. Marsh, on a new family of horned Dinosaurians (*Ceratops montanus*) recently discovered *in situ* in the Laramie deposits of the Cretaceous period, in Montana This reptile was a very formidable animal, armed not only with horns of great strength, but with a thick dermal hide, and varying in length from 25 to 30 feet.

SOCIETIES AND ACADEMIES.

London.

Royal Society, December 6.—"Some Observations on the Amount of Light reflected and transmitted by certain kinds of Glass." By Sir John Conroy, Bart., M.A., Bedford Lecturer of Balliol College and Millard Lecturer of Trinity College, Oxford. Communicated by A. G. Vernon Harcourt, F.R.S.

Conclusions.—It seems probable that the amount of light reflected by freshly polished glass varies with the way in which it has been polished, and that, if a perfect surface could be obtained without altering the refractive index of the surface-layer, then the amount would be accurately given by Fresnel's formula, but that usually the amount differs from that given by the formula, being sometimes greater and sometimes less.

The formation of a film of lower refractive index on the glass would account for the defect in the reflected light; but to account for the excess, it seems necessary to assume that the polishing has increased the optical density of the surface-layer, and the changes produced in the amount of light transmitted and in the angle of polarization support this view.

After being polished, the surface of flint glass seems to alter somewhat readily, the amount of the reflected light decreasing, and the amount of the transmitted increasing, whilst with crown glass the change, if any, proceeds very slowly.

There is no evidence to show to what particular cause these changes are due.

The values of the transmission coefficients for light of mean refrangibility for the two particular kinds of glass are given, and show that for 1 cm. the loss by obstruction amounts to 2·62 per cent. with the crown glass and 1·15 per cent. with the flint glass.

Linnean Society, December 6.—Mr. W. Carruthers, F.R.S., President, in the chair.—Mr. W. H. Beeby exhibited and made some remarks on specimens of *Valeriana mikanii* and *sambucifolia*, and a series of *Potamogeton fluitans*.—Mr. F. W. Oliver described the nature and growth of leaf-emergences in *Eriospermum folioliferum*.—Mr. E. M. Holmes exhibited specimens of a new Assafœtida plant (*Ferula fœtidissima*), and a monstrosity of *Zea Mays*.—Mr. J. G. Baker exhibited a curious variety of *Vicia sepium*, found in North Yorkshire.—Mr. T. Christy exhibited specimens of an undetermined species of *Echium* received from Persia, and employed medicinally as a good alterative.—The first paper read was one by Dr. Costerus on malformation in *Fuchsia globosa*, upon which Prof. Bower offered some critical remarks.—The next paper was by Mr. B. T. Lowne, who gave an admirable demonstration of the mode of development of the egg and blastoderm of the blow-fly. His conclusions were criticized by Prof. Stewart, Prof. Howes, and Mr. A. R. Hammond.—In continuation of the Reports on the collections made by Mr. Ridley in Fernando Noronha, a paper was read on behalf of Mr. Boulenger, enumerating the fishes and reptiles which had been identified by him.

Physical Society, December 8.—Prof. Reinold, F.R.S., President, in the chair.—The following communications were read:—Note on a modification of the ordinary method of determining electro-magnetic capacity, by Dr. J. W. W. Waghorne.—On some facts connected with the systems of scientific units of measurement, by Mr. T. H. Blakesley.—Some improved polarizing apparatus for microscopes were exhibited and described by Dr. S. P. Thompson. For polarizer, he uses an Ahrens' prism, and for analyzer a flat-ended one of his own design. The Ahrens' prism is formed from a rectangular block of spar, two faces of which are perpendicular to the optic axis; two cuts parallel to the axis are made from the middle of one side to the ends of the opposite, and the cut faces are polished and cemented by Canada balsam. A short prism with wide angle is thus obtained, which can be readily fitted to the substage of the microscope. The analyzer, which consists of two wedges of spar, is mounted in a tube which fits on the eye-piece, and by recognizing that the upper end need not be larger than the pupil of the eye, the author has been able to considerably reduce the length of the prism, and still keep the bottom end large enough to collect all the rays passing through the eye-piece. Several ingenious methods of cutting spar so as to produce prisms with minimum waste were described and illustrated by models, and a "Nicol" made by the inventor at the age of 79 was exhibited. Mr. Lant Carpenter asked the author why he condemned analyzers placed directly behind the objective, for in

his experience this arrangement gave the most satisfactory results. In reply, Dr. Thompson said his experience was decidedly different from that of Mr. Lant Carpenter, and mentioned that Zeiss had abandoned the common arrangement, and now introduced his analyzers between the two lenses of his Huyghenian eye-pieces.

Chemical Society, December 6.—Mr. W. Crookes, F.R.S., President, in the chair.—The following papers were read :—A method of determining vapour-density, applicable at all temperatures and pressures, by Dr. W. Bott. The apparatus consists of a large Victor Meyer's bulb, carrying a detachable head-piece, which can be connected with the air-pump. The neck of the bulb communicates with a mercury pressure-gauge, which again is connected with a wide measuring tube attached to an adjustable mercury reservoir. The experiment is conducted as follows. The substance having been placed in the head-piece of the vessel, the latter is heated until the volume has become constant. The apparatus is then exhausted as far as may be requisite, and the reservoir so adjusted that the graduated measuring tube is filled with mercury. The pressure indicated by the gauge having been carefully noted, the substance is allowed to drop into the hot part of the vessel, and the surplus pressure produced by its evaporation is removed by drawing off an equivalent volume of air into the measuring tube until the initial pressure in the gauge has been restored. From the volume of gas measured in the graduated tube, the density referred to hydrogen is obtained by the formula $d = 8484893 \frac{S(1 + 0\cdot00367t)}{VP}$; in which S = weight of substance, V = volume of gas in measuring tube, P = pressure of gas in measuring tube, t = temperature of gas. In the discussion which followed, Prof. Ramsay, remarking on a statement made by the author that he proposed to make use of the apparatus in studying the influence of pressure on dissociation, said that recent investigations had shown that the Victor Meyer form of apparatus was by no means a suitable one for the study of such problems ; and he expressed the opinion that for this reason results such as those recently published by Nilson and Petterssen could not be accepted as final.—Some derivatives and new colouring-matters of α-pyrocresole, by Dr. W. Bott and Mr. J. B. Miller. The authors have prepared di- and tetranitro- and di- and tetramido- derivatives of α-pyrocresole oxide. Both amido-derivatives can be diazotized, and the diazo-salts interact with β-naphthol in alkaline solution, yielding two oxyazo-compounds ; these are insoluble in water, but can be converted into soluble sulphonic acids, which dye silk and wool maroon and salmon colour respectively.—Berberine, by Prof. W. H. Perkin. When oxidized with excess of potassium permanganate in slightly alkaline solution, berberine yields, as principal product, hemipinic acid, $C_{10}H_{10}O_6$, as Schmidt and Schilbach (*Arch. Pharm.* [3], 164–170) have already shown. In view of the interesting results lately obtained by Goldschmiedt in his examination of hemipinic and methemipinic acids, the author has carefully re-examined the hemipinic acid from berberine, and is convinced that it is identical with that obtained by the oxidation of narcotine. The acid from berberine contains two (OCH_3) groups : on fusion with potash it yields protocatechuic acid ; and on distilling it with ethylamine, hemipinethylimide, melting at 96°, is formed. This latter substance possesses all the properties of the hemipinethylimide obtained by Liebermann by the action of ethyl iodide on the potassium salt of hemipinimide (from narcotine), and thus the identity of the two hemipinic acids from berberbine and narcotine is proved. Oxidation with a limited quantity of permanganate results in the production of a number of new substances, three of which have been obtained in a state of purity : a new acid, $C_{20}H_{17}NO_9$ (m.p. = 143°), and two neutral substances, $C_{20}H_{17}NO_8$ (m.p. = 236°) and $C_{20}H_{15}NO_7$ (m.p. = 150°), all of which yield protocatechuic acid on fusion with potash. Employing Zeisel's method, the author finds that two methoxyl (OCH_3) groups are present in the berberine molecule.—The action of ammonia on some tungsten compounds, by Dr. S. Rideal.—Condensations of α-diketones with ethylic acetoacetate, by Dr. F. R. Japp, F.R.S., and Dr. F. Klingemann. A preliminary note on the reactions of ethylic phenanthroxylene-acetoacetate, the condensation compound of phenanthraquinone with ethylic acetoacetate.—Thionyl thiocyanate, by Mr. G. C. McMurtry.—Mercuric chlorothiocyanate, by the same.—The action of chromium oxychloride on pinene, by Messrs. G. G. Henderson and R. W. Smith.—Tectoquinone, by Dr. R. Romanis. A continuation of the author's work on the quinone-like compound, $C_{18}H_{16}O_2$, found in teak resin (*Tectonia grandis*), and also in the products of the destructive distillation of the wood. Tectoquinone crystallizes in oblique rhombic prisms of an amber colour, resembling sulphur, melts at 171°, and is a very stable substance, dissolving in nitric and sulphuric acids without change. On reduction it yields a hydrocarbon, $C_{18}H_{22}$, and it is suggested that it may be the hitherto unknown retenequinone.—The decomposition of nitroethane by alkalies, by Prof. W. R. Dunstan and Mr. T. S. Dymond. Nitroethane in contact with potassium carbonate or its aqueous solution readily yields potassium nitrite and an oily liquid having the formula C_6H_9NO, which boils at about 170°, decomposing at a somewhat higher temperature with the formation of what appears to be a pyridine derivative.

Zoological Society, November 20.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the months of June, July, August, September, and October, 1888, and called attention to the acquisition of three specimens of Pallas's Sand-Grouse (*Syrrhaptes paradoxus*), captured out of the many flocks of this Asiatic bird that have lately visited the British Islands.—A letter was read from Prof. J. B. Steere, giving a preliminary account of the "Tamaron," a bovine animal found in the Island of Mindoro, Philippines, which he believed to be allied to the Anoa of Celebes.—Mr. Edgar Thurston exhibited and made remarks upon a collection of Corals from the Gulf of Manar, Madras Presidency.—Mr. H. Seebohm exhibited and made remarks on a specimen of a new species of Pheasant (*Phasianus tarimensis*), obtained by General Prjevalsky at Lob Nor, Central Asia.—Mr. H. Seebohm also exhibited a specimen of a species of Plover new to the British Islands (*Vanellus gregarius*), which had been shot in Lancashire about twenty-five years ago, and had been previously supposed to be a Cream-coloured Courser.—Mr. J. W. Hulke, F.R.S., read a paper on the skeletal anatomy of the Mesosuchian Crocodiles, based on fossil remains from the clays near Peterborough, in the collection of Mr. A. Leids, of Eyebury.—Mr. Oldfield Thomas read a paper on a collection of small Mammals obtained by Mr. William Taylor in Duval County, South Texas. The collection contained examples of one new species and one new geographical variety, besides adding no less than six species to the national collection of Mammalia.—A communication was read from M. L. Taczanowski, containing a supplementary list of the birds collected in Corea by Mr. Jean Kalinowski.

December 4.—Prof. Flower, F.R.S., President, in the chair.—Mr. Howard Saunders exhibited and made remarks on an adult male of the American Green-winged Teal (*Querquedula carolinensis*), shot in Devonshire in 1879.—Mr. Oldfield Thomas gave an account of the Mammals obtained by Mr. C. M. Woodford during his second expedition to the Solomon Islands. The author stated that the total number of species of Mammals now known from the Solomons was brought up by the present collection from 13 to 22, and that of these no less than 8 had been discovered by Mr. Woodford, his previous collection having contained examples of 2 and the present of 6 new species. There were also two new genera of Bats to be added to the one previously described.—Mr. F. E. Beddard read a paper upon the genus *Clitellio*, which had been recently investigated by him at the Marine Biological Station at Plymouth.—Prof. Howes and Mr. Davies read a paper on the distribution and morphology of the supernumerary phalanges in the Anurous Batrachians.—A communication was read from Mr. J. J. Lister, giving a general account of the natural history of Christmas Island, in the Indian Ocean, which he had visited in 1887 as naturalist to H.M. surveying-vessel *Egeria*.—Mr. Oldfield Thomas read a paper on the Mammals of Christmas Island, obtained by Mr. Lister during the same expedition.—This was followed by reports on the Reptiles of Christmas Island obtained during the expedition, by Mr. G. Boulenger ; on the Terrestrial Mollusks, by Mr. Edgar A. Smith, on the Coleoptera, by Mr. C. J. Gahan ; on the Lepidoptera, by Mr. A. G. Butler ; on the other Insects, by Mr. Kirby ; and on the Annelida, Myriapoda, and Land Crustacea, by Mr. R. I. Pocock.

Entomological Society, December 5.—Dr. D. Sharp, President, in the chair.—Mr. W. F. Kirby exhibited, for the Rev. Dr. Walker, a variety of the female of *Ornithoptera brookiana* ; also, for Major Partridge, an undetermined species of the genus *Hadena*, captured last summer in the Isle of Portland.—Mr. R. South exhibited a series of *Tortrix piceana*, L.,

from a pine wood in Surrey; also melanic forms of *Tortrix podana*, S.—Prof. Meldola, F.R.S., exhibited, for Dr. Laver, a melanic specimen of *Catocala nupta*, taken last September at Colchester.—Mr. E. B. Poulton exhibited preserved larvæ of *Sphinx convolvuli*, showing the extreme dark and light forms of the species.—Mr. McLachlan, F.R.S., called attention to a plate, representing species of the genus *Agrotis*, executed by photography, illustrating a memoir by Dr. Max Standfuss, in the *Correspondenz-Blatt*, Verein *Iris*, in Dresden, 1888. He considered it was the best example of photography as adapted for entomological purposes he had ever seen.—The Rev. Canon Fowler exhibited a specimen of *Mycterus curculionides*, L., sent to him by Mr. Olliff, and taken near Oxford about 1882.—Mr. W. Nicholson exhibited several melanic varieties of *Argynnis niobe* and *A. pales*, collected by himself last summer in the Engadine.—Mr. J. H. Leech exhibited a collection of Lepidoptera formed last year at Kiukiang. It included several new species.—Mr. H. Goss exhibited, for the Rev. T. A. Marshall, fifteen undescribed species of British *Braconidæ*.—M. A. Wailly exhibited a collection of Lepidoptera lately received from Assam, containing upwards of thirty-five species of *Papilio*, *Ornithoptera*, *Charaxes*, *Diadema*, *Cyrestis*, and other genera.—Mr. Meyer-Darcis exhibited specimens of *Sternocera tricolor*, Kerr, and *S. variabilis*, Kerr, from Lake Tanganyika; and two new species of *Julodis* from Syria.—Mr. F. Merrifield exhibited, and made remarks on, a long series of *Selenia illustraria*, *S. illunaria*, and *E. alniaria*, in illustration of his paper on "Pedigree Moth-breeding."—Lord Walsingham, F.R.S., exhibited, and made remarks on, a series of species representing the genera *Snellenia*, Wlsm., *Œdematopoda*, Z., and *Eretmocera*, Z.—The Rev. T. A. Marshall communicated a paper entitled "A Monograph of British *Braconidæ*, Part III."—The Rev. Dr. Walker communicated a paper entitled "Description of a variety of the female of *Ornithoptera brookiana*."—Lord Walsingham read a paper entitled "A Monograph of the genera connecting *Tinægeria*, Wlk., with *Eretmocera*, Z." A discussion ensued, in which Mr. Stainton, F.R.S., and Dr. Sharp took part.—Mr. Merrifield read a paper entitled "Incidental Observations in Pedigree Moth-breeding." This paper contained a detailed account of experiments with *Selenia illustraria*, *S. illunaria*, and *E. alniaria*, which, so far as they had yet proceeded, indicated that retardation of development in the growing stages of the larvæ, as well as in the pupal stage, was the cause of the darkening of colour in the perfect insects; and that a low temperature had the effect of causing such retardation. Lord Walsingham, Mr. Poulton, Prof. Meldola, Mr. White, and Mr. Merrifield took part in the discussion which ensued.—Mr. J. H. Leech read a paper entitled "Description of a small collection of Lepidoptera from Kiukiang." Captain Elwes said he had examined this collection with very great interest, and was struck with the similarity of many of the species to those from Sikkim.

Geological Society, December 5.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—Notes on two traverses of the crystalline rocks of the Alps, by Prof. T. G. Bonney, F.R.S. These journeys were undertaken in the summer of 1887, in the company of the Rev. E. Hill, in order to ascertain whether the apparent stratigraphical succession among the gneisses and crystalline schists which the author had observed in the more central region of the Alps, held good also in the Western and Eastern Alps. At the same time all circumstances which seemed to throw any light on the origin of the schists were carefully noted. The author examined the rocks along two lines of section:—(1) By the road of the Col du Lautaret from Grenoble to Briançon, and thence by the Mont Genèvre and the Col de Sestrières to Pinerolo, on the margin of the plain of Piedmont. (2) From Lienz, on the upper waters of the Drave, to Kitzbuhel; besides examining other parts of the central range, east of the Brenner Pass. The specimens collected have subsequently been examined microscopically. The results of the author's investigation may be briefly summarized as follows:—(1) While rocks of igneous origin occur at all horizons among the crystalline series of the Alps, these, as a rule, can be distinguished; or, at any rate, even if the crystalline schists in some cases are only modified igneous rocks, these are associated with recognizable igneous rocks of later date. (2) There are, speaking in general terms, three great rock-groups in the Alps which simulate curiously, if they do not indicate, stratigraphical sequence. The lowest and oldest resembles the gneisses of the Laurentian series; the next, those rather "friable" gneisses and schists called by Dr. Sterry Hunt the Montalban series; the third and uppermost is a great group of schists, generally rather fine-grained, micaceous, chloritic, epidotic, calcareous, and quartzose, passing occasionally into crystalline limestones, and (more rarely) into schistose quartzites. (3) The Pietra Verde group of Dr. Sterry Hunt, so far as the author has been able to ascertain, consists mainly of modified igneous rocks, of indeterminable date, and is at most only of local, if, indeed, it be of any classificatory value. (4) Of the above three groups the uppermost has an immense development in the Italian Alps and in the Tyrol, north and south of the central range. It can, in fact, be traced, apparently at the top of the crystalline succession, from one end of the Alpine chain to the other. (5) The middle group is not seldom either imperfectly developed or even wanting, appearing as if cut out by denudation. It was not seen in the traverse of the Franco-Italian Alps, except perhaps for a comparatively short distance on the eastern side, being probably concealed by Palæozoic and Mesozoic rocks on the western side. It is not very completely developed in the Eastern Tyrol, and seems to prevail especially in the Lepontine Alps, and on the southern side of the watershed. (6) The lowest group is fairly well exposed, both in the French Alps and in the Central Tyrol. (7) As a rule, the schists of the uppermost group had a sedimentary origin. The schists and gneisses of the middle group very probably, in part at least, had a similar origin. In regard to the lowest group it is difficult, in the present state of our knowledge, to come to any conclusion. (8) The slates and other rocks of clastic origin in the Alps, whether of Mesozoic or of Palæozoic age, though somewhat modified by pressure, are totally distinct from the true schists above mentioned, and it is only under very exceptional circumstances, and in very restricted areas, that there is the slightest difficulty in distinguishing between them. The evidence of the coarser fragmental material in these Palæozoic and later rocks indicates that the gneisses and crystalline schists of the Alps are very much more ancient than even the oldest of them. (9) The remarks made by the author in his Presidential address, 1886, as to the existence of a "cleavage-foliation" due to pressure, and a "stratification-foliation" of earlier date, which seemingly is the result of an original bedding, and as to the importance of distinguishing these structures (generally not a difficult thing), have been most fully confirmed. He is convinced that many of the contradictory statements and much of the confusion in regard to the origin and significance of foliation are due to the failure to recognize the distinctness of these two structures. In regard to them it may be admitted that sometimes "extremes meet," and a crystalline rock pulverized *in situ* is very difficult to separate from a greatly squeezed fine-grained sediment; but he believes these difficulties to be very local, probably only of a temporary character, and of little value for inductive purposes. After the reading of this paper, the author's conclusions were discussed by the President, Mr. Teall, Dr. Hicks, Mr. Bauerman, Prof. Blake, and Dr. Geikie.—On fulgurites from Monte Viso, by Frank Rutley.—On the occurrence of a new form of tachylyte in association with the gabbro of Carrock Fell, in the Lake District, by T. T. Groom. Communicated by Prof. T. McKenny Hughes.

PARIS.

Academy of Sciences, December 10.—M. Daubrée in the chair.—Observations of Faye's comet, made at the Marseilles Observatory, with the 0·80 m. Foucault telescope, by M. Stephan. These observations cover the period from December 5-8, when the nucleus was equal to a star of the eleventh or twelfth magnitude.—Geographical work in Brazil, by M. L. Cruls. The Imperial Observatory of Rio de Janeiro having undertaken to determine the geographical positions of a number of stations on the railway between the capital and Sabara, the results are given for the first two stations of Rodeio and Entre-Rios.—On the application of the theta functions of a single argument to the problems of rotation, by M. F. Caspary. A *résumé* is given of the calculations which the author has worked out with a view to determining the formulas relative to the problem of rotation of a heavy body suspended at a point of its axis.—On a general proposition regarding linear equations with partial derivatives of the second order, by M. Emile Picard.—On the employment of oxygenated water for the quantitative analysis of the metals of the iron group: (1) chromium, by M. Adolphe Carnot. On the solutions of various metals of the iron group oxygenated water determines certain reactions, sometimes of a reducing, sometimes of an oxidating, character. This property is capable of being

turned to account in chemical analysis, and the author proposes to deal successively with chromium, manganese, iron, cobalt, and nickel. In the present paper he shows the remarkable reaction furnished by chromic acid with oxygenated water, known as the Barreswil reaction.—On a latex of *Bassia latifolia*, Roxb., by MM. Edouard Heckel and Fr. Schlagdenhauffen. This plant, the well-known *Mohwa* of British India, is found to yield by incision a latex capable of supplying a kind of gutta-percha. When evaporated to about one-fourth of its volume, the sap furnishes an adhesive mass in the proportion of 6·67 per cent., which is partly soluble in alcohol and acetone, and which in the insoluble state leaves 27·027 per cent. of a gutta, the composition and industrial properties of which will form the subject of a future memoir.—On some new or little-known Infusoria, by M. J. Kunstler. Several minute intestinal parasites of various animals are described, including a remarkable ciliated Infusorium peculiar to *Periplaneta americana*.—On the *coussinet* (*cushion* or *pad*), a new organ attached to the sting of the Hymenoptera, by M. G. Carlet. This organ, here for the first time described, appears to be a sort of pivot round which the sting revolves, preventing this weapon from adhering to the teguments, and facilitating its movements. But the chief function of the "pad" is to retain in the sacs of the trachea the supply of air necessary for their inflation. This it effects by facilitating the action of the operculum, which thus appears to be a veritable safety-valve in the abdominal region.—On the measurement of the large bones in the human system, and on its applications to anthropological and medico-legal questions, by M. Etienne Rollet. The results are given of the measurements, made with Broca's apparatus, of the large bones of fifty men and fifty women lately deceased in the hospitals of Lyons. Much asymmetry was discovered between the bones on the right and left sides of the skeleton. An attempt made to determine stature from the size especially of the femur and humerus yielded satisfactory results. Compared with those of negroes and negresses, these measurements show generally that in the black race the upper and lower members, especially tibia and radius, are longer than in the South European, the difference being more marked in the female than in the male sex.—On the phosphated deposits of Montay and Forest, Département du Nord, by M. J. Ladrière. The author describes the origin and composition of these deposits, which in some places are rich enough to be worked with profit.—The dislocations of the primitive formations in the north of the central plateau of France, by M. L. de Launay. The lacustrine and independent origin of the various coal basins in this region is generally admitted by modern geologists. Here the author goes further, and endeavours to determine the causes to which was due the creation of the pre-Carboniferous lakes themselves, as well as their actual position on the plateau. This study is largely based on an entirely new and minute examination of the foldings to which the gneisses and mica-schists of the underlying systems have been subjected. The history of these movements, comprising a considerable number of progressive phases, shows that the successive dislocations invariably took place in the same direction, each great disturbance being merely an intensified repetition of the preceding. The general result in this region was to connect in one vast V-shaped system the Breton and Morvan foldings running respectively in the directions from north-east to south-east and from north-east to south-west.

AMSTERDAM.

Royal Academy of Sciences, November 24.—M. Hugo de Vries read a paper on the pangenesis of Darwin, expressing the conviction that Darwin's doctrine presents a great many more data for the explanation of various phenomena in the domain of heredity than the doctrine of Weismann. The author especially tried to demonstrate that the hypothesis of the transport of gemmules may be rejected without endangering the validity of the arguments implicated in that hypothesis, which would connect the separate properties of any organism with some definite species of particles of living matter. He also pointed to the fact that the theory expounded after Darwin's time, according to which the nucleus of the germ-cell must be the seat of heredity, is in accordance with the import of the last-mentioned hypothesis.—On tænodal points, by M. Korteweg. The author treated of their first appearance and disappearance on a gradually deformed surface. There exist four kinds of singular points of the first order of exceptionality, where two or more tænodal points come together, viz. two quite different species of double tænodal points—oscular points and conical points. When a double tænodal point occurs on the variable surface, a couple of tænodal points pass from reality to non-reality, or *vice versâ*. An oscular point is not accompanied by any change in the number of real tænodal points. In a conical node, as many couples as there are real double lines (six at most) at the conical node of the cubic surface obtained by neglecting all terms of the fourth order and higher, become real or imaginary according to the direction of the deformation. The other couples (six at least) cannot emerge from non-reality. As an immediate result of his general theory, the author deduces the theorem: The difference between the number of real tænodal points and real lines is the same for every cubic surface, and equal to three.—M. Martin showed that the lower jaw, found in the year 1823 when digging the canal called "Luid-Willemsvaart," in the Kaberg, near Maestricht, and hitherto regarded as the remains of a so-called fossil or diluvial human being, was not found in the geological formation which harbours such remains, but in another of more recent date, so that the importance of this jaw—found by the author after a long and troublesome search in the anatomical cabinet of the Leyden University—can no longer be maintained. —M. Martin stated that he had discovered recently, in a parcel of petrifactions collected by the mining engineer, J. A. Hooze, in Martapoera, some characteristic fossils from the chalk-formation; so that it is positively ascertained that in the south-eastern part of Borneo there exists a chalk-formation, as was formerly supposed by Geinitz.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Seas and Skies in many Latitudes: Hon. Ralph Abercromby (Stanford).—The Region of the Eternal Fire, Popular Edition: C. Marvin (Allen).—The Floral King; a Life of Linnæus: A. Alberg (Allen).—The British Journal Photographic Almanac, 1889 (Greenwood).—Nature's Fairy-Land, 2nd edition: H. W. S. Worsley-Benison (E. Stock).—Hand-book to the Optical Lantern: Welford and Sturmey (Iliffe).—The Blowpipe in Chemistry, Mineralogy, and Geology, 2nd edition: Lieut.-Colonel W. A. Ross (Lockwood).—Round about New Zealand: E. W. Payton (Chapman and Hall).—Through the Heart of Asia, 2 vols.: G. Bonvalot (Chapman and Hall).—Natural History Collections made in Alaska between the Years 1877 and 1881: E. W. Nelson (Washington).—Our Rarer Birds: C. Dixon (Bentley).—The Alps: Prof. F. Umlauft, translated by L. Brough (K. Paul).

CONTENTS.

THURSDAY, DECEMBER 27, 1888.

THE BUTTERFLIES OF THE EASTERN UNITED STATES AND CANADA.

The Butterflies of the Eastern United States and Canada, with Special Reference to New England. By Samuel H. Scudder. Part I. (Cambridge, Mass., U.S.A. : Published by the Author, 1888.)

THIS is perhaps the most remarkable work on butterflies which has ever been published; and though it has some features which cannot meet with universal approval, it will make a mark in entomological literature which cannot fail to influence future writings. It has, as the author tells us in his prospectus, been twenty years in preparation, of which eight have been entirely given up to it, and embodies thirty-five years of experience in the field, as well as an immense deal of literary research. The result is certainly a work of which, notwithstanding its defects, both the author and his countrymen may well be proud; and considering that it is published at the sole cost and risk of Mr. Scudder himself, who informs me that a sale of 350 copies is necessary before the cost of production can be repaid, it is to be hoped that scientific societies and entomologists in all parts of the world will support his arduous undertaking by subscribing to it The work is a large quarto, and will be completed in twelve monthly parts, each containing eight plates, coloured and plain, and about 144 pages of text. Of the plates, seventeen are to be devoted to butterflies, six to their eggs, eleven to caterpillars, two to the nests of caterpillars, three to chrysalides, two to parasites, thirty-three to structural details, nineteen will be maps and groups of maps, illustrating the geographical distribution of butterflies, and three are portraits of early American naturalists,—in all, about 2000 figures on ninety-six plates, together with over 1700 pages of letterpress. Considering that both letterpress and plates are of a high character—the chromolithographs by Sinclair and Son, of Philadelphia, being the best I have ever seen, and far superior in detail, fineness, and accuracy to many hand-coloured plates—and that the uncoloured plates are often of microscopical details which require the greatest care and accuracy, I do not think that the price, which is 5 dollars a part, or 50 dollars for the entire work if the whole is paid before January 1, 1889, is too high; though it will certainly place the book beyond the means of many who would wish to possess it. When complete, which will probably be in the course of the next year, the work will only be sold bound in three volumes at 75 dollars, so there is a decided advantage to early subscribers.

From the systematic list at the end of the first volume it appears that the number of species recognized as occurring in New England is about 124, to which 42 not found within these limits will be added in the appendix; so that the amount of space devoted to each species is very much larger than in any other work on butterflies with which I am acquainted. A great deal of the work, however, is taken up with detailed descriptions of the eggs, larvæ, chrysalides, and imagos, which seem to me to be of unnecessary length when accompanied by so many and such good figures. There are also full analytical tables of the families, genera, and species, based on the characters, not only of the imago, but also on those of the egg, larva, and chrysalis, which is a feature not attempted to anything like the same extent in any previous work; though I am somewhat doubtful whether its practical utility is in proportion to the labour it entails both on author and student. How far these tables will prove useful and correct when applied to species and genera not found in New England, and therefore not examined with the same care by the author, is another question; for it appears to be one of the gravest weaknesses of this work that it attempts to deal in a systematic way—far more minute than any which has hitherto been thought possible—with the species of a very limited fauna; apparently without sufficient consideration of the very much more numerous, and probably more variable, allied species and genera found in other parts of the world.

It has long been known that Mr. Scudder's views on nomenclature are peculiar to himself; and looking on nomenclature, as I do, as merely a means to an end, and of very minor importance provided the same names are used for the same objects by all naturalists, I regret deeply that the utility of such a work as this should be marred to some extent, by the fact that the generic names are in many cases used by no other American or European lepidopterist but Mr. Scudder himself. To such an extent does this peculiarity of nomenclature prevail, that out of seventy-six generic names used for 124 species of butterflies occurring in New England only twenty are in general use; nine or ten more are in partial use; and the remainder are mostly the fancies of Hübner—which have been practically ignored by recent systematists—or the creations of Mr. Scudder himself.

The specific names, however, are happily in most cases the same as those used by Edwards, Strecker, and other lepidopterists; and the English names, of which there is a pretty variety, may be used by those who are amused by them and do not wish to be understood by others.

What is really delightful in this book, and what makes it a monument of industry, care, and patience, is the way in which the life-history, transformations, and habits are worked out; in many cases at a cost of numerous journeys undertaken for this special purpose to remote and difficult parts of the country.

To show the style of the book, we may take the article on "*Œneis.*" First we have four pages devoted to the genus *Œneis*, of which two and a half are descriptive of the imago, egg, larva, and chrysalis; but no allusion is made to the species on which this generic description is based, and nothing, unfortunately, is said as to the allied (some of them very nearly allied) species found elsewhere. This is a grave defect, as, however confident Mr. Scudder may be that *Œ. semidea* is peculiar to the United States, it has at least several congeners of fully equal interest in the Arctic region, a sketch of whose distribution could not be out of place, or without interest to his readers, and would certainly be of more value in almost all cases than very wordy non-comparative descriptions, which nine readers out of ten will entirely overlook. Then we have an excursus, of which there are many interspersed through the work, of eight

pages, giving a charming and life-like account of the White Mountains of New Hampshire and the butterflies found there. This seems to be one of Mr. Scudder's most happy hunting-grounds, and is the only known home, with the exception of the Alps of Colorado, of *Œ. semidea*, the White Mountain butterfly, whose history comes next, and takes up no less than sixteen pages. These include the synonymy and references, which are full and appear accurate; two snatches of poetry (which, by the way, is freely scattered throughout the work); a description of the imago, covering two whole pages of close print; others of the egg, larva, and chrysalis, which take two more; geographical distribution occupies another page. The remainder is devoted to life-history and habits, and ends with a paragraph mentioning the desiderata left to be filled up before our knowledge of the history of the insect is complete.

Not so satisfactory, in my opinion, is the history of *Cercyonis* (*Satyrus*) *alope* and its near congener *nephele*, because, in the first place, not a word is said, in a generic, description extending over three pages, to show how this genus differs (if it does differ) from the genus *Satyrus* or *Hipparchia*, in which the American species are included by other authorities; and, secondly, nothing is said as to whether this generic description is based on the two forms which alone are found in New England, or on the characters of other North American species; which, indeed, are not even named, though their existence is alluded to. As an instance of the difficulty of getting any agreement as to what constitutes a species among butterflies, even when they have been bred as largely, and studied as closely, as this species has been by W. H. Edwards, I may quote Mr. Scudder's remarks:—

"It has been generally conceded of late years that these two types of butterflies were only dimorphic forms of a single species; and I have myself shared in this view, which has been supposed proven by breeding experiments and direct comparisons of a large amount of material made by Edwards, who, far more than all other observers together, has increased our knowledge of the natural history of these butterflies.

"He has instituted comparisons between them at every stage of life; and while he sees differences between caterpillars and chrysalids born of different types, he finds no constant and universal distinctions; while as to the relation of the early stages of the butterflies, he has proved by breeding that 'south of the belt of dimorphism,' as he calls that strip of country where *C. alope* and *C. nephele* both occur, '*alope* produced *alope*,' but inside the belt, *alope* produced intergrades, and *nephele* produced *alope* and also an intergrade. . . . That *nephele*, north of the belt, breeds true, is certain, because the intergrades and *alope* are not found here.

"This would be conclusive if the complete parentage in each case were known; but, as only the mother was known in any case, another explanation is not only possible, but in view of all the facts probable. The intergrades found throughout the belt forming the northern boundary of the typical *alope*, and the southern boundary of the typical *nephele*, seem to be far more easily explainable on the hypothesis of hybridism, since they occur only where such a phenomenon is possible, and wherever it is possible. The same argument applied to the case of *Basilarchia*, as has been done by Edwards, would logically prove more than he would agree to, viz. the specific identity and trimorphism of all the eastern species except *Basilarchia archippus*. That the species of *Cercyonis* here described are certainly distinct, I would by no means maintain; only that, in view of the facts of distribution, it seems more probable that they should be looked upon as having reached in their development the stage of specific distinction, whilst they are readily fertile *inter se*, and produce intergrades, where they meet on common ground."

It seems to me, however, that there is yet another explanation, which, from what we know of the effect of heat and damp on the variation of butterflies of the same family in India, is even more probable; and that is, that the species is one which, having a wide range of distribution, is affected in the southern part of this range by climatic conditions which do not exist in its northern habitat, and has become modified in consequence, whilst in the central part of its range, the climatic conditions being more variable, the insect is also itself more variable. The most ardent devotee of minute subdivision of species cannot fail to allow, after Mr. de Nicéville's experiments on breeding in Calcutta, that climate can and does produce in the same locality, at different seasons, changes which are far greater than the difference which exists between *alope* and *nephele*, a difference which can be matched in other species of Satyridæ, about the specific identity of which there has hitherto been little or no question.

It does not seem to me logical for Mr. Scudder to treat of these two forms as different species, when he allows the specific identity of such forms as *Cyaniris* (*Lycæna*) *pseudargiolus*, *lucia*, *violacea*, and *neglecta*; this view being based, as it must be, on similar breeding experiments, carried out by the same naturalist, who proved to his own satisfaction and to mine the identity of *alope* and *nephele*.

A marked and novel feature of this work, which I cannot too highly praise, is the separated map of geographical distribution given on Plate 18 for most of the species dealt with in this part. On a small chart of the United States and Canada, the range of each species is coloured in brown, so that one can see at a glance what the distribution is; and though, no doubt, in the less known parts of the country these maps are not strictly exact, yet they give a very fair idea of what would otherwise require much reading to understand.

I look forward with the greatest interest to the succeeding parts, and to the early completion of, this most valuable work, which will take a high place among biological monographs, and will rank like Edwards's "Butterflies of North America," as one of the most important, beautiful, and painstaking books which America has ever produced

H. J. ELWES.

POLE'S LIFE OF SIEMENS.

The Life of Sir William Siemens, F.R.S. D.C.L., LL.D. By William Pole, F.R.S. (London: John Murray, 1888.)

IS it perhaps to the spirit of this book-making age that we ought to attribute the fact that examples of unsatisfactory biographies have been frequent in recent years? Perhaps works of this kind are too hurriedly compiled, and are laid before the friends and the public at a date too early to allow of such a memorial proving really satisfactory to the one class or to the other.

The compilation of a biography is almost proverbially difficult. To give a true and unbiased account of a life that has passed away, to show *the man* as he was and as he ought to be known, requires at all times delicacy, and tact, and peculiar ability in the writer to enter into the spirit of the life. The biography of a scientific man and of a great public character requires special gifts besides. But when the friend, the public man, has but recently stood among us, the difficulty is greatly enhanced. To hold the middle course between disclosing too much and too little; to avoid entering into particulars which might prove offensive to friends, or injurious to the material interests of those who are left to carry on the work of him who is gone, and yet to make the biography something more than a mere lifeless catalogue of events and undertakings, of successes and failures—to do this is difficult under any circumstances, almost impossible except after the lapse of many years.

We cannot feel that Mr. Pole's efforts have been crowned with entire success. Undoubtedly he has not erred on the side of indelicately disclosing what should not be made public. But above all things Siemens was sociable and friendly, domestic and hospitable, and ready to throw himself into the concerns of others, whether personal or scientific. This man, always kindly, always lovable, we find too little of in the biography now before us.

We cannot think that the form of writing which Mr. Pole has adopted is happy or advantageous. Chapter III., which gives an account of Siemens's school and college days, will probably be found, by general readers the most interesting part of the book. This is simply on account of the continuity in its style. The remaining chapters are divided into short sections, each with a separate heading in capitals or italics—like an American newspaper. Each one of these little sections gives an account of the progress of some invention during two or three years perhaps. The story then breaks off, and another invention is put before the reader. At the end of the chapter comes a short paragraph headed *Domestic Life*. Then the round recommences. Two or three hundred pages of these paragraphs leave the mind in a state of perfect bewilderment. We admit the difficulty of giving a continuous and interesting account of the life of this many-sided man; but we do not think it has been lessened by this method of treatment.

The letters also which are printed, with the exception of those from Dr. Werner von Siemens, the Berlin brother, are very disappointing. The remainder—from the Shah and other Princes, and from Ambassadors and secretaries of great men—are absolutely without interest. The same must be said of the pages of little obituary notices, many of them three lines long, from the morning and weekly newspapers.

Turning to the subject of the biography, our task is more congenial. A very chequered life lies before us, so far as anxiety and happiness are concerned—great failures, great successes, difficulties which to most men would have proved insuperable, enthusiastic determination and indomitable courage in this man which overcame them all, a life-long struggle steadily growing to remarkable success.

To those who knew Sir William Siemens only as the successful engineer of Palace Houses or the hospitable owner of Sherwood, as President of the British Association or of the Society of Telegraph-Engineers, it is instructive and interesting to trace his early days of mixed failure and success. His ingenuity and inventive power were very striking. At the age of twenty we find him making inventions in connection with electro-plating, governing of steam-engines, printing, &c., patenting them and selling the patents in England. His knowledge at this early age was, naturally, not equal to his enthusiasm and to his inventive fertility. The results obtained were by no means always satisfactory. Sometimes he made a little money: as often what he made by one invention was swallowed up almost to the last penny in endeavouring to realize or to bring forward something new.

Siemens's first undoubted success was his water-meter, in 1852. He had already been engaged in several important undertakings, besides having, early in life, taken many patents, to which we have just alluded; and he had invented and realized his regenerative heating, which subsequently became of the highest importance. But the water-meter supplied a real need in a thoroughly satisfactory way; and it was immediately taken up and yielded him a handsome income. With its success commenced the thorough success of its inventor, and he was thus, as Sir William Thomson remarks, "enabled eventually to find his home among us, and to give us primarily the benefit of his great inventiveness in all directions." It is interesting to chronicle this result, for there are many, to whom the name of Siemens is almost a household word, who have never so much as heard of the invention.

The two great engineering labours with which Siemens's name will always remain associated are electric telegraphy and regenerative heating. With regard to the former, the initiative seems to have come from Berlin, where his elder brother, Dr. Werner von Siemens, had commenced an electrical business about 1844. This business at first consisted in designing and making telegraph instruments; but soon the construction of land lines became a part of the work. About 1848, William Siemens was appointed agent in England for the Berlin firm; and his work grew with its growth. The time was, of course, opportune in the extreme. Soon we hear of the Berlin firm undertaking enormous land line contracts; and, naturally, when the time came, the English firm, which had arisen out of the agency of 1848, commenced to take part in the prodigious English work of girdling the earth with submarine cables. The history of these vast undertakings is most interesting; but unfortunately it is marred in the book before us by the misfortune of being scattered over many chapters, mixed up with a host of matters comparatively unimportant.

With regard to regenerative heating, we cannot do much more than remark that its importance is probably not yet fully realized. One great difficulty Siemens had to contend with was the cheapness of fuel! When he attempted to introduce his method among the salt manufacturers, it was scarcely worth their while to make the necessary changes in their evaporating plant so long as fuel could be so easily obtained. In works on the large scale, such as iron-making and glass-making, the improvements introduced by him are already appre-

ciated; and unfortunately the days cannot be very far distant when economy in fuel will become even more necessary than it is now.

Siemens, as is well known, had greatly at heart the subject of smoke abatement. He applied his principles to the construction of domestic fires, which are the main causes of smoke and fog in many of our large towns. His improvements have not yet to any considerable extent been adopted, but it is greatly to be desired that a reform in this direction should speedily be brought about.

To describe his labours in connection with the introduction of the electric light, the electric furnace, electric transmission of power, electric propulsion on railways, would be quite beyond our limits. We can only refer our readers to Mr. Pole's account of these subjects. The necessary explanations are given with admirable clearness, difficult as it is to compress them into moderate limits; and the book, in spite of our strictures at the commencement of this notice, will be found full of instruction and interest.

SOME PALÆOZOIC DIPNOAN FISHES.

Fauna der Gaskohle und der Kalksteine der Permformation Böhmens. Band II. Heft 3, Die Lurchfische, Dipnoi, nebst Bemerkungen über silurische und devonische Lurchfische. Pp. 65–92, Pls. 71–80. Von Anton Fritsch. (Prag: in Commission bei Fr. Řivnáč, 1888.)

ANOTHER part of Dr. Anton Fritsch's well-known work upon the Vertebrate fauna of the Permian rocks of Bohemia has lately appeared, and the description of the fishes is thus commenced. The nature of the subject does not admit of the introduction of so many novelties as characterized some of the previous parts; but the interest and value of the work is fully sustained, and the discussion of the characters of *Ctenodus*—the only known Dipnoan fish of the Bohemian Gas-coal—is supplemented by some remarks upon a few of its Palæozoic allies, with special reference to the supposed evidence of Dipnoans from the Upper Silurian of Bohemia. In addition to the ten beautifully-executed plates, the text is accompanied by numerous woodcuts, and no less than ten of these represent illustrative fossils that are not Bohemian, while six are devoted to important features in the skeletal anatomy of the living *Ceratodus*.

Dr. Fritsch commences by emphasizing the intimate relationship existing between the genera *Ctenodus* and *Ceratodus*; and each portion of the skeleton of the Permian fish, so far as determinable, is then compared in detail with the corresponding element in the existing genus. The fossils, unfortunately, are for the most part fragmentary, almost all the head-bones being scattered, and none of the bones of the trunk and fins being discovered in natural series; but many can be identified with considerable certainty when rigorously compared according to the author's method.

In the skull of *Ctenodus* there are several ossifications in parts that remain permanently cartilaginous in *Ceratodus*, and the dermal roof-bones are much more numerous than in the last-named genus. A bone that was formerly described as the pelvis of a Stegocephalian is now recognized as the squamosal of *Ctenodus*. There is no certain evidence of maxillæ and premaxillæ; and the mandible exhibits possibly another feature of close agreement with *Ceratodus* in the small size and scale-like character of the bone named dentary by Huxley. Dr. Fritsch considers that the latter element is too insignificant to represent the dentary, and may thus be more appropriately termed "dermomental"; but his argument appears to us far from satisfactory.

An interesting point is remarked upon in connection with the dentition of *Ctenodus*. The commonest of the two species recognized at Kounová was originally founded upon the evidence of detached teeth, and named *Ceratodus barrandei*, Fritsch; but it is now identified with the well-known English Coal-Measure species, *C. obliquus*, Hancock and Atthey. The teeth vary so much in size and so little in characters that Dr. Fritsch represents a series to illustrate several stages in the life-history of the fish; and the small teeth named *C. elegans*, Hancock and Atthey, thus appear to pertain merely to young individuals of *C. obliquus*.

Proceeding to a discussion of the axial skeleton of the trunk, Dr. Fritsch finds evidence of the persistence of the notochord, with the same arrangement of the neural and hæmal arches as is met with in *Ceratodus*. In regard to the parts of the appendicular skeleton, however, satisfactory comparisons are as yet impossible; though it is considered likely that, in the pectoral arch, *Ctenodus* exhibited a greater number of distinct elements than the existing genus.

The scales of *Ctenodus* are large, thin, and round, and the outer surface of each "appears smooth in the middle, and is only seen to be rugose when highly magnified. The border exhibits concentric lines of growth, of varying width, parallel to the margin. Across these extend small parallel ridges, on the middle of which are rows of minute pits, apparently indicating the spots that originally supported denticles." Another noteworthy feature is the forked appearance of the sensory canal upon a detached scale of the lateral line—a condition unknown in *Ceratodus*.

As the result of his researches, Dr. Fritsch concludes that the Bohemian examples of *Ctenodus obliquus* must have attained a length of about 140 centimetres. In every part of the skeleton there is evidence of more complete ossification than is observable in the existing *Ceratodus*; and the occurrence of a greater number of dermal roof-bones in the skull of *Ctenodus* as compared with that of the living Dipnoan is a parallel case to that of the Amphibia previously discussed,—the Permian groups having the skull better armoured than their allies at present existing.

After defining the teeth of a new species (*C. applanatus*) from the Gas-coal of Kounová, and having also briefly noticed another form (*C. trachylepis*) known only by three scales from Nyřan, Dr. Fritsch proceeds, in conclusion, to treat of some of the remains of Dipnoan fishes met with in the Devonian and Silurian, mostly of the Continent. A new genus and species, *Dipnoites perneri*, is indicated by a supposed head-bone from the Upper Silurian of the neighbourhood of Prague. A new and more satisfactory figure of the type-specimen of *Gompholepis panderi*, Barrande, is next given; and this, too, is regarded as

a dermal bone of the cranial roof of a Dipnoan, an interesting bone of *Ctenodus* being figured for comparison. The Old Red Sandstone fossil, named *Phyllolepis concentricus* by Agassiz, is also interpreted as probably the head-bone of a closely-allied fish; and the remarkable skull-fragment known as *Archæonectes pertusus*, H. von Meyer, is considered to be the bony portion of the palate, wanting the teeth.

To the appended synopsis of the literature of the subject, we might add two important papers on *Ctenodus*, by W. J. Barkas, published in the Proceedings of the Royal Society of New South Wales, 1876–77; and the list of known Palæozoic Dipnoan fishes must be reduced by one (*Strigilina*), which is founded upon a tooth of a Selachian. We would also remark that the so-called *Campylopleuron* is almost certainly founded upon the tail of *Ctenodus*, as pointed out by Dr. Traquair (NATURE, vol. xviii. p. 483). In every respect, however, the memoir affords evidence of the most elaborate and painstaking research; and it must long remain a standard work of reference for all who are interested in the palæontology of the Dipnoan fishes.

A. S. W.

OUR BOOK SHELF.

A Revision of the Heterocyst Nostocaceæ. By Ed. Bornet and Ch. Flahault. (Separate reprint with index.) (Paris, 1886–88.)

IN this very important contribution to our knowledge of this interesting group of Algæ, the publication of which commenced in the third volume of the *Annales des Sciences Naturelles*, vii. série (1886), and was concluded in the seventh volume, published this year (1888), we have a work of great labour and research, upon the happy completion of which the authors are to be congratulated.

Those who have investigated the forms of these Algæ will remember the extreme difficulties they have experienced in determining the so-called species of many of the authors whose works they were obliged to consult. Too often it has happened that, unable to recognize a form collected, it has been described as new, and so but added to the big pile of synonyms.

Accepting the division of the hormogonous Nostocs into the two subdivisions of those with "uniform cells" and those with "dissimilar cells," it is with the latter group that the present "Revision" has to do. For many years the authors have investigated all the examples, living or dried, that they could procure. The herbaria of Brébisson, Chauvin, Grunow, Lenormand, Thuret, and the collections of the Museum of Paris, have all been consulted, with the result that an immense mass of described species have been treated as synonyms; while a certain number, of which type-specimens were not to be had, or on account of difficulties of referring them to known genera, have been enumerated as "species inquirendæ."

Introductory to the description of the genera and species we have an account, up to the present state of our knowledge, of the vegetative cells, the filaments, and the trichomes, the outer envelope (cytioderm), the heterocysts, the ramification, the hormogonia, and the spores. Of these two latter modes of reproduction, that by the "hormogones" has been the longest known, and is the one to be found in most of the genera; while that by "spores," destined to preserve the species during the intervals of vegetation, and enabling it to resist desiccation, is known only to occur in some few of the genera, but the authors add "that it is permissible to think that it will soon be known to occur in all." These spores are easily distinguished by their size, their more rounded form, and their more marked granular contents. They often possess a brownish-yellow epispore; in some instances they have been proved to retain their vitality for a considerable time; the spores of *Cylindrosperum licheniforme*, Kutzing, have germinated after a nine years' sojourn in a dried state, in an herbarium. About their behaviour just at the period of germination we have still something to learn. The "conidia" of Borzi are also alluded to. A conspectus of the genera of each of the four tribes of the sub-family is given; the four tribes recognized being the Rivulariaceæ, Sirosiphoniaceæ, Scytonemaceæ, and Nostocaceæ. After the list of genera comes the list of species, with analytical keys and detailed diagnosis of each. There is a very full account of the geographical distribution of all the species.

In a notice the main object of which is to call the attention of our readers interested in these for the most part fresh-water Algæ, it would not be proper to enter into minute details, so we will content ourselves with a statement of our belief that this memoir of Dr. E. Bornet and M. Flahault will not only do very much to assist the botanist, but will also go far to awaken a new interest in a group of plants in which there is abundant field for further research.

E. P. W.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Mr. Spottiswoode's Mathematical Papers.

AT the request of Mrs. Spottiswoode, I have undertaken to edit these papers. Mrs. Spottiswoode has kindly looked over her collection, and put into my hands author's copies of such papers as she has. I still lack the following, which some one or more of the late President's friends may perhaps be able to lend me. The numbers are those of the Royal Society's Catalogue.

I. *Phil. Mag.*, xxxvi. (1850); VI. *Crelle*, xlii. pp. 169–78; VIII. Tortolini, *Annali*, iii. (1852); IX., X. *Camb. and Dub. M. Journ.*, viii. (1853); XIII. R.S. Proc., vii. (1854); XX., XXI. same, xi. (1860); XXII. [as regards Geog. Soc. Proc., v. (1861)]; XXIV. Brit. Assoc. Report (1861); XXV. *Crelle*, lix. (1861); XXXIII. [as regards *Quart. Journ.*, vi. 1864]; XXXVII., XXXVIII., *Quart. Journ.*, vii. (1866); XLVIII., XLIX., and some other papers in the *Comptes rendus* (1874–76). I am especially anxious to receive the *first* part of the "Meditationes Analyticæ," of which we have not found a complete set. *All* the papers are in Mrs. Spottiswoode's library, she believes; but I am anxious to preserve these if possible from passing into the printers' hands.

R. TUCKER.

27 Cantlowes Road, N.W.

Statistics of the British Association.

IT is to be feared that the "ladies' curve" in the diagram (NATURE, December 13, p. 153) fails to give anything like accurate information respecting the number of ladies at the meetings of the British Association. Omitting foreigners, the attendance is made up of "members," "associates," and "ladies," but, as a matter of fact, a large number of ladies are members or associates, while only the remainder take "ladies' tickets,"—that is, tickets transferable to ladies, the only transferable tickets issued by the Association.

The column headed "Ladies," in the table (see the Association's Report for 1887, pp. lv.-lvi.) states that 493 ladies attended the last Manchester meeting; but that merely shows that 493 of the ladies who were present took "ladies' tickets," and does not include the lady members and lady associates who were at that meeting. During its meetings the Association publishes lists of the members and associates who are present, exclusive of the holders of "ladies' tickets." By going over the Manchester lists it will be found that upwards of 700 ladies who were either members or associates were in attendance; so that,

including the three groups, more than 1200 ladies attended the Manchester meeting, instead of 493 only. Again, of the ladies at the recent Bath meeting, there were 84 members, *plus* 455 associates, *plus* the, to me, unknown number who took "ladies' tickets."

WM. PENGELLY.

Torquay, December 18.

On the Formulæ of the Chlorides of Aluminium and the Allied Metals.

IN recent numbers of NATURE there have appeared several interesting accounts of determinations of the vapour-densities of certain metallic chlorides. According to the views generally held by chemists, the molecules of some of these chlorides should be represented by the general formula M_2Cl_6, but conclusions are drawn from the experiments described (1) that the formula MCl_3 is applicable to all these chlorides; (2) that the chlorides of the formula M_2Cl_6 are probably incapable of existence in the gaseous state, and that this formula should therefore be given up; (3) that the lower chlorides of these metals should be expressed by the general formula MCl_2 instead of M_2Cl_4, chlorides corresponding to the second formula being incapable of existence.

In view of the great interest that is felt in this question at the present time, it seemed to me that a tabulated statement of the results obtained by various chemists would be of value, in order that a comprehensive view of the question might be obtained:—

VAPOUR-DENSITIES (AIR = 1) OF CHLORIDES, &c.

Aluminium chloride: M.P. 187°; B.P. 183°. Calc. $\left\{ \begin{array}{l} Al_2Cl_6 = 9·2, \\ AlCl_3 = 4·6. \end{array} \right.$

Temperature.	Pressure.	Vapour-Density.	Method.	Observer.	Remarks.
° C.					
350	1 atm.	{9·38, 9·32} 9·35	Dumas	Deville and Troost	
440	,,	{9·34, 9·33, 9·37} 9·35	,,	,,	
		No result	V. Meyer	V. Meyer	Dissociated at 697°.
218	0·59 atm.	8·87	Dumas	Friedel and Crafts	In a later paper these observers state that they heated aluminium chloride to a very high temperature, and allowed the vapour to diffuse through a porous substance, but were unable to detect the presence of free chlorine.
218·3	0·88	9·17			
218·3	0·99	9·69			
218·1	0·39	9·54			
218·1	0·29	9·34			
218·1	0·40	9·93			
263·2	0·98	9·50			
263·7	0·99	9·51			
306·5	0·97	9·46			
306·5	0·95	9·44			
356·9	0·89	9·34			
356·9	0·97	9·17			
357·3	0·96	9·42			
398·2	0·97	9·20			
390	0·79	9·11			
400	0·95	9·27			
415	0·57	8·73			
429	0·97	8·31			
429	0·54	8·71			
429	0·87	8·39			
433	0·90	8·96			
440		7·79	V. Meyer	Nilson and Pettersson	
758		4·80			
835		4·54			
943		4·56			
1117		4·27			Platinum vessel attacked, showing the presence of free chlorine.
1244		4·25			
1260		4·28			

Aluminium bromide $\left\{ \begin{array}{l} AlBr_3 = 9·2. \\ Al_2Br_6 = 18·4. \end{array} \right.$

440	1 atm.	18·62	Dumas	Deville and Troost

Aluminium iodide $\left\{ \begin{array}{l} AlI_3 = 14·0. \\ Al_2I_6 = 28·1. \end{array} \right.$

440	1 atm.	27·00	Dumas	Deville and Troost

Aluminium ethyl: B.P. 194°. $\left\{ \begin{array}{l} Al(C_2H_5)_3 = 3·93. \\ Al_2(C_2H_5)_6 = 7·86. \end{array} \right.$

234	?	4·5	Gay Lussac	Buckton and Odling	
235		8·0 + 8·2 = 8·1	V. Meyer	Louïse and Roux	
258		6·0, 6·3, 6·4 = 6·2			
310		2·5			Complete decomposition
350		2·5			

Louïse and Roux obtained results agreeing with the formula $Al_2(C_2H_5)_6$ by Raoult's freezing-point method, employing ethylene dibromide as solvent.

Temperature.	Pressure.	Vapour-Density.	Method.	Observer.	Remarks.
Aluminium methyl : B.P. 130°. $\left\{\begin{matrix}Al(CH_3)_3 = 2{\cdot}48.\\ Al_2(CH_3)_6 = 4{\cdot}97.\end{matrix}\right.$					
° C.					
130		$\left\{\begin{matrix}4{\cdot}36\\4{\cdot}40\end{matrix}\right\}$ 4·38	Gay Lussac	Buckton and Odling	
163		4·10			
160		4·10			
162		3·90			
240		2·80			
220		2·80			
220		2·81			
Gallium chloride : B.P. 215°–220°. $\left\{\begin{matrix}GaCl_3 = 6{\cdot}1.\\ Ga_2Cl_6 = 12{\cdot}2.\end{matrix}\right.$					
247	1 atm.	13·4	Dumas	Lecoq de Boisbaudran	
273	,,	11·9			
357	,,	10·0			
440	,,	7·8			
307	0·87	10·61	,,	Friedel and Crafts	
357·15	0·64	9·08			
377·6	0·57	7·82			
237·0	0·24	11·73			
357		8·5	V. Meyer	Friedel	
440		6·6			
350		8·85	,,	Nilson and Pettersson	
440		6·12			
606		6·14			
1000–1100		5·18			
Gallium dichloride, $GaCl_2$ = 4·86.					
1000–1100		4·82	V. Meyer	Nilson and Pettersson	
1300–1400		3·57			
Indium trichloride $\left\{\begin{matrix}InCl_3 = 7{\cdot}58.\\ In_2Cl_6 = 15{\cdot}17.\end{matrix}\right.$					
Dull red		7·87	V. Meyer	V. and C. Meyer	
606		8·16	V. Meyer	Nilson and Pettersson	Slow volatilization
850		7·39			Rapid volatilization
1048		6·72			
1100–1200		6·23			
Indium dichloride, $InCl_2$ = 6·36.					
958		7·67	V. Meyer	Nilson and Pettersson	
1167		6·54			
1300–1400		6·43			
Indium monochloride, InCl = 5·14.					
1100–1150		5·30	V. Meyer	Nilson and Pettersson	
1200–1300		5·38			
1300–1400		5·53			
Chromium trichloride $\left\{\begin{matrix}Cr_2Cl_6 = 10{\cdot}96.\\ CrCl_3 = 5{\cdot}48.\end{matrix}\right.$					
1065		6·13	V. Meyer	Nilson and Pettersson	Slow volatilization
1191		5·52			Normal ,,
1277		5·42			
1347		4·83			
1100–1200		5·67			
1250–1350		5·18			
1350–1400		4·58			
Very high		Mol. wt. = 154·9; Calc. for $CrCl_3$ = 159	V. Meyer	A. Scott	
Chromium dichloride $\left\{\begin{matrix}CrCl_2 = 4{\cdot}26.\\ Cr_2Cl_4 = 8{\cdot}51.\end{matrix}\right.$					
1300–1400		7·80	V. Meyer	Nilson and Pettersson	Very difficult to volatilize
1400–1500		7·28			
1500–1600		6·24			

Ferric chloride $\begin{cases} FeCl_3 = 5·60. \\ Fe_2Cl_6 = 11·19. \end{cases}$

Temperature.	Pressure.	Vapour-Density.	Method.	Observer.	Remarks.
°C.					
440	1 atm.	{11·42, 11·37} 11·39	Dumas	Deville and Troost	
440		{10·75, 10·97} 10·86	,,	Friedel and Crafts	In atm. of nitrogen
321·6	0·23	11·41	,,	,,	In atm. of chlorine
325·2	0·19	12·47			
356·9	0·12	12·04			
357·0	0·50	11·85			
442·2	0·27	11·66			
442·2	0·14	11·30			
440		11·14	V. Meyer	V. Meyer	
619		11·01	,,	,,	
Very high		Molecular weight = 136·1 ; $FeCl_3$ = 162·5	,,	A. Scott	
448		10·49	,,	Grünewald and Mayer	Dissociation above 518°, with liberation of chlorine.
518		9·57			
606		8·38			
750		5·40			
1050		5·21			
1300		5·13			
448, 518		Same as above			In atm. of chlorine

Ferrous chloride $\begin{cases} FeCl_2 = 4·39. \\ Fe_2Cl_4 = 8·78. \end{cases}$

Temperature.	Pressure.	Vapour-Density.	Method.	Observer.	Remarks.
Yellow heat		{6·67, 6·38} 6·52	V. Meyer	V. Meyer	
1300–1400		4·34	,,	Nilson and Pettersson	
1400–1500		4·29			

It has been pointed out by Messrs. Friedel and Crafts that the method of Dumas has this advantage over that ot Victor Meyer, that the pressure of the vapour can be accurately ascertained, whereas in Meyer's apparatus diffusion takes place to an unknown extent, so that the actual pressure of the vapour is indeterminable.

It appears to me that the following conclusions, which are in general accordance with the views of Friedel and Crafts, may be drawn from the results so far obtained :—

Aluminium Compounds.—The evidence in favour of the existence of the molecules Al_2R_6 is overwhelming, but it is probable that at high temperatures they undergo dissociation, thus $Al_2R_6 = 2AlR_3$,

Gallium Trichloride.—The results point to the existence of Ga_2Cl_6 at low temperatures, and $GaCl_3$ at high, the more complex molecules undergoing dissociation.

Gallium Dichloride.—Most probably $GaCl_2$.

Indium Chlorides.—Formulæ probably $InCl_3$, $InCl_2$, $InCl$; very little indication of the existence of the more complex molecules, but the results are all by V. Meyer's method.

Chromium Chlorides.—The trichloride $CrCl_3$ undoubtedly exists; little or no evidence in favour of Cr_2Cl_6. The dichloride, even at the highest temperature reached, appears to consist largely of molecules of the formula Cr_2Cl_4.

Iron Chlorides.—All the results by Dumas's method, and those of V. Meyer by his own, point to the stability of the molecule Fe_2Cl_6 up to 500° or 600°. The values obtained by Grünewald and Mayer probably point to dissociation into the simpler molecules $FeCl_3$, but since they observed the liberation of free chlorine above 518°, and as the results above 750° are lower even than that calculated for the simple molecule $FeCl_3$, there is clearly necessity for caution in drawing deductions from the experiments.

The values obtained for ferrous chloride indicate, as far as they go, a gradual dissociation of Fe_2Cl_4 into $FeCl_2$.

SYDNEY YOUNG.

University College, Bristol, November 6.

THE UTILITY OF SPECIFIC CHARACTERS.

THE question of the utility or inutility of specific characters is one which is of considerable importance in the philosophy of biology on account of its connection with the action of natural selection ; and it is one which is of special interest at the present time, because of the attention which has been drawn to it by Dr. Romanes's essay on physiological selection, by the Presidential address to the Biological Section at the recent meeting of the British Association, and by various letters and articles in NATURE and elsewhere. This is a matter upon which a biologist who is practically acquainted with species can alone express an authoritative opinion. It is only the naturalist who has an intimate knowledge of the characteristics and the habits of species who can judge accurately of the relations between such structural features and the animal's habits and surroundings, and who can appreciate the fact that many structures or variations of structure may be of importance, although their precise functions and relations to environment may not yet be known.

The more minutely a group of organisms is studied, the more the object or utilitarian significance of the specific characters becomes evident. In the Tunicata, the class of animals I happen to have paid most attention to of late years, I am convinced of the practical importance or usefulness of the recognized specific modifications :—such as the condition of the muscular system, the arrangement of the vessels in the branchial sac, the number and arrangement of the tentacles, and so on—these structures being all related to most important functions, such as respiration and the regulation of the food-supply. Even in the case of such apparently trivial characters as the shapes and

distribution of the minute spicules throughout the colonies of *Leptoclinum* and some other Compound Ascidians, I know from experience that they affect the hardness and roughness, as well as the colour, of the colony, and so may be of considerable importance in repelling enemies and in keeping the colony free from injurious parasites. As a matter of observation, I find that the colonies of Didemnidæ (which are provided with calcareous spicules) are much freer from both external and internal parasites than are the softer-tested Compound Ascidians.

During the last few years I have had occasion to study closely the fauna of the sea-shore at different parts of our coast. I have spent many hours on the rocks at Puffin Island and elsewhere at extreme low water, watching the animals in the pools and under the ledges in their natural conditions. Such work impresses very forcibly upon the observer the reality and importance of such fundamentals of evolution as variation, over-crowding, and struggle for existence, the action of natural selection, the benefit of protective colouring, the completeness and the advantage of mimicry, the benefit of spicules, shells, various shapes, &c., the purposes and origins of peculiar habits, the complicated relations between the animals and their environment, and finally the utility of specific characteristics.

Of all the regions of sea and land, so far as my limited experience goes, by far the most prolific of animal life is that region of the shore which is known as the upper edge of the Laminarian zone. It lies just beyond the ordinary beach, and is only exposed at the lowest spring tides. There, amongst the tangled masses of *Laminaria*, especially if there are large irregular stones with many pools and crevices between, marine invertebrate life is to be seen in very great profusion ; in a favourable locality, such as Puffin Island, all the chief groups of marine animals being abundantly represented. There, competition is probably very keen amongst allied forms of animals, and the conditions necessary for natural selection to take place, and the results of that process, may most advantageously be studied.

I have lately been noting which animals in this region of the shore appeared to be the most conspicuous by their colour or shape or other peculiarities on various occasions and in different localities, with the object of seeing how far the want of protective colouring or attempt at concealment can be accounted for ; and the result is that, so far as I have observed, all the most brightly-coloured or otherwise very noticeable species are provided with some defensive or offensive contrivance which appears to protect them from enemies. Amongst these conspicuous forms are: the white calcareous and some of the scarlet and other brightly-coloured siliceous Sponges (which are well protected by their numerous pointed spicules), the gleaming white Compound Ascidians (provided with sharp-pointed calcareous spicules), some of the Annelids (protected by their setæ, elytra, &c.), some of the Polyzoa, such as the Escharidæ and other incrusting forms (covered by calcareous ectocysts, often provided with spines and other projections), and a few erect forms such as *Bugula* (which are protected by the presence of numerous sharp-beaked snapping avicularia). The bright orange-coloured *Bugula turbinata* is certainly one of the most conspicuous animals on certain parts of the shore about low-water mark at Puffin Island. Of course such protective characters as these animals possess are not all necessarily specific, but may be generic, or characteristic of still larger groups, and I am not now citing them as instances of useful specific characters, although I do not doubt they would prove such, if the details were properly worked out.

As examples of these last (useful specific characters), I would point to the distinctive features of the species of Ascidians, where even the very varied external shapes may be regarded as useful modifications, since they allow of, or correspond to, particular forms of the muscular mantle and the branchial sac and the other viscera within the test, and of course the shapes of the mantle and branchial sac are of functional importance. It is important to note that the one external feature which it is difficult to see any use in—viz. the number of lobes surrounding the branchial and atrial apertures—is *not* a specific character, but is distinctive of genera, and even more so of families and sub-families.

On passing to the interior of the body in the Ascidians, we find that the best-marked characters are taken from the condition of the mantle and of the branchial sac, and its neighbouring structures the tentacles and the dorsal lamina. Now, these are all organs with most important functions to perform in regard to respiration, nutrition, the circulation of water through the body, and the collection and agglutination of food-particles. And all the structural modifications found are such as must evidently be of actual use to the possessors. The current of water passing through the body of an Ascidian—in at the branchial aperture, through the stigmata in the walls of the branchial sac into the atrial cavity, and from that out by the atrial aperture—is of primary importance, since it serves the following purposes : (1) it conveys oxygen into the body for respiratory purposes, (2) it brings the food-matters into the body, (3) it removes waste matters from the body, and (4) it conveys to the exterior the ova and spermatozoa. This current of water is caused and guided by (*a*) the shape of the mantle and the arrangement of the sphincters and other muscles, and (*b*) the cilia covering certain of the vessels and other parts of the wall of the branchial sac. Hence modifications of the form of the mantle and of its muscles, and of the vessels, bars, papillæ, &c., forming the wall of the branchial sac (which are precisely the characters made use of in distinguishing the species), must surely be of functional importance, or, in other words, are useful modifications, such as would be produced by the action of natural selection.

It is scarcely necessary to call attention to such important adaptive characters as the arrangement of the blood-vessels and water-passages in the walls of the branchial sac, but it may be pointed out that even such trivial structures as the spine-like scales lining the branchial siphon in some Cynthiidæ may well be more or less useful, according to their shape and size, in keeping out small unwelcome intruders, such as the young of the parasitic Copepoda, sometimes found in the branchial sacs of some Ascidians.

Another point in which species of Ascidians differ is the condition of the tentacles round the entrance to the branchial sac—*i.e.* their number, shape, branches, and arrangement. These organs probably perform various functions: they break up and distribute the currents of water, they intercept and guide the food-particles, they probably act as sensory organs, and they form a more or less perfect grid for preventing large objects from entering the branchial sac. Hence there can be little or no doubt that in this case also the various modifications are really useful.[1]

These few instances are, perhaps, sufficient to show that, in the Tunicata at least, specific characters are of actual importance to their possessors, and are adaptive modifications such as would be produced by the action of natural selection ; and I fancy that the same will be found to be the case in other groups of animals, if those biologists who are intimately acquainted not only with the characters of the various species, but also with their habits in a condition of nature, and the environment generally, would turn their attention to the matter.

W. A. HERDMAN.

[1] I am working up the matter in further detail for the Report upon the Tunicata in the second volume of the "Fauna of Liverpool Bay."

THE INTERNATIONAL BUREAU OF WEIGHTS AND MEASURES.

ALTHOUGH the International Bureau of Weights and Measures has now been in existence for some years, its work hardly appears to have been so generally recognized as it ought to be. This may be owing partly to the fact that the results of its labours are published in volumes accessible only to a few; and, partly, that the researches of the Bureau, being in the direction of the highest accuracy in physical measurement, are hardly appreciated at first in the every-day work of ordinary life. It may be well, therefore, to mention briefly the contents of the five annual volumes of the Bureau which have already been issued, before noticing the contents of the sixth volume which lately reached us.[1]

The first volume issued in 1881, under the immediate direction of Dr. O. J. Broch, gives the results of the researches made at the Bureau on the tension of aqueous vapour; on the fixed points of thermometers; on the true weight of a litre of air; and on the specific gravity of water. The second, third, and fourth volumes contain papers on the dilatation of the standard metres; on the weighing of the standard kilogrammes; and on the dilatation of mercury. In the fifth volume may be found an exhaustive consideration of the methods of verifying subdivided linear measures; of calibrating thermometers; and of correcting the progressive errors of micrometer screws. This latter volume also contains thermometric studies; as well as studies of the theory of the balance as applied to scientific weighings.

The new volume (tome vi. pp. 620) contains three papers, one by Dr. René Benoît, on the measurement of dilatations by the method of M. Fizeau; one on the comparison of mercurial thermometers with the air or hydrogen thermometer, by Dr. P. Chappuis; and a third paper on practical formulæ for the transformation of thermometric coefficients, by Dr. A. E. Guillaume.

M. Fizeau's method of determining the rate of expansion of solid bodies by heat is well known (*Annales de Chimie et de Physique*, 1864-66), but its application has hitherto been difficult. Dr. Benoît has succeeded in removing this difficulty, and by clear explanation and complete formulæ of reduction, has brought the use of this dilatometer within the reach of the ordinary student. We regret that our space prevents us giving an outline of Dr. Benoît's methods and a sketch of the particular form of dilatometer he uses; the following, however, are the coefficients of linear expansion for 1° C., from 0° to t°, as ascertained by him during the past year:—

Quartz—Direction parallel to the axis	$\alpha = +0{\cdot}0000071107$; $\beta = +0{\cdot}00000000856$
Direction perpendicular to axis	$\alpha = +0{\cdot}0000131620$; $\beta = +0{\cdot}00000001263$
Beryl—Direction parallel to the axis ...	$\alpha = -0{\cdot}0000013403$; $\beta = +0{\cdot}00000000403$
Direction perpendicular to axis	$\alpha = +0{\cdot}000009942$; $\beta = +0{\cdot}00000000465$
Platinum	$\alpha = +0{\cdot}0000088405$; $\beta = +0{\cdot}0000000189$
Platinum-iridium (Mr. Matthey's alloy, Type I.)	$\alpha = +0{\cdot}000008615$; $\beta = +0{\cdot}00000000221$
Gold coin, English sovereign and half-sovereign	$\alpha = +0{\cdot}000014472$; $\beta = +0{\cdot}00000000421$
Steel cast by Messrs. Jessop and Sons ...	$\alpha = +0{\cdot}000010387$; $\beta = +0{\cdot}00000000595$
Brass and Bronze— Copper, 73·75; Zinc, 24·18; Lead, 0·57; Tin, 1·52	$\alpha = +0{\cdot}000017820$; $\beta = +0{\cdot}00000000584$
Copper, 81·20; Zinc, 8·60; Lead, 0·17; Tin, 9·87	$\alpha = +0{\cdot}000017431$; $\beta = +0{\cdot}00000000618$
Phosphor Bronze (rich in phosphorus, hard)— Copper, 94·6; Tin, 4·7; Phosphor, 0·7	$\alpha = +0{\cdot}000016881$; $\beta = +0{\cdot}00000000618$

Dr. Chappuis states that since the study of the mercurial thermometer has shown that this instrument is susceptible of the greatest precision, there has been recognized a necessity for stating its indications in a scale unique and invariable. The choice of such a scale presents serious difficulties, for even in the best measurer of temperature—as Regnault's air or gas-thermometer—practical measurement is dependent on the limits of pressure between which the measurement is made, and on the nature of the air or gas used. The mechanical theory of heat has given a new definition to temperature, independent of any supposition as to the regularity of the dilatation of bodies, and furnishes therefore an absolute scale of temperature, which, on the supposition that a perfect gas is used, should accord with the definition of temperature of the gas-thermometer.

A rigorous verification and intercomparison have therefore been made by Dr. Chappuis of a gas-thermometer of large capacity, with eight of Tonnelot's hard-glass standard mercurial thermometers. He found that the scale-readings of the mercurial thermometers differ as follows from the scale-readings of air, hydrogen, and carbonic-acid thermometers, the hydrogen-thermometer presenting the greatest divergence:—

0°·107 C. at 40° C. for hydrogen,
0°·097 ,, 40° ,, air,
0°·049 ,, 35° ,, carbonic acid;

the difference in the march of the air and hydrogen thermometers (7), and between the carbonic acid and hydrogen thermometers (8), being represented by the following formulæ:—

$$(7)\quad T_{Az} - T_H = +0{\cdot}00542995(100 - T_m)T_m + 1{\cdot}4118126 \times 10^{-4}(100^2 - T_m^2)T_m - 1{\cdot}322986 \times 10^{-6}(100^3 - T_m^3)T_m.$$

$$(8)\quad T_{CO^2} - T_H = +0{\cdot}03591397(100 - T_m)T_m - 0{\cdot}2340806 \times 10^{-4}(100^2 - T_m^2)T_m - 0{\cdot}510047 \times 10^{-6}(100^3 - T_m^3)T_m.$$

Dr. Benoît has accordingly given the coefficients of expansion of the several bodies above referred to in terms of mercurial thermometers made of crystal glass and of hard glass respectively, and of the hydrogen thermometer; of which the following is an instance:—

Linear Coefficients of Dilatation for 1° *C.*

By mercurial thermometer made of crystal glass.	By mercurial thermometer made of hard glass.	By the hydrogen thermometer.
GOLD (English Standard, 1½)		
$10^{-9}(14473 + 4{\cdot}27t)$ ...	$10^{-9}(14497 + 4{\cdot}03t)$ ...	$10^{-9}(14571 + 3{\cdot}19t)$
PLATINUM.		
$10^{-9}(8840{\cdot}5 + 1{\cdot}89t)$ ...	$10^{-9}(8855 + 1{\cdot}74t)$ ...	$10^{-9}(8901 + 1{\cdot}21t)$

We have recently called attention to the fact that the work of the Comité International des Poids et Mesures is now approaching completion, so far as relates to the delivery to each country of a verified copy of the metric standards of measure and weight, the prototypes of which are to be kept and maintained at the offices of the Bureau, situated at the Pavillon de Breteuil, Sèvres, near Paris; and we are glad to see from the *Procès-verbaux*, also recently issued by the Comité, that the Government of this country have requested the Comité to furnish the Standards Department of the Board of Trade with exact standards of the metre, and of the kilogramme, made in platinum-iridium.

[1] "Travaux et Mémoires du Bureau International des Poids et Mesures," tome vi. (Paris: Gauthier-Villars et Fils, 1888.)

ON THE PLASTICITY OF GLACIER AND OTHER ICE.[1]

THE nature of the motion of glaciers has been the subject of an immense number of observations by Forbes, Agassiz, Schlagintweit, Tyndall, &c., and the following facts amongst others have been established.[2] (1) The velocity decreases gradually and continuously from the centre to the sides, where it is sometimes almost imperceptible, though in other cases it reaches one-third of its value at the centre. (2) The motion is in general continuous from day to day, and even from hour to hour. (3) The motion is generally most rapid at the hottest time of the year, and slowest at the coldest, the ratio being often 4 to 1. But the effect of temperature is at present by no means properly worked out.

One main result of these observations may be summed up in the statement that a glacier moves like a plastic body. The most natural conclusion would be that ice is plastic. But this conclusion was for a long time almost universally rejected. Hand specimens of ice show no sign of plasticity to casual observation, and no doubt few people realized what very slow yielding under stress would account for the observed motion. So the rigidity of ice was treated as an obvious fact. At any rate I have not come across any mention of careful experiments which failed to show plasticity within a few degrees of the melting-point. As will be seen below, however, such results might readily have been obtained on suitable ice.

True plasticity, then, being rejected, some other explanation had to be found. The one generally adopted is due to James Thomson. He proved theoretically that the freezing-point of water is lowered by pressure at the rate of 0°·0075 C. per atmosphere. This was afterwards verified experimentally by Sir Wm. Thomson. The former held further that any kind of stress lowers the freezing-point. Now glaciers are believed to be throughout at or very near the temperature 0° C. Thus the ice should melt at places where the stress is most severe, and an equal quantity should be formed elsewhere. There are at least two difficulties in this explanation. In the first place, the melting must absorb heat, and the work done by pressure in the contraction of volume is quite an insignificant source of heat. So the temperature would be immediately lowered, and the process be brought to a standstill, before it had well commenced, unless heat were supplied by conduction. When we remember that, even when the stress is most severe, the melting-point is only lowered a few hundredths of a degree, and that there must be considerable distances between points of great stress when the ice melts, and points of little stress when it forms, it is difficult to believe that sufficient heat can be conveyed from one to the other to produce much effect. Some rough experiments I have made show ice to be a far worse conductor than any rock, and nearly as bad as wood. In the second place, it has yet to be proved that the mass of the glacier is permeated by water. Recent experiments by Prof. Forel (*Arch. des Sciences Phys.*, Geneva, July 1887) go far to show that the capillary fissures containing water are confined to the surface layer.

But the point which I especially desire to bring out is that this explanation is confessedly only a way out of a dilemma. If glacier ice can be shown in the laboratory to be plastic, the dilemma no longer exists, and there is no necessity to have recourse to any other explanation until it can be proved that the plasticity is insufficient, or otherwise fails to account for the observed facts. The existence of this plasticity in glacier ice we claim to have established in our experiments last winter.

The false plasticity due to melting and regelation is put out of the question by operating at a temperature below even $-0°{\cdot}1$ C., for to lower the melting-point by a tenth of a degree requires a pressure of thirteen atmospheres. If true plasticity is found at lower temperatures, it is impossible to deny its existence at the melting-point itself. And plasticity has been found several degrees below 0° C. by many experimenters, such as Matthews, Bianconi, Aitken, Pfaff, &c.[1] Most of their experiments were made on the bending of bars, in which case the stress is too complicated to furnish any but the vaguest idea of the relation between strain and stress. Further, none of them dealt with glacier ice, for I do not include the experiments of Coutts Trotter, made at 0° C.

Matters were in this state when Dr. Main began his experiments at St. Moritz the winter before last (Roy. Soc. Proc., vol. xlii. p. 329). A winter sojourn in the Engadine affords peculiar facilities for experiments of this nature. During December, January, and February, one can count on almost continuous frost. In a room on the north side of the house, with the window kept permanently open, the temperature seldom rises above the freezing-point. Dr. Main wished not merely to settle the question of the existence of plasticity, but also to determine accurately its amount under various conditions of stress and temperature. He decided to apply tension. This has great advantages over other kinds of stress for purposes of accurate measurement, as it is comparatively easy to isolate from other stresses. Pressure, for instance, applied to the ends of a bar of ice makes it bend, and we have then a complicated set of stresses to deal with. And if the bar be so short and thick that bending is improbable, the contraction to be measured becomes very small. There are, however, certain obvious inconveniences in applying tension, viz. the difficulty of getting a good grip of the ends of the bar of ice, and the constant risk of fracture.

Main used a mould for his ice, which turned out a round bar with a conical enlargement at one end, which would fit into a conical iron collar. A conical piece of ice fitting another collar was frozen to the other end of the bar of ice, and the tension was applied through the two collars. Accurate measurements of the distance between the collars were taken from time to time. In this way he established the existence of plasticity in this kind of ice at all temperatures down to $-6°$ C. It is to be noticed that the ice cones are subjected to both pressure and shearing stress, and some of the observed extension must have been due to the distortion of these cones; but that nearly all of it was due to pure tension in the bar he found by measuring the distance between marks on pieces of paper gummed on to the bar itself. In this last way he found the bar extended during three days at the rate of 0·02 mm. per hour per length of 10 cm., while the temperature remained below $-2°$.

As his health prevented him from spending last winter at St. Moritz, he suggested that I should continue the experiments, kindly putting all his apparatus at my disposal. I should not have been able to carry out such an undertaking had I not been fortunate enough to secure the assistance of an able coadjutor in Mr. Kidd, on whom fell by far the greater part of the labour of experiment. We started, like, I believe, all investigators before us, under the impression that one piece of clear ice would do as well as another, no matter how it had been formed. Thus it was merely owing to the difficulty of obtaining clear ice in the mould that we took our first experimental bar from a different source. We imagined that since Main had established the fact of extension under tension, all that was left was to determine its amount at various temperatures and under various tensions. So we were a good deal surprised by the behaviour of our first bar. It practically refused to stretch. We had taken the pre-

[1] For full details of the experiments herein described see a Paper by James C. McConnel and Dudley A. Kidd, published in the Royal Society's Proceedings, June 1888.

[2] See Heim's "Gletscherkunde," published by Engelhorn, Stuttgart, 1885.

[1] See NATURE, vol. xxxii. p. 16. and Heim, *loc. cit.* p. 315, who cites a paper by Matthews, *Phil. Mag.*, 1869.

caution of observing the extension of the bar proper by measuring the distance between two needles fixed in the bar near either end. We used a cathetometer in the first instance, but that generally unsatisfactory instrument was particularly untrustworthy in our circumstances, and the small extension we found may have been due to errors of reading. We applied, therefore, a system of light levers to the needles, which would indicate a very minute extension, though it was not well adapted to measure large extensions with accuracy. Under this far more severe test, the bar still maintained its rigid character. Between two of the readings there was a slight extension of 0·044 mm. This we attributed to a sort of surface crack which we found in the bar after the experiment. With this trifling exception, the whole of the lengthening seemed to be caused by a gradual rise of temperature which took place. This supposition gave, indeed, a coefficient quite concordant with the latest results obtained by others. Even without making any allowance for the rise of temperature, the mean rate of extension during six days was less than 0·0002 mm. per hour per length of 10 cm., about 100 times as small as Main had found. This enormous difference had nothing to do with either the temperature or the tension, for the former averaged about the same and the latter was slightly greater in our experiment. The cause evidently was to be sought in the nature of the ice itself, and we were not long in discovering a satisfactory explanation.

Ice is, as is well known, a crystalline body, and its molecular structure is no doubt perfectly regular and definite so far as it is revealed by the polariscope or spectrometer. We have no reason to expect any bending of the optic axis or gradual change of the indices of refraction within any one crystal. Every piece of ice, therefore, is either itself a single uniform crystal, or is built up of pieces, each of which is a single uniform crystal. Thus, bars of ice fall into two classes—homogeneous and heterogeneous. Main's bars were heterogeneous, ours was homogeneous. We concluded, therefore, that *heterogeneous ice is plastic, while homogeneous ice is rigid;* and this conclusion was confirmed by subsequent experiment.

It is generally impossible to tell with the naked eye whether a piece of ice is heterogeneous or homogeneous. But a polariscope settles the question at once. We put together a rude form of polariscope in which the light from a sheet of white paper is reflected at an angle of 57° by a pile of three glass plates towards a Nicol prism held in the same framework. We generally turned the Nicol so as to make the field dark. Looking through the Nicol, and holding a bar of heterogeneous ice between the Nicol and the glass plates, some of the crystals would look dark, some light, and some, perhaps, coloured. If the crystals overlapped and interlaced much, the appearance was very complicated; but in any case it was easy to make out the line where the interface of any two crystals cut the surface of the bar. Our first bar was square, with the optic axis at right angles to two of the sides. It was about an inch thick, and it showed under the polariscope the coloured rings and black cross of a uniaxial crystal very well. And these remained stationary and unbroken while the bar was moved parallel to itself across the field of view, showing that it was a single crystal. To obtain the ice we had put out a large bath of water in, as it happened, comparatively mild weather, and cut the bar from the ice formed at the top. The water was from the ordinary hotel supply, the same as had been used by Main.

Glacier ice, as is well known, is markedly heterogeneous, being composed of irregular lumps accurately fitting each other, each of which is a single crystal. These lumps are called in German *Gletscherkörner*, and in French *grains du glacier;* so in English we may use the term glacier grains. They are found of all sizes, from that of a pea to that of a melon. But the average size diminishes rapidly as we follow a glacier upwards towards its source. At the surface of a glacier the ice is of course quite disintegrated by the sun, and the original structure has vanished, and on the side of a crevasse or in an ice cave where the clear ice is seen, the grains are frequently quite indistinguishable with the naked eye. But, if a fragment of this clear ice be exposed to the sun for a few minutes, the dividing surfaces of the grains come out very clearly through thin films of water being formed. Moreover, in each crystal a number of small disks appear, perhaps the tenth of an inch in diameter, with their planes at right angles to the optic axes. This peculiarity helps to mark off one grain from another.

On account of this structure it was probable that glacier ice would prove to be plastic; but it would have been extremely rash to repeat the mistake into which others had fallen, and deduce the properties of glacier ice from experiments on other ice. Fortunately, it was an easy matter to obtain access to a glacier. For the restaurant at the foot of the Morteratsch Glacier and the road thereto are now kept open in winter, and the distance from St. Moritz is only eight or nine miles. We procured some pieces from the natural ice caves, whence the stream issues at the foot of the glacier, and sawed them into bars at our leisure. We tested three bars, which put beyond a doubt the plasticity of glacier ice under tension. The rate of extension varied, however, in the most extraordinary manner in each bar, not merely with the temperature and the tension, but also with changes in the nature of the bar, due, apparently, to the process of extension itself. To make the results obtained with different bars comparable, I shall give all the rates of extension in millimetres per hour per length of 10 centimetres. The first bar extended at a rate of from 0·013 mm. to 0·022 mm., the variations being attributable to changes of temperature. The second began at a rate of 0·016 mm., and gradually slowed down till it reached at the same temperature a rate of 0·0029 mm., at which point it remained tolerably constant, except for slight temperature fluctuations, until the tension was increased by one-half. This brought the rate at once up to 0·0110 mm. This increased rate in its turn showed a tendency to sink, more or less counterbalanced by a rising temperature. This piece of ice was under tension for twenty-five days, and extended altogether about 3 per cent. of its length. The third piece behaved in a very different manner. It began at the rate of 0·012 mm., increased its speed, with the tension nearly doubled, to 0·026 mm., and stretched faster and faster, with unaltered tension, till it reached the extraordinary speed of 1·88 mm. We put on a check by reducing the tension by one-third, whereupon the speed fell at once to 0·35 mm., and gradually declined to 0·043 mm. The lowest temperature reached during our experiments, except with the intractable bath ice, was with this specimen. For twelve hours the temperature never rose above $-9°$, and it probably averaged $-10°·5$. The tension happened to be very light—only 1·45 kilos per sq. cm.; but the rate was easily measurable. It was 0·0065 mm. The arrangement of the grains in these bars was too complex for description. The size averaged, perhaps, that of a walnut. Nearly one-third part of the third piece was one crystal, which ran three-quarters of the length between the needles.

Some, though not all, of the ice of the St. Moritz Lake is possessed of a curious structure. It is built up of vertical columns whose sections are of quite irregular shapes. The thickness of each column is not quite uniform; still, the sides are nearly vertical. An average column is about as thick as an ordinary pencil, and in length is only bounded by the depth of the clear ice—*i.e.* a foot or more. Each column is a single crystal, and the optic axes are generally nearly horizontal, though otherwise arranged at random. The columns become visible

to the naked eye when the ice begins to melt, and, if this melting is caused by sunshine, they often become quite detached and fall apart. The appearance presented on the lake when the ice melts in the spring is described as very curious. The crackling of the breaking columns, when the loose ice drifts against the shore, can be heard at some distance. It would be interesting to learn if such columns have been noticed in England. (Prof. Heim informs me that he has found a columnar structure in lake ice in the Swiss lowlands, but the optic axes were all vertical.) A few experiments we made on freezing water in a bath led us to attribute this structure to the first layer of ice having been formed rapidly—for example, in air below $-6°$ C. No doubt the nature of the first crystals formed settles the structure of all the rest of the ice.

This lake ice afforded a capital opportunity for testing our notion that the crystals themselves are rigid, and that the apparent plasticity is due to some action at the interfaces of the different crystals. We first tried a bar whose length was parallel to the columns. This was, really, trying to stretch a bundle of long thin crystals. We were able to measure an extension, but it was excessively small, amounting to about 0·12 mm. on one side of the bar and 0·07 mm. on the other during 208 hours, giving a mean rate per hour per length of 10 cm. of 0·00046 mm. I do not believe that the crystals stretched by even this small amount. For those that were slightly inclined to the direction of pull would be pressed against their neighbours, there would be yielding at the interfaces, and consequent minute lengthening of the bar. We next cut a bar such that the columns ran in a slanting direction across it at an angle of about 45° to the length. The difference was very striking. The new bar stretched at a rate of 0·015 mm. per hour per length of 10 cm.,—more than thirty times as fast.

Towards the end of the winter we determined to try the effect of pressure, and after some thought decided on the following arrangement, which proved in practice very satisfactory. We found in Dr. Main's stock two sheets of thick plate glass, about 25 centimetres by 17. We laid one of these on the table, on it three pieces of ice, and on them the other glass plate. The three pieces were cut as nearly alike as possible, each being about an inch cube. So they were short and thick enough to preclude the likelihood of bending. They were arranged at the angles of an equilateral triangle 9 cm. in the side. Pressure was applied by means of a lever and weight at a point vertically over the centre of this triangle, so the pressure on each block of ice was the same. Measurements of the distance between the plates were taken with calipers at three points at the edge, so selected that it was easy to calculate from the measurements the contraction of each block. Our first experimental result was that the coefficient of friction of ice on glass is very small. The moment the weight was applied, the three pieces of ice shot out on to the floor. Afterwards this inconvenient tendency was held in check by freezing pieces of paper on to the ends of the blocks.

Three pieces of glacier ice showed that this substance is just as amenable to pressure as to tension. The mean rates during five days were respectively 0·035 mm., 0·056 mm., and 0·007 mm. per hour per length of 10 cm. We could not discover any material difference between the three under the polariscope. They were all composed of smallish grains averaging perhaps 7 mm. in diameter, and all three were from the same lump. They were under exactly the same conditions of temperature, and under, at any rate nearly, the same pressure, and yet the second piece gave eight times as much as the third. Of course the arrangement of the interfaces was very complicated in both pieces, and it may have been much less favourable to distortion in the third, but it seems more probable that there was some obscure difference in the state of the interfaces. Bubbles, at any rate, seem to have had no bearing on the matter, for the third piece contained far the most, and the first piece the fewest.

We next tried lake ice with the columns vertical. The mean rate of the three pieces during four days was 0·001 mm. per hour per length of 10 cm. This was only just perceptible to the calipers, and we think it may have been entirely due to the yielding of the films by which the paper was attached or to the same cause as in the case of tension.

Our evidence for the rigidity of an ice crystal rests on three experiments. One of these was on a single crystal of the bath ice, and tension was applied; and the other two on lake ice with the stress applied parallel to the columns: tension in the first case, pressure in the second. These showed that the plasticity of an ice crystal is either non-existent, or is at any rate of a very different order of magnitude from that of ordinary heterogeneous ice. The optic axis in the first case was exactly at right angles to the stress, and in the two latter it was not very far from that position. It would have been perhaps more satisfactory if we had applied stress in other directions. But it seems, *a priori*, very unlikely that any homogeneous substance should be rigid in one direction and plastic in another, and in our Royal Society paper we have given more conclusive reasoning to show that the rigidity must extend to the direction parallel to the axis.

If a bar composed of a number of crystals of irregular shape stretches, while remaining compact, the crystals must necessarily change their shape. It is probable, therefore, that molecules separate themselves from one crystal, and moving across the interface attach themselves to another. But to unravel the laws which govern the direction and rate of the motion of the molecules further experiment is necessary. Mr. Buchanan's experiments, recently described in NATURE (vol. xxxv. p. 608, xxxvi. p. 9), throw some light on the matter. They render it likely that a large part of the soluble impurities in the ice will be collected at the interfaces, and will keep a certain amount of water in the liquid state. This liquid, however, must be a very thin film, for it does not interrupt the optical continuity. If the thickness of the film were not small compared with a mean wave-length of light, there would be reflection, and the interface would be visible to the naked eye. Nevertheless an invisibly thin film might play a very important part in providing a mobile medium for the transmission of the molecules. According to Mr. Buchanan, the amount of liquid present would be roughly inversely proportional to the number of degrees below 0° C. This law is very accurate near 0° C. With any one salt the amount of liquid at low temperatures would be rather greater than is given by the law, but at a certain temperature, the freezing-point of the cryohydrate of that salt, the liquid would completely solidify. According to Guthrie, the cryohydrate of $CaCl_2$ freezes at $-37°$ C., of NaCl at $-22°$, of Na_2SO_4 at as high a point as $-0°·7$. If this thin film of liquid be an essentia factor, ice should be perfectly rigid at a temperature low enough to freeze all the cryohydrates. On the other hand, the amount of liquid should become indefinitely great as zero is approached, so that the plasticity might be expected to be very largely increased when the air surrounding the ice rises above zero. We did not find this was the case. In a tension experiment on an icicle, the surrounding air for five hours was at about $+0°·5$ C., and yet the rate of extension was not strikingly greater than it had been a few degrees lower.

The temperature variations proper were so small compared with the irregular variations spoken of above, that it was difficult to secure any satisfactory measure of them. Still, I have a few figures to offer. In the case of the second piece of glacier ice, while at $-3°·5$ the rate was 0·0029 mm., two days before and two days afterwards it was about 0·0020 at $-5°$, and a few days earlier 0·0013

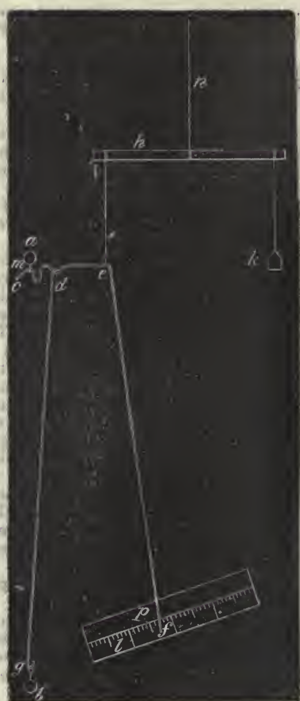
at $-8°$. In the icicle, when the temperature variations seemed paramount, the rate at $-2°$ was 0·0028; and at $-0°·2$, 0·0034. Under pressure the influence of temperature seems much more powerful. In all three pieces of glacier ice the rate rose at $-3°$ to about ten times its value at $-5°$.

The effect of a change of tension was very striking. I append a list of all the cases which occurred.

Specimen.	Change of tension: kilos per sq. cm.	Change of rate: mm. per hour per 10 cm.
Glacier ice C	2·55 to 3·85	0·0018 to 0·0110
Glacier ice D	1·45 ,, 2·55	0·0075 ,, 0·026
,, ...	2·55 ,, 1·03	0·105(?) ,, 0·010
,, ...	1·03 ,, 2·50	0·010 ,, 0·228
,, ...	2·50 ,, 1·80	1·88 ,, 0·35

The 0·105 is uncertain owing to an accident. It was certainly not less, and may have been a good deal greater.

I think it will be interesting to describe the system of levers which we found so effective in measuring small extensions. It is shown in the figure. *a* and *b* are sections of the projecting ends of glass needles fixed in the ice from 12 cm. to 20 cm. apart; *cdef* is a bent iron wire, "the indicator," hooked to a wire loop, *m*, securely fastened to *a*; *h* is a wooden lever suspended by a thread *n*, which, owing to the counterpoise *k*, pulls the indicator upwards with a thread fastened to a wire loop at *e*. The indicator is kept from rising by the connecting fibre, a piece of stiff wire hooked at one end to the loop *g*, fastened to *b*, and at the other to a bend d^1 in the indicator. The lower end of the indicator gives the reading on a paper millimetre scale *l*, gummed on to the mirror *p*. The mirror, of course, enables the observer to avoid errors of parallax. The stand of the mirror is glued to the lower collar. To appreciate the action of the levers, regard *a* for the moment as fixed, then lowering *b* through a small distance *r* will move *f* through a distance $s = vr$ at right angles to *mf*, where *v* is the ratio of the distance *mf* to the perpendicular let fall from *m* on the line *gd* produced if necessary. If *md* be the perpendicular to *gd*, when *f* is in the middle of the scale, the multiplier *v* remains practically constant. This precaution was not always taken, but allowance is made for the resulting error. Two lever systems were required, one for the outer ends, and the other for the inner ends of the needles passing through the ice.



In the following table is given a summary of our results:—

SUMMARY. *Extension Experiments.*

Description of specimen.	Duration.	Rate per hour in mm. per length of 10 cm.	Tension, kilos per sq. cm.	Maximum temperature.	Mean temperature.
Bath ice uncorrected for temperature	5½ days	0·00028	4·9	$-1°·0$	$-4°·5$
,, corrected for temperature	,,	0·00000	,,	,,	,,
Mould ice	28 hours	0·048	3·8	0·0	$-5·0$?
Glacier ice A, maximum rate	5 ,,	0·022	1·66	0·0	$-2·0$
,, minimum rate	4 ,,	0·013	,,	$-1·0$	$-2·5$
Glacier ice B, maximum rate	24 ,,	0·016	2·7	$-2·5$	$-3·5$
Glacier ice C, ,, ...	23 ,,	0·0068	2·55	$-2·5$	$-4·5$
,, minimum rate	3 days	0·0013	,,	$-6·0$	$-9·0$
Glacier ice D, maximum rate	10 mins.	1·88	2·50	$-2·1$	$-2·1$
,, minimum rate	16 hours	0·0054	1·45	$-6·0$	$-10·0$
,, lowest temperature...............	12 ,,	0·0065	,,	$-9·0$	$-10·5$
Icicle, maximum rate	5 ,,	0·0041	2·2	0·0	0·0
,, minimum rate	8 ,,	0·0015	,,	$-0·7$	$-1·7$
Lake ice, parallel columns	7 days	0·00039	2·1	0·0	$-5·5$
,, greater tension	2 ,,	0·00076	2·8	$-4·0$	$-5·5$
Lake ice, oblique to columns { maximum rate ..	6 hours	0·034	2·75	$-5·6$	$-5·8$
Lake ice, oblique to columns { minimum rate ..	16 ,,	0·010	,,	,,	$-6·0$

Compression Experiments.

			Pressure, kilos per sq. cm.		
Glacier ice E	5 days	0·035	3·2	$-2·8$	$-6·0$
Glacier ice F	,,	0·056	,,	,,	,,
Glacier ice G	,,	0·007	,,	,,	,,
Lake ice, parallel to columns { A	3 days	0·0002	3·7	$-3·9$	$-6·0$
Lake ice, parallel to columns { B	,,	0·0012	,,	,,	,,
Lake ice, parallel to columns { C	,,	0·0018	,,	,,	,,

[1] This was a deeper bend than is shown in the figure.

Glacier ice C is the same piece as B, cut rather shorter. The rates of extension given here are of course the mean of the rates observed on the two sides of the bar, which were generally far from being equal. Sometimes the greater speed would fluctuate from one side to the other; in other words, the bar would bend first one way then the other. In other cases one side would always extend faster, *e.g.* in glacier ice D the total extension of one face was 2·9 mm., of the other 9·7 mm. The breaking tension we found in the bath ice to be about 8 kilos per sq. cm., but for obvious reasons we did not care to approach this limit too closely. One curious fact deserves notice. The icicle, which was built up of very small crystals, stretched very slowly; while, on the other hand, the most plastic of our pieces of glacier ice contained one very large crystal. This may have been accidental, or it may have been due to the impurities. The fewer the interfaces the greater the quantity of soluble salts at each.

Let us compare the figures in the table with the plasticity actually observed in glaciers. Heim has collected a number of observations on the increase of velocity from the sides to the centre of a glacier. The most rapid increase he mentions among the Alps is on the Rhone glacier on a line 2300 metres above the top of the ice-fall. At 100 metres from the western bank the mean yearly motion from 1874 to 1880 was 12·9 metres, at 160 metres from the bank it was 43·25 metres This gives an increase of velocity in each metre across the glacier of 0·000058 metre per hour. The stretching involved in this distortion is shown in the paper to be greatest in a direction inclined at 45° to the direction of motion, and then to amount to 0·0029 mm. per hour per length of 10 cm. Hence the plasticity we have found in hand specimens is amply sufficient to account for the distortion of a glacier, even without the aid of crevasses.

It may be said that the term plasticity can not be properly applied to the property of ice that I have described, but there is no other convenient word. Further, it is quite possible that sealing-wax and pitch may be built up of microscopic or ultra-microscopic crystals, and that their plasticity is fundamentally similar to that of ice, the difference being merely one of scale. Helmholtz has suggested somewhere that ice, with its definite and easily ascertainable structure, may furnish the clue to the solution of many difficult problems in the properties of matter.

JAMES C. MCCONNEL.

NOTES.

AT the annual meeting of the Paris Academy of Sciences on December 24, the Bordin Prize, awarded for perfecting the theory of the movement of a solid body, was awarded to Madame Sophia Kovalevsky, a professor at Stockholm University, and a lineal descendant of Matthias Corvinus, King of Hungary from 1458 to 1490. In astronomy, the Valz Prize was awarded to Mr. E. C. Pickering, and the Janssen Prize to Dr. William Huggins. The Montyon physiology prize was divided between Mr. Augustus D. Waller and M. Léon Frédéric.

DR. SCHWEINFURTH has removed his residence from Cairo to Berlin. The German Government has placed at his disposal a house for the accommodation of his African collections, which after his life-time will become the property of the State, but in the meantime remain in his charge, the Government meeting all the expenses of their maintenance.

AT present Dr. Schweinfurth is on his way to Arabia Felix for the purpose of making botanical collections in the mountains of Yemen. Judging from what is known of the limited but extremely peculiar flora of Aden, and from the specimens which Major Hunter, the assistant Resident at Aden, has transmitted to Kew from the interior, the results of Dr. Schweinfurth's explorations are likely to be of the very greatest interest.

A NEW part of the "Scientific Results of Prjevalsky's Expeditions" has just been published by Prof. Hertzenstein. It contains a description of the fishes, and is illustrated by eight plates.

DR. FRANÇOIS, of the Science Faculty of Rennes, has been despatched, by the French Minister of Public Instruction, to Tahiti, to investigate thoroughly corals and coral formations there.

IT is intended that the next general meeting of the Association for the Improvement of Geometrical Teaching shall be held at University College, Gower Street, on January 19, 1889. The morning sitting, at which the Reports of the Council and the Committees will be read, and new officers and members elected, will begin at 11 a.m. After an adjournment for luncheon at 1 p.m., members will reassemble at 2 p.m., when an address will be delivered by Prof. Minchin, of Cooper's Hill, on "The Vices of our Scientific Education."

LAST Friday, Mr. Mundella asked the Chancellor of the Exchequer whether he was able to remove the uncertainty and embarrassment of the provincial Colleges by publishing his scheme for grants in aid; and whether, in consideration of the delay which had already taken place, and the pecuniary position of several Colleges, he would provide that the grants should take effect from January 1 next. In reply, Mr. Goschen said he was not able to make any statement as to the particulars of a scheme for grants in aid to University Colleges in the provinces. In any case it would not be possible for the grants to take effect from January 1 next, as they would be included in the Estimates for the financial year 1889–90, nor could the grants be of such amounts as to retrieve the position of any College in serious financial embarrassment. Government grants, though they would be a valuable addition, could in no case be, and were not intended to be, an effective substitute for local contributions, which must always bear the greater share of the burden. With respect to the scheme in general, Mr. Goschen was anxious to state that any delay which had arisen was due entirely to the number and importance of the subjects competing for the attention of the Government during the session. The Government regarded grants to local Colleges as a step of great importance, and possibly of far-reaching effects. It was absolutely impossible to propose a scheme without the most careful consideration of its bearings, more especially the proportions and the conditions on which any assistance from Imperial funds should be given to local institutions for higher-class education. It was not from any neglect of the matter, but rather from their sense of its extreme importance, that the Government had not been able to formulate their proposal, although they hoped to do so at a very early date.

AT a recent meeting of the Senate of the Sydney University it was announced that the Hon. William Macleay, besides presenting to the University his valuable museum of natural history, which comprises specimens from all the Australian colonies, New Guinea, and the various groups of islands in that quarter of the globe, has also given the sum of £6000 to endow a curatorship for that mu eum.

WE learn from *Science* that Mr. J. W. Osborne, of Washington, the well-known inventor of photo-lithography, has presented to the United States National Museum and to the Art Museum in Boston his large and valuable collection of proofs and specimens illustrative of the development of photo-mechanical printing. All the important and typical processes are fully represented in each by specimens collected by Mr. Osborne in the art centres of Europe and America, and include the works of all who have

in any measure achieved success in the graphic arts. As soon as it can be properly classified, the collection intended for the National Museum will be exhibited in the section of graphic arts. Mr. Osborne's contribution, the Museum authorities assert, has laid a substantial foundation for an exhaustive collection of kindred productions under Government auspices at Washington.

M. BIALOVESKI, of Oostnamenogorsk, Western Siberia, writes to us to suggest that an international journal of geology is greatly needed. Geology, as he points out, is making continual progress, and the number of investigators steadily increases. It is difficult for students to keep up with the advance of the science, since many important communications are contributed to periodicals which are not generally accessible. An international journal, our correspondent thinks, would supply exactly what is wanted. He suggests that it should be edited in some great centre, such as London or Paris, and that the language adopted should be either Latin or English. The question might perhaps be discussed with advantage at the next meeting of the International Geological Congress.

THE geological history of the Caspian depression is the subject of a remarkable article, by N. Andrusoff, in the November number of the *Izvestia* of the Russian Geographical Society. All that is known about the geological structure of the Caspian depression and the surrounding highlands has been turned to account by the author, and he carefully distinguishes between fact and hypothesis. He gives, first, a condensed but well-conceived description of the Caspian Sea; then he analyzes the geological structure of the Great Balkhans in the Transcaspian region, the two parts of the Caucasus—Western and Eastern—and the mountains of the Crimea. He comes to the conclusion that the upheaval of the Crimea-Caucasus-Balkhan system began after the Jurassic period in the Crimea, and was continued through the Chalk period. A considerable raising took place during the earlier parts of the Tertiary period, and the Miocene epoch was characterized by subsidences, especially in the south. Great changes in the relative altitudes of the region followed after the Sarmathian period. Then he sharply separates the two parts of the Caspian Sea—the shallow northern part, and the deep southern part—which originated in different ways, and at different epochs. The history of the basin during the Tertiary period is treated in detail. The want of data for reconstituting it in full is indicated with great precision, but a map is given to show approximately the extensions of the sea during the periods after the middle Pliocene period. Finally, the fauna of the Caspian (a full list of which, including 187 species, is given), and its bearing upon the question, as well as its probable origin, are discussed. The paper is so important that we hope it may soon be translated into English.

THE same number of the *Izvestia* contains a paper on the earthquake of May 27, 1887, at Vyernyi (already described in NATURE); a note on the geodetical connection of Spain with Algeria, by General Stebnitzky (with a map); and a note, by A. Zolotareff, on the surface and population of Persia. Measured on Petermann's map, the surface of Persia appears to be 29,986 square geographical miles, while the probable population is taken at 6,000,000. Two instructions, one for observations on shifting sands, and another for meteorological observations by travellers, are issued in the same number by the Council of the Society.

IN his Annual Report, lately issued, the President of the Johns Hopkins University, Baltimore, presents a very interesting account of the work done at that admirable institution during the past year. The instruction given has never, Dr. Gilman thinks, been more quickening and successful, nor has the progress of literary and scientific undertakings, in charge of the principal teachers, ever been more satisfactory. During the year there has been a noteworthy advance in the facilities for the study of astronomy, theoretical and practical. There has also been a considerable increase in the number of students attending astronomical lectures. In the department of physics the new physical laboratory justifies the expectations which led to its construction; it not only affords increased facilities for instruction, but enables investigations to be carried on with greater efficiency. The only cause for anxiety with regard to this University is the loss of income from the stocks which were given to it by its founder. Strenuous efforts are about to be made to provide new sources of revenue, and there ought to be no doubt as to their success.

THE peninsula of Florida contains innumerable isolated ponds varying from a few square rods to many square miles in area. Many of these are simple hollows filled with rain-water, without any connection with other waters. Some of them are on high ground, where no flood can establish temporary connection with other waters, through which fish might be admitted. The smaller ones often dry up entirely in seasons of drought, yet when filled with water they do not seem to be behind their neighbours in population. They all swarm with fish. For instance, at Orange Heights, in Eastern Alachua County, which is one of the most elevated regions of the State, as is plainly shown by the radiating streams which rise in that vicinity, there is a small pond on the top of the highest elevation in all that region. Mr. Charles B. Palmer, who records these facts in *Science*, says he has twice known this pond to be dry, yet it now contains an abundance of small fish. "How have they been preserved from destruction," asks Mr. Palmer, "and whence came the original stock?"

THE *Times* says that the collection of *Salmonidæ* ova has been made on a large scale this season at the Midland Counties Fish Culture Establishment, Malvern Wells, and Mr. William Burgess, its founder and proprietor, has laid down for incubation large quantities of eggs. Arrangements have been made for rearing such an extraordinary number of fish that the hatcheries are being taxed to their uttermost. Eggs will be received and hatched out, free of charge, for public bodies. The acclimatization of the American whitefish, *Coregonus albus*, is to be attempted by Mr. Burgess, with the co-operation of the United States Fish Commissioners, who have expressed their willingness to forward consignments of the ova of this valuable food-fish, which is held in high favour in America. In order to carry their naturalization to a successful issue, special habitats have been provided of great size and depth, while all that is necessary to their existence has been furnished. The operations of last season have resulted in an extensive distribution of fish in various lakes and other waters in this country. Coarse fish, such as perch, tench, carp, and roach, have been propagated artificially by Mr. Burgess with a success that has induced him to increase his labours in this direction by enlarging his establishment.

THE Minister for Agriculture in Victoria announces that the Government will probably select some land on which to build an institution where attention will be paid to vine-growing solely, under the control of the Central Board of Viticulture.

THE northern limits of the culture of the silkworm are being steadily extended. Experiments made last summer at Astrakhan showed that it could easily be carried on at the mouth of the Volga. Notwithstanding the age of the mulberry-trees, which were planted at Astrakhan thirty-five years since, the results of the experiments proved satisfactory, and 20,000 cocoons were received this year.

WE have on several occasions drawn attention to the progress of the meteorological service in Queensland since Mr. C. L.

Wragge undertook its reorganization about two years ago. We learn, however, from the Adelaide *Evening Journal*, that at the recent Meteorological Conference at Melbourne serious objections were made to the issue of intercolonial weather forecasts from the central office at Brisbane, and that a resolution was passed to the effect that no forecast should be telegraphed from one colony to another. It is evident that if this proposal were carried out, it would practically check the advance in weather prediction which it has been Mr. Wragge's aim to foster, and the suggested alteration was, naturally, strenuously opposed by Mr. Wragge. The success of the service in Queensland is greatly due to the co-operation of the Post and Telegraph Departments, observations being taken at every station in the colony. Mr. Wragge proposes to establish other stations in the far north and west districts, and to examine the climatological factors of the stations generally, with the view of the cultivation of wheat in Central Queensland as profitably as in South Australia.

We have received from Prof. C. Wagner, Director of the Observatory of Kremsmünster, a discussion of the rainfall and thunderstorms at that place; the paper contains some interesting results, especially with regard to thunderstorms. The Observatory is situated in lat. 48° 4′ N., and long. 14° 8′ E. (in Austria), and possesses a very long series of observations, dating back to 1763. The older series, from 1763–1851, have been discussed in vol. i. of the Vienna *Jahrbuch*, 1854. The first rain-gauge was erected in April 1820, so that Prof. Wagner is able to publish the observations for each month of the years 1821–87, one of the longest periods existing, yet he finds that the period is too short to determine with accuracy the range of the rainfall for single months. The average yearly amount is 38·5 inches. The greatest falls occur in July, the least in February, and there is a second maximum in November. It would appear that the rainfall has increased latterly: dividing the series into two periods, it is found that the average number of rainy days from 1821 to 1850 is 123·4, and from 1851 to 1887, 142·8 days. The thunderstorm observations are given for each month since 1802. The average number of storms yearly is 35; they occur mostly in June and July. During the whole eighty-six years only one storm occurred in December. The author gives the daily and yearly range according to the direction of the storms, and also the daily range without reference to direction. These tables show a regular period of frequency. The maximum occurs from 4h.–5h. p.m., the number then decreases until 7h. p.m., and from 7h.–8h. there is a second maximum. A third maximum also occurs from 1h.–2h. a.m. He also investigates the possible influence of the moon on the frequency of the storms, and finds (as has before been observed at Prague) that a maximum occurs at the times of the full moon and last quarter. The same fact shows itself when the series is divided into two sets.

A shock of earthquake was felt at Tashkent, on November 28, at 11.40 a.m.

The Caucasian papers give the following details as to the earthquakes which were felt at Kars and the neighbouring region in September last. The first shock was felt at Kars on September 23, at 3.25 a.m.; it reached its maximum intensity on the high left bank of the Kars River, where several crevices appeared in the barracks of Mukhlis, while on the right bank of the river, which is flat and low, it was felt with much less intensity. The direction of the earthquake was from south-west to north-east. The second and third shocks were felt at 6 and at 9.30 a.m. respectively. They were feeble, and were followed, at 3.20 p.m., by a much stronger shock, which lasted for about five seconds, and had the same direction. A new shock followed at 8.25 p.m.; it had the same direction, but the undulatory movement of the soil was also accompanied by vertical shocks. The direction of the undulatory movement of the soil seems to have taken a more northern direction in the next shock, which was felt at 11.20 p.m. Several shocks followed during the night and the next morning, and a very strong shock, lasting for about ten seconds, was felt on the next day at 2.35 p.m.; its direction was, first, towards the north-north-east. A vertical shock soon followed. Crevices appeared in most buildings, and several houses in the Armenian village Tchighirgan were destroyed. Strong shocks followed at 10.5 and at midnight. On September 13, the earthquake was continued by several slight shocks and a strong one at 9 a.m.; next day, there was a slight shock at 11.25 a.m. This was almost vertical; but an improvised seismometer shows a deflection of the point towards the north. In the Ghel division of the Ardahan district, the same shocks were felt, and had more serious consequences. In the villages of Altun-bulack, Hoshtulbent, Plor-mori, Mehkerek, Shaki, Tondash, Kalpikor, and Kundun-su, most houses were destroyed. Five persons were killed.

At the last meeting, November 16, of the Russian Geographical Society, D. N. Ostrovski made an interesting communication upon the Lapps. Their numbers are estimated at 28,000, of whom 25,000 are living in the territories of Sweden and Norway, and the remainder in Russia and Finland. Almost all the Lapps who live in Sweden are nomads; those who stay in Norway are half nomadic. Those of Finland are all settled, and some of them even have no reindeer; a school was opened for them last year. Sea fisheries are their chief occupation. At the end of the summer they fish in the lakes in the interior of the Koha peninsula, and in the winter they stay in their small houses (*tups*) in the neighbourhood of the marshes covered with moss, where their reindeer obtain food. Their settlements spread in Norway only as far as the 62nd degree of latitude, but in Finland they go as far north as Lake Enare. Their former dwelling-places were farther south, but they were compelled to migrate northwards by the Finns, who steadily extend their settlements in this direction, clear the forests, and take the best grazing-grounds. The folk-lore of the Lapps is full of traditions about their struggles with the Finns. The opinion formerly entertained as to their dying out is not quite exact, although it is true that the Lapps are being steadily absorbed by the Finns. A rich collection of photographs, and samples of various domestic implements, and of the dress of the Lapps, have been brought in by M. Ostrovski.

A correspondent, writing from Glasgow, describes the following incident, an account of which was given to him by an eye-witness. It occurred at Dumbarton on Friday, the 17th inst. A hare came across the marsh or common there, on to the embankment of the River Leven, near the slaughter-house at present being erected. At the same time a man came along the embankment of the river, making for the new building. The hare, seeing her escape cut off by the man on one side, and by the workmen at the building on the other (she might have escaped across the marsh, the way by which she came, as she did not seem to be pursued), took to the water and swam across. But, unfortunately, at the opposite side her landing was barred by another man. When she saw the danger, she turned, and made for the point she came from, but the man walking along the embankment had by this time come up, and was awaiting her return. On approaching the bank, and seeing that escape was hopeless, the hare gave up, and made no further effort. The man then stepped into the river, and getting hold of her, extinguished any life that was left. "Is it a common occurrence," asks our correspondent, "for the hare to take to the water? I never read or heard of it before."

Dr. Croll, F.R.S., has just completed a volume on "Stellar Evolution and its relations to Geological Time." It will be published immediately.

A THIRD edition of the well-known "Orient Line Guide," by the Rev. W. J. Loftie, has been issued by Messrs. Sampson Low. The abandonment of the Cape route, as the editor points out, has left room for expanded notices of places hardly mentioned in former editions. The whole plan of the volume has accordingly been changed. In its present form the work contains a continuous narrative of a voyage from London to Australia, broken only by a journey home from Naples, and by various interesting excursions. In the accomplishment of this difficult task Mr. Loftie has received aid from several eminent writers, whose contributions add largely to the value of the book.

THE "Record of the Excursions of the Geologists' Association—1860 to 1884," which has been prepared by Mr. T. V. Holmes, F.G.S., is now ready for the press, but it will not be printed until a sufficient number of subscriptions have been promised. The work will consist of over 500 pages, and contain accounts of all the sections and districts visited by the Association down to the end of 1884, with the illustrations (sections, &c.), which have from time to time appeared in the Circulars and Proceedings.

THE Gamble Prize Medal at Girton College has been awarded to Miss Marion Greenwood, certificated student of Girton College, for an essay on "The Digestive Process in certain Simple Organisms—Amœba, Actinosphærium, and Hydra."

"M. F." has sent to the *Times* a list of sixty-nine different species of wild flowers which have been found in blossom during the present month in the neighbourhood of Hardingham, Norfolk. "This fact," he says, "is no doubt partly accounted for by the unusual mildness of the season, but it also speaks well for the climate of the eastern counties, which has been given, I think, a worse character than it deserves. I can vouch for all the flowers being genuinely wild, as they have, without exception, been gathered by my sisters or myself. Among the most remarkable are poppy, white ox-eye, strawberry, pimpernel, primrose, and field scabious."

THE Report of the Director of the Colombo Museum for the past year says that the Reports which have been written by him on the collection of snakes, lizards, and frogs have been printed, and that on the birds is in the press. The Report on the monitors and skinks is finished, but is yet in manuscript. A Report has also been written on the butterflies, to the end of Nymphalidæ, and on the moths to the end of Bombycides. These two latter are not to be printed for the present, for it is hoped that the notes will be supplemented; and it is suggested that the classification and nomenclature be made the same as those adopted in De Nicéville's book on the butterflies of India, which will soon be published. The difficulties met by the Director in the formation of a collection have been many. The entomological specimens have been with difficulty preserved from the attacks of fungus and mites. A strong solution of creosote and benzene was found to be perfectly useless, but sponges soaked in citronella oil and placed in the cases have been fairly successful. At present, the mode of treatment in the Museum is as follows. When the insect is removed from the setting-board, its body is bathed in benzene, and if this does not keep the mites away, the bath is renewed; in case of fungus, the insect is touched with a solution of carbolic acid in benzene, and, as has been just mentioned, sponges soaked in the best citronella oil are always kept in the cases. That this method is effective is shown by the fact that there are specimens in the collections for the past fifteen or twenty years which have been treated in this way, and are now free from attacks. During the past year a great improvement has been made in the transportation of insects to the Museum. A layer of naphthaline is put at the bottom of tins, such as are used for tobacco and butter, then a layer of cotton-wool is spread over this, then a layer of insects, and so on till the tin is filled with alternate layers of wool and insects. By this means the insects are kept relaxed for upwards of a fortnight, and there is thus time to despatch them from any part of the island to Colombo. This plan is far superior to that formerly in use by insect-boxes, setting-boards, and all the other necessary apparatus which were carried from place to place at great trouble and expense. Mr. Haly hopes to establish stations throughout the island, from which insects may be sent from time to time to Colombo. In searching for marine fauna, Mr. Haly found that the dredge employed by naturalists in Europe is almost useless in Ceylonese boats; but one similar to that used by Prof. Agassiz in the Gulf of Mexico, with lighter arms than the common European one, and with perfectly flat scrapers, answered very well. Some fossil crabs from Kuchavelli have also been collected during the past year. In other departments the Museum has been enriched by various rare and interesting specimens.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Coati (*Nasua rufa*) from Pernambuco, presented by Mr. J. W. Bell; a Blue-fronted Amazon (*Chrysotis æstiva*) from Brazil, presented by Miss Hayes; three Common Partridges (*Perdix cinerea*), British, presented by the Rev. F. T. Scott; two Moorish Geckos (*Tarentola mauritanica*) from the South of France, presented by Masters F. and O. Warburg; an Egyptian Cat (*Felis chaus*), a Paradoxure (*Paradoxurus* sp. inc.) from India (?), two Long-tailed Fowls (*Gallus domesticus* var.) from Japan, deposited; a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

MADRAS MERIDIAN CIRCLE OBSERVATIONS, 1865, 1866, 1867.—We have recently received from the Director of the Madras Observatory, the "Results of the Observations of the Fixed Stars made with the Meridian Circle at the Government Observatory, Madras, in the Years 1865, 1866, and 1867," and are glad to see by its appearance that Mr. Pogson is continuing his efforts to remedy the most deplorable delay which has attended the publication of the observations made under his care. The present volume is in continuation of the one which appeared a year and a half ago, and which contained the results for the years 1862–64. The instrument employed and the class of objects observed were the same as in the three earlier years; the objects selected being the moon and moon culminators, Mars and companion-stars, minor planets, the brighter stars down to the fifth magnitude, and as many unnamed stars as possible below 120° N.P.D., and not fainter than the eighth magnitude. The present and preceding volumes have been confined to stellar observations, and these are given separately for each year in the twofold form of star-ledger and annual catalogue. The Star Catalogue will follow at the conclusion of the publication of the results for the separate years, and it is hoped that it may be succeeded by a volume of planetary and cometary results. Very little interest or value now attaches to these sadly overdue volumes of annual results, except as an indication that the evil of delayed publication is now really recognized, and as affording a hope that the one really useful work, the General Star Catalogue, may soon appear. It is but due to Mr. Pogson, however, to remember that his position is one which has presented many difficulties, seeing that he had, as he states in the present volume, "no European assistance, and too inadequate a staff of natives even to admit of duplicate calculations."

COMET 1888 *e* (BARNARD, SEPTEMBER 2).—One of the positions from which Dr. Becker computed the hyperbolic orbit for this comet (NATURE, November 29, p. 114) has been found to be in error, as compared with neighbouring observations by as much as 13″ in declination, and fresh normal places having been formed a parabola is found to satisfy them well The following

elements and ephemeris are by Dr. A. Berberich (*Astr. Nach.*, No. 2867):—

$$T = 1889 \text{ January } 31{\cdot}23814 \text{ Berlin M.T.}$$

$$\left.\begin{aligned} \omega &= 340^\circ\ 28'\ 1''{\cdot}5 \\ \Omega &= 357\ 24\ 48{\cdot}8 \\ \iota &= 166\ 22\ 11{\cdot}2 \end{aligned}\right\} \text{Mean Eq. } 1888{\cdot}0.$$

$$\log q = 0{\cdot}258900$$

Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Jan. 0	0 19 36	7 19·5 S.	0·2688	0·2403	5·5
2	0 15 48	7 13·2			
4	0 12 20	7 6·5	0·2664	0·2633	5·0
6	0 9 9	6 59·3			
8	0 6 14	6 51·8	0·2644	0·2850	4·6
10	0 3 33	6 43·9			
12	0 1 6	6 35·8	0 2626	0·3053	4·2
14	23 58 51	6 27·5			
16	23 56 48	6 19·0 S.	0·2612	0·3242	3·9

The brightness at discovery is taken as unity.

Dr. Copeland (*Dun Echt Circular*, No. 165) anticipates that, though the computed brightness is decreasing, the intrinsic brightness will increase until the beginning of February. Dr. Copeland also announced, in a paper read before the Royal Astronomical Society at its last meeting, that the spectrum of the carbon bands had become decidedly more conspicuous lately than when the spectrum of the comet was first observed.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *DECEMBER* 30—1889 *JANUARY* 5.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on December 30

Sun rises, 8h. 8m.; souths, 12h. 3m. 3·2s.; sets, 15h. 58m.: right asc. on meridian, 18h. 40·5m.; decl. 23° 8′ S. Sidereal Time at Sunset, 22h. 36m.

Moon (New on January 1, 21h.) rises, 4h. 55m.; souths, 9h. 36m.; sets, 14h. 9m.: right asc. on meridian, 16h. 13·1m.; decl. 17° 16′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury	8 26	12 8	15 50	18 45·4	24 52 S.
Venus	10 19	14 57	19 35	21 35·2	16 17 S.
Mars	10 18	15 2	19 46	21 39·8	15 10 S.
Jupiter	6 52	10 49	14 46	17 26·1	22 50 S.
Saturn	19 25*	2 53	10 21	9 29·4	15 58 N.
Uranus	1 20	6 44	12 8	13 21·0	7 52 S.
Neptune	13 31	21 14	4 57*	3 52·5	18 29 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Date	h.	
1888 Dec. 31	14	Jupiter in conjunction with and 2° 11′ south of the Moon.
31	16	Sun at least distance from the Earth.
1889 Jan. 1	—	Total eclipse of the Sun: not visible in the United Kingdom, but will be seen generally over the continent of North America.
2	2	Mercury in conjunction with and 2° 34′ south of the Moon.
2	13	Venus in conjunction with and 0° 40′ south of Mars.
4	22	Mars in conjunction with and 2° 4′ north of the Moon.

Meteor-Showers.

	R.A. °	Decl. °	
From Cancer	120	16 N.	Swift; bright.
Near θ Ursæ Majoris	142	56 N.	Very swift; short.
The *Quadrantids*	228	53 N.	January 1–3.

Variable Stars.

Star	R.A. (1889·0) h. m.	Decl. (1889·0) ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Jan. 3,	22 14	*m*
Algol	3 1·0	40 32 N.	Dec. 30,	0 46	*m*
			Jan. 1,	21 35	*m*
R Canis Majoris	7 14·5	16 11 S.	,, 3,	19 18	*m*
			,, 4,	22 34	*m*
U Monocerotis	7 25·5	9 33 S.	,, 5,		*M*
β Lyræ	18 46·0	33 14 N.	,, 5,	19 0	*M*
R Lyræ	18 52·0	43 48 N.	,, 3,		*m*
X Cygni	20 39·0	35 11 N.	,, 2,	7 0	*M*
T Vulpeculæ	20 46·8	27 50 N.	,, 3,	7 0	*m*
			,, 4,	9 0	*M*
Y Cygni	20 47·6	34 14 N.	Dec. 31,	5 40	*m*
			and at intervals of	36 0	
δ Cephei	22 25·0	57 51 N.	Dec. 31,	2 0	*m*
			Jan. 1,	17 0	*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

IN a paper (accompanied by a map) in the last number of the Proceedings of the Royal Geographical Society, we are told by the author, Captain Langen, that the Key, or Ké, Islands derive their name from the native word Ké (pron. Kay), which signifies "What do you say?" The native tradition runs that when Macassar traders first landed there, and inquired in the Malay tongue after the name of the land they had set foot on, the Key natives answered "Kay?" which expression was mistaken by the questioners for the name of the islands; and under this name, which has been changed into "Key," or "Ké," we find them on charts of the East Indian Archipelago. The islands are very incorrectly laid down on all nautical charts, and Captain Langen's brother has taken great pains in drawing up a map according to his own survey, which is given in illustration of the paper. The group consists of two larger islands, of which the westerly one bears the name of Nuhu-roa, or Little Key, and the easterly one Ju-ud, or Great Key, surrounded by a number of smaller islands. There is no doubt that Great Key is geologically much older than Little Key and the other surrounding islands; it possesses elevations of from 2000 to 3000 feet, whilst Little Key and the other islands are very low. Great Key principally consists of a rocky and volcanic formation, whereas Little Key and the surrounding islands are formed of coral and interveined by flint and quartz. The traveller will find on the highest inland elevations of Little Key (200 feet) shells of various species, greatly damaged through long exposure to wind and weather. About thirty-five years ago, according to the most reliable chiefs, Little Key was raised out of the sea during the shocks of a severe earthquake, attended by a tidal wave; since then no earthquakes occurred until 1884, in the month of April. The members of the European settlement report that the day opened sultry and stifling, the sun shone till about 9 a.m., when the sky became overcast, and at 10 a.m. the first shock was felt, which caused our saw-mill, then in course of erection, to sway to and fro. No sooner had the workmen felt this first shock than they rushed out of the building, and after a short lapse of time two more slight shocks were felt, but fortunately without doing any serious damage. About half an hour's walk eastward from Doelah, is an extinct crater, filled with fresh water, of a great depth, and of a very dark blue colour. Every island belonging to the Key group is covered, down to the water's edge, with dense tropical jungle, gigantic creepers winding themselves from one tree to another, thus forming a close network and great impediment to travelling. These forests contain choice kinds of timber, which formed one of the principal inducements for the establishment of the present German colony.

THE Hon. F. T. Gregory, the well-known Australian traveller, died some weeks ago at his residence in Queensland. He was one of the earliest settlers in Western Australia, where he remained for upwards of thirty years. So far back as 1846, he headed an Expedition to explore the country around the Swan River, and in 1857 he was engaged in exploring the northern coast of Australia. In this latter journey he discovered the Ashburton and Fortescue Rivers. He also drew up a geological map of Western Australia. He went from the latter colony to

Queensland, where he remained till his death, occupying many public posts, the last that he filled being that of Postmaster-General. Some years ago he received the gold medal of the Royal Geographical Society.

THE FARMER'S GUIDE TO MANURING.[1]

THE low average yield of wheat in Australia, of some 8 bushels per acre, appears to be due in a great measure to defective cultivation. Victoria, however, enjoys a more promising soil, and in the little pamphlet before us, 15 to 18 bushels of barley are given as probable yields of this cereal on unmanured land. It has frequently been observed that the most worn out soils respond with the greatest effect to the application of fertilizers, and in agreement with this principle we find that by the use of artificial manures, the 15 bushels is converted into 50, and the 18 bushels into 47. Such results could not happen upon a well-cultivated English farm. The law of the land with reference to its condition appears to be that the higher it is in degree of fertility the greater is the difficulty of producing further increments of produce. This is really a *crux* in English farming. Every succeeding bushel is wrung out of the soil at a greater cost than the last, and this constitutes one of the most difficult problems in connection with high cultivation. Now, in a country like Australia, or even like many of the States of America, this difficulty does not as yet exist, and the land is able, according to Mr. Pearson, to answer with extraordinary alacrity to the application of fertilizers. This is the only way in which we can account for the statement made in the pamphlet in question, and for the fact that it is thought worthy of being published by the Government at Melbourne. Profits of 145 and of 215 per cent. from artificial dressings are somewhat startling, but we are not disposed to dispute their possibility. Such results are not entirely beyond our experience, on worn out soils, when the crop is apparently entirely due to applications of dressings. We have seen on such soils a miserable crop on the portion left unmanured and a good crop on the plots liberally treated, and this, of course, is parallel to the cases cited in Victoria.

The merit in Mr. Pearson's suggestions lies not so much in their originality as in their practical utility. We are familiar with the wants of plants for nitrogen, potash, lime, and phosphoric acid; and there is nothing new in the idea of a "complete manure." But the suggestion that manures should be carefully compounded for experimental purposes, properly labelled, and sent out by well-known firms, subject to Government control, is, if not novel, at least enterprising. These manures are labelled as follows: C. for cereals, L. for Leguminosæ, R. for root crops; and are accompanied with simple directions for their proper application.

Plots one-fortieth of an acre in extent are recommended, and full directions for their measurement are supplied thus:—

If the lands measured from the middle of one furrow to the middle of the next be	9 feet wide,	the length taken should be 121 feet.
	10 ,,	,, ,, ,, ,, ,, 109 ,,
	11 ,,	,, ,, ,, ,, ,, 99 ,,
	12 ,,	,, ,, ,, ,, ,, 91 ,,
	13 ,,	,, ,, ,, ,, ,, 84 ,,
	15 links	,, ,, ,, ,, 1 chain 67 links.
	16 ,,	,, ,, ,, ,, 1 ,, 56 ,,
	17 ,,	,, ,, ,, ,, 1 ,, 47 ,,
	&c.,	&c., &c.

Nothing could well be simpler or more calculated to enlist the sympathies of agriculturists. With properly mixed, weighed, and labelled bags, neatly turned out by good firms, and controlled in composition by the Agricultural Department, and with simple rules for measuring off plots, and with directions for carrying out these simple experiments—what more could a farmer want?

In conclusion, farmers, and all engaged in cultivating the ground, are earnestly recommended to give a trial to this system of test plots. By making use of them they can see from season to season what manure produces the best effect upon their land, and instead of working in the dark they will know exactly how they are laying out their money.

The question is, Will they avail themselves of the offer of the Department, and the assistance promised in carrying out the instructions? Probably not; but one thing is certain—that if the farmers of Australia and America once grasp the idea of intensive culture as opposed to their present system of extensve cultivation, and go in for producing high yields per acre, the promised relief from foreign competition in wheat-growing must be indefinitely postponed. We look forward with some interest to learn how far the efforts of Mr. Pearson and the Minister of Agriculture will be seconded by the farmers of Victoria.

JOHN WRIGHTSON.

[1] "The Farmer's Guide to Manuring," by A. N. Pearson. Printed by order of the Hon. J. L. Dow, Minister of Agriculture, by authority. (Melbourne: Government Printing Office, 1888.)

ON THE DISCOVERY OF THE OLENELLUS FAUNA IN THE LOWER CAMBRIAN ROCKS OF BRITAIN.

THE brief paper on the "Stratigraphical Succession of the Cambrian Faunas in North America," communicated to NATURE (vol. xxxviii. p. 551), will have been read by British students of the geology of the Lower Palæozoic rocks with especial interest and satisfaction, as it puts an end to a controversy between European and American geologists, and brings into harmony the sequence and palæontology of the Cambrian faunas on both sides of the Atlantic.

The remarkable fauna of the *Olenellus*, or lowest Cambrian zones, originally discovered in America, by Dr. Emmons, in 1844, was first recognized in Europe by the late Dr. Linnarsson in 1871, in the basal beds of the Cambrian, near Lake Miösen in Norway; but its typical genus, *Olenellus*, was then referred by him to the allied but more recent *Paradoxides*. This reference was corrected by Prof. Brögger in 1875, and the various brilliant papers on the primordial formations by this author have given the *Olenellus* fauna a marked and peculiar interest. In 1882, Linnarsson next made known the existence of the *Olenellus* fauna in Scania, at the base of the Swedish Cambrian. In 1866 the same fauna was detected by Mickwitz in the Lower Cambrian of Russia (Esthonia), and this Russian fauna has been lately figured and described in detail by Dr. Schmidt, of St. Petersburg. Still more recently (December 1877), Dr. Holm has signalized the existence of the *Olenellus* fauna in the Cambrian of Lapland, where it was detected by Mörtsell in 1855. Thus the existence of this peculiar fossil-group (the oldest well-marked fauna yet recognized by geologists), in the Lower Cambrian rocks, has been already demonstrated in three main regions: (1) in the region of the Rocky Mountains; (2) in the region of North-East America; and (3) in the region drained by the Baltic Sea. But, up to the present time, no notice of its presence has been recorded from the British Islands, where the oldest fauna hitherto described is that of the overlying *Paradoxides* zones, or Middle Cambrian formation.

The existence of traces of the *Olenellus* fauna in the Cambrian rocks of the west of England, has, however, been known to myself for some time. The first recognizable fragments of the characteristic genus *Olenellus* were detected by me on the flanks of Caer Caradoc, in Shropshire, in 1885, but they were too imperfect for description. During the summers of 1887 and 1888, Mr. H. Keeping, who has been collecting under my direction the characteristic fossils of the Lower Palæozoic rocks of the district for the Woodwardian Museum, has obtained a sufficiency of fragments to enable us to recognize a large and well-marked species of *Olenellus*. This species possesses characters apparently intermediate between the European form *Olenellus Kjerulfi* (Linnarsson), and the undescribed American form *Olenellus Bröggeri* (Walcott, *MS.*); but it is so closely allied to the last-named species, that I prefer to await the publication of Walcott's diagnosis of his form before publishing its specific description. I have provisionally named it *Olenellus Callavei*, after Dr. C. Callaway, F.G.S., who was the first to demonstrate the presence of fossiliferous Cambrian rocks in this Shropshire district, and to collect Cambrian fossils from the strata under notice.

The Lower Cambrian or *Olenellus* formation of this Shropshire area consists of two main members: (*a*) the basal Quartzite of Lawrence Hill and Caer Caradoc, and (*b*) an overlying green sandstone, the Comley Sandstone (Hollybush Sandstone of Dr. Callaway). This formation follows unconformably upon the so-called Uriconian volcanic rocks of the district, and occurs in many localities, as at Lilleshall, the Wrekin, Caer Caradoc, Cardington, &c. In mapping this formation through the district, I find that its fossils are mainly confined to the sandstones, and to certain calcareous and phosphatic beds within them. In addition

to *Olenellus*, we find in various localities such characteristic Lower Cambrian forms as *Kutorgina*, *Mickwitzia* (?), and *Acrothele*. The strata of this *Olenellus* zone are succeeded irregularly by (usually faulted against) the Shineton Shales of Dr. Callaway, which are known to contain in their highest zones an abundant fauna of Tremadoc (Upper Cambrian) age. No trace of the intermediate or *Paradoxides* fauna has yet been detected.

Although this discovery has been well known to my fellow-workers among the Lower Palæozoic rocks, I have refrained from placing it upon record until my identifications had been confirmed by foreign palæontologists familiar with the *Olenellus* fauna abroad. As the specimens I exhibited at the London meeting of the Geological Congress were unhesitatingly referred to the typical *Olenellus* fauna both by Mr. Walcott and Dr. Schmidt, there is no longer any excuse for withholding its publication. The necessary geological and palæontological details will appear in due course, but as these new facts may, it is to be hoped, lead geologists in the meantime to a renewed investigation of the strata and fossils of the more ancient formations, it will perhaps be of service to point out that the detection of this lowest Cambrian fauna in beds superior to the Wrekin quartzite opens out a fresh series of problems in British geology. Thus the presence of *Olenellus* in these beds appears at first sight to fix distinctly the pre-Cambrian age of the so-called Uriconian rocks of the Wrekin and their British equivalents, and even to render the pre-Cambrian age of the Longmyndian a matter of fair probability. With the Longmyndian would possibly go the Torridon rocks of North-West Scotland, the schists of St. Lo in France, the Sparagmites of Norway, &c. Again, if the Wrekin quartzite is, as has been more than once suggested, the extension of that of Nuneaton and Durness, then our so-called Upper Cambrian of the Malverns, Central England, and North-West Scotland may be in reality a greatly attenuated representative of the Cambrian system in general, the British extension of the remarkably attenuated Proterozoic formations of Western Europe. If so, this attenuated Cambrian may eventually be mapped as patches of an originally fairly continuous band, ranging from Lapland, through Esthonia, Scania, Norway, Scotland, Central England, France, and Spain, to the Island of Sardinia. The Sardinian and Durness formations, on the extreme south-east and north-west points of this line, would agree in lithology, age, and fauna, both ranging from the base of the Cambrian up to the lowest zones of the Ordovician. It should be carefully borne in mind, however, that in the present state of our knowledge these suggestions must be regarded simply as constituting a provisional working hypothesis, of service mainly as a stimulus to future discussion, investigation, discovery, and correction.

Grouping together, however, such facts as are already known, and employing Mr. Walcott's nomenclature, we are now able to parallel his American tables by the following European equivalents:—

TABLE I.—*North-Western Europe.*

		Norway.	Sweden.	Esthonia.	Lapland.
Cambrian System.	Upper Cambrian or *Olenus* Zones.	*Dictyonema* and *Olenus* Zones.	*Dictyonema* and *Olenus* Zones.	*Dictyonema.*	Unknown.
	Middle Cambrian or *Paradoxides* Zones.	*Paradoxides* Zones.	*Paradoxides.*	Unknown.	?
	Lower Cambrian or *Olenellus* Zones.	*Olenellus* Zones.	*Olenellus.*	*Olenellus.*	*Olenellus.*

TABLE II.—*British Islands.*

		Shropshire.	St. David's.	Merionethshire.	Central England.	Durness.
Cambrian System.	Upper Cambrian or *Olenus* Zones.	*Dictyonema* and *Olenus* Zones.	*Olenus.*	*Dictyonema* and *Olenus.*	*Dictyonema* and *Olenus.*	*Salterella* and *Archæocyathus* Fauna.
	Middle Cambrian or *Paradoxides* Zones.	Unknown.	*Paradoxides.*	*Paradoxides.*	Unknown.	
	Lower Cambrian or *Olenellus* Zones.	*Olenellus.*	Unknown.	Unknown.	Unknown.	

TABLE III.—*Central and South-Western Europe.*

		Central Europe.	Belgium.	Montagne Noire, South-East France.	Spain.	Island of Sardinia.
Cambrian System.	Upper Cambrian or *Olenus* Zones.	*Olenus* (Hof.).	*Dictyonema.*	*Olenus.*	?	*Paradoxides* and *Archæocyathus* Fauna.
	Middle Cambrian or *Paradoxides* Zones.	*Paradoxides* (Bohemia).	Unknown.	*Paradoxides.*	*Paradoxides.*	
	Lower Cambrian or *Olenellus* Zones.	Unknown.	Unknown.	Unknown.	Unknown.	

C. LAPWORTH.

THE FORESTS OF UPPER BURMAH.

MR. H. C. HILL, the Conservator of the Forests of Upper Burmah, in his Report for the past year—that is, the first year of the existence of a Forest Department in that territory—says that even in that short time a great advance has been made in the protection of forests. Though the Secretary of State sanctioned the appointment of a staff of two conservators and nineteen assistant conservators, and the Indian Government decided that for the present one conservator and fifteen assistants should be appointed, yet the staff in July numbered no more than eleven. The addition of two more is, however, promised at an early date. The work done by this small staff has been very difficult. The areas are enormous, a division averaging about 4000 square miles, the forest land of that tract being from one-third to one-half of the area. Besides, every opposition has been put in the way of the work of the Department by the natives, who have been accustomed in the past to cut the forest timber as they liked. And so it became necessary to send armed escorts with the officers. The Inspector-General's suggestion that a sufficiently strong force of armed men should be organized to protect the forest officers was not acted on. A body of about two hundred police are now constantly engaged in this service. Occasionally difficulties have cropped up, especially when long marches were to be made, but on the whole the present system has worked well. The knowledge that Dacoits might be met with at any turn has to some extent hampered the operations of the forest officials. From the reckless cutting of timber in the past it is probable that the supply in accessible forests will prove smaller than was anticipated. No actual demarcation has as yet taken place, but 5560 acres in the Ruby Mine district have been inspected, and a further area of 2440 acres has been described as suitable for reserve land. During the year fifty-seven persons were convicted of various offences against the forest regulations. Nurseries have been established at Bernardymo and Mogouk, where besides the work done in forest trees, 500 European grafted fruit trees were planted, and of these 149 grew, including pears, apples, peaches, apricots, and plums. One great difficulty has been the disputes between the original lessees of the Royal forests and the present Forest Department. The Government offer has been accepted by the Bombay Burmah Corporation, but other lessees have not yet assented to the terms. The Government propose to continue the rights under the various leases to the holders under new agreements, substituting a system of payment on the timber actually extracted for the yearly lump-sum payments, and enforcing the rules and regulations as regards girdling, felling of green teak, and all other matters connected with the girdling of the forests. The effect of the war can be seen in the returns of the amount of timber felled. Thus the Bombay Burmah Corporation extracted from the Pyinmana forests in 1885, 63,000 tons; in 1886, 18,000 tons; in 1887, 26,000 tons. There has been a serious loss to the forest revenue in the past year by the wholesale plundering of unmarked timber by local traders. After passing through various hands, this timber finally reached a revenue station, where it was passed into the market centres on payment of the local duty.

THE COCOA-NUT PALM.

THE Government Press at Madras recently issued "A Monograph on the Cocoa-nut Palm, or *Cocos nucifera*," by Dr. John Short, which, the introduction tells us, was written at the request of the Director of Revenue, Settlement, and Agriculture. The author begins by pointing out the area of distribution of the cocoa-nut tree. It is indigenous in the East, and is now largely cultivated on the coasts of India and Ceylon, and in the islands of the Eastern Archipelago. There are as many as twenty millions in the south-west of Ceylon. The palm frequently grows wild in distant and isolated islands, whither the germ has been borne by the sea, the thick fibrous padding around the nut protecting it from the action of the water. So we constantly see that coral reefs, as soon as they make their appearance above the surface of the water, are taken possession of by these trees. The sea-shore is the home of the palm; it grows quite down to the water's edge, and is in many places constantly washed by the waves. Thus, along the Brazilian coast for a distance of nearly 280 miles, from the River San Francisco to the bar of Mamanguape, these trees extend. We also, however, find them far inland, and at the height of several thousand feet above the level of the sea. At Bangalore they flourish and produce fruit in abundance at a height of 3000 feet above the sea-level. From a dietetical and economical point of view, the cocoa-nut palm is a most valuable plant; sugar, starch, oil, wax, wine, resin, astringent matters, and edible fruits are its gifts to man. An alluvial or loamy soil is the most suitable for planting it, and no more than 80 plants an acre should be planted to get the maximum amount of fruit possible. Nuts obtainable from trees of from fifteen to thirty years old are the best for planting. There are numerous varieties of this tree, there being as many as thirty in Travancore alone. One dwarf variety bears fruit when it is only 2 feet in height. Toddy is the sap of the cocoa-nut palm, and when the toddy-drawer wishes to get out the sap of the tree, he binds the flower spathe tightly with fibres of the tree, and beats it twice a day for three or four days with a short stick. The top is then sliced, and as soon as the sap begins to flow, a vessel, either earthen or made of bamboo, is tied to the spathe to receive the sap. The spathe is kept bleeding by making a fresh wound in it each day. The fluid, when fresh, has a pleasant taste, and is slightly aperient. When kept for a few hours, it ferments and becomes somewhat intoxicating, and it may then be distilled into spirits or vinegar. With bakers it takes the place of yeast. The quantity of toddy taken out varies with the age and locality of the spathe, but the average quantity obtained for two or three weeks is three or four quarts every twenty-four hours. The liquid is also boiled down into a coarse kind of sugar called jaggery, which is either converted into molasses, or refined before fermentation sets in into white or brown sugar. In some places the occupation of toddy-drawer is an hereditary one. Their mode of work is very simple, but is extremely dangerous. A thong made of bullock or buffalo hide, from 3 to 6 inches in width, and long enough to surround the tree and the body of the climber, is fastened with a peculiar kind of knot. The worker then stretches the thong to its utmost by throwing his whole weight on it, and draws up his legs. He has a ring of rope of palmyra fibres around his insteps, which allows him to grasp the tree between his heels. While his left hand is pressed against the trunk he shifts the thong up the tree with his right and draws his body up with it.

"Cocoa-nut day" is celebrated in most parts of India during the full moon in August. On that day numbers of nuts are thrown into the sea as an offering to the Hindu gods. Occasionally one meets with deformed nuts, consisting of the husk with small deformed nuts having no kernel inside. The natives attribute this blighting of the fruit to the tree frog (*Polypedates maculatus*), which, by smelling the flower, can prevent the fruit from coming to maturity. The kernel of the nut is frequently made into ornaments for the hair, or necklaces. The plants, Dr. Short says, are subject to disease from two opposite causes: first, from too much moisture, as in swampy soils, where the fronds are usually small and ill-formed, and the fruit scarce; secondly, from lack of moisture, where the soil is hard and dry, the sap-bearing vessels shrink and the plant perishes. Amongst the insects and animals destructive to the palm may be mentioned the *Calandra palmarum*, or cocoa-nut weevil, which eats its way into the heart of the tree, and forms its cocoon there; the *Butocera rubus*, or cocoa-nut beetle; the *Oryctis rhinocera*, or rhinoceros beetle; the *Pteromyes petaurista*, or flying squirrel; the *Sciurus palmarum*, or common striped palm squirrel; the *Pteropus edwardsi*, or flying fox; and the *Paradoxurus musanga*, or tree-dog. The rat family is very destructive, particularly in the Laccadives. It is exceedingly difficult to get at these rats, they make to themselves so many hiding-places amongst the trees. Rat hunts are, however, occasionally got up, and to these all the inhabitants turn out with sticks and poles. While some of the hunters climb the trees and drive out the rats, the rest surround the trunks and kill the animals as they rush down. On some of these occasions thousands of rats are killed. The people, being Mohammedans, cannot be induced to keep dogs. It only remains to add that there are ten excellent illustrations in this monograph.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following open scholarships and exhibitions were awarded at St. John's College, in mathematics and natural science, on December 21:—

Foundation Scholarships: Hough, Christ's Hospital (£80); Pocklington, Yorkshire College (£80); Chevalier, Cowper Street School (£60); Rosenberg, private tuition (£50). Exhibitions: Morton, Queen's College, Belfast (£70); Franks, Coatham School, Redcar (£50); Le Sueur, University College, Aberystwith (£33⅓). Sizarships: Cummings, Henderson, Dale, Legg.

SCIENTIFIC SERIALS.

We have received a new instalment of the current volume of the Annals of the Moscow Observatory (series ii. vol. i. fasc. 2), published in French and German by Prof. Bredichin. It contains four papers, by the editor, on the comets of 1886 and 1887 I.; the results of M. Bélopolsky's observations of the last total solar eclipse at Yurievets, with interesting reproductions of photographs of the corona, and remarks upon the movements on the surface of the sun; photoheliographic observations made in 1885, by the same author; photometric observations, by W. Ceraski; and a paper on the rotation of the red spot in Jupiter, by P. Sternberg. All published observations which were made in Europe and the States from 1879 to February 1888 are given, and the conclusion is, that the spot did not change its position in 1879 and 1880, but has changed it by 0·0893h. since 1880–81, which change cannot be explained by mere variations of its shape.

The last volume of the *Mémoires* of the Kharkoff Society of Naturalists contains a very full list of vascular plants in the neighbourhood of Voronezh, by L. Gruner. The names of 778 species are given, but, the aquatic plants being still only imperfectly known, the Voronezh flora will probably include more than 800 species of Phanerogams.—The *Mastigophoræ* and *Rhizopodæ* of the salt lakes of Slavyansk are described by M. Vysotski; the *Chlorosporæ* of Kharkoff, by M. Alexéenko; and the *Chrysididæ* and *Tenthredinidæ* of Kharkoff, by Prof. Jarochewsky.

Journal of the Russian Chemical and Physical Society, vol. xx. fasc. 7.—Full reports on the eclipse of the sun of August 19, 1887 (continued). The reports of the various observations at Krasnoyarsk are given in full.—Notes on the action of acids, the tertiary acetate of amyl, and on the combinations of amylene with acids, by D. Konovaloff.

SOCIETIES AND ACADEMIES.

London.

Royal Society, December 6.—"On *Thylacopardus australis*, Owen. By Sir Richard Owen, F.R.S.

This paper, illustrated by drawings of the natural size, was descriptive of a skull discovered in one of several small caverns at a depth of 80 feet from the surface, in New South Wales. The essential characters of the dentition were those of the feline mammals, save that the piercing and killing teeth were the foremost pair of incisors; the work of molars was mainly done by a single large trenchant or sectorial tooth on each side of both jaws. This tooth was followed by a small tubercular molar in close contact therewith, and by a second similar molar with an intervening vacancy. The latter character is that by which the present fossil differs from the Australian one described in the author's work "The Extinct Mammals of Australia," 2 vols. quarto, 1877. The largest carnivorous kind, equalling the lion in size, bears the name of *Thylacoleo*; the skull of the present fossil shows that a Carnivore of the size of a leopard formerly, also, roamed over the land of kangaroos. These, being the largest native quadrupeds seen by Banks and Captain Cook, were described by Dr. Shaw under the name of *Macropus major*.

Anthropological Institute, December 11.—Francis Galton, F.R.S., President, in the chair.—Dr. J. G. Garson exhibited a new form of anthropometric instrument, specially designed for the use of travellers.—Dr. R. H. Codrington read a paper on social regulations in Melanesia. The part of Melanesia in view comprised the Northern New Hebrides, the Banks's Islands, Sta. Cruz, and the South-Eastern Solomon Islands. The social regulations which obtain among the people were described from personal observation, and from information given by natives. A considerable portion of the whole subject was thus in view, and with particular differences there is a general agreement, from which a wider likeness throughout the Melanesian population may be inferred. The social regulations dealt with were only those relating (I.) to *Marriage*, and (II.) to *Property*. *I. Social Regulations relating to Marriage.* (1) The entire arrangement of society depends on the division of the whole people, in every settlement, large or small, into two or more classes, which are exogamous, and in which descent follows the mother. This division comes first of all things in native thought, and all social arrangements are founded upon it. A woman regards mankind as divided into husbands and brothers; a man regards womankind as divided into wives and sisters—at least, on about the same level of descent. (2) The members of these divisions are all intermixed in habitation, property, subordination to a chief, and in the well-understood relationship through the father; the divisions, therefore, are not tribes. (3) Examples from two regions—(*a*) where these divisions are two, as in the Banks's Islands and Northern New Hebrides; (*b*) where there are more than two, as in Florida, in the Solomon Islands. (*a*) 1. Where there are two divisions there is no name to either. In Mota there are two *veve* (distinction); in Lepers' Island two *wai-vung* (bunch of fruit). 2. The divisions are strictly exogamous; irregular intercourse between members of the same is a heinous crime; avoidance of the person and name of father-in-law, &c., is the custom. 3. No communal marriage in practice, or tradition of it; yet a latent consciousness of the meaning of the words used for husband and wife, mother, &c. The story of Qat shows individual marriage. The levirate, and practice of giving a wife to set up a nephew in the world. 4. Descent through the mother makes the close relation of sister's son and mother's brother; the son takes his mother's place in the family pedigree. Certain rights of the sister's son with his uncle. The mother is in no sense head of the family. The bridegroom takes his bride into his father's house, if not into his own. 5. A certain practice of *couvade* prevails. 6. No capture in marriage. Adoption of no importance. (*b*) 1. In Florida, in the Solomon Islands, and the neighbourhood, is found an example of four or six divisions, called *kema*. In strict exogamy, descent following the mother, and local and political intermixture, all is the same as in the Banks's Islands. But each *kema* has its name, and each has its *buto*, that which the members of it must abstain from. The names are some local, some taken from living creatures. The *buto* is mostly something that must not be eaten. 2. Question whether totems are present. The bird which gives its name to one *kema* is not the *buto* of it, can be eaten. Comparison from the Island of Ulawa. 4. Exceptional condition of part of Malanta and San Cristoval, in the apparent absence of exogamous divisions of the people, and in descent being counted through the father. *II. Property and Succession. A.* 1. Land is everywhere divided into (1) the town; (2) the gardens; (3) the bush. Of these, the first two are held in property, the third is unappropriated. 2. Land is not held in common, *i.e.* each individual knows his own; yet it is rather possession and use for the time being of what belongs to the family, and not to the individual. A chief has no more property in the land than any other man. Sale of land was very rare before Europeans came; and sale of land by a chief beyond his own piece, no true sale. Example at Saa of the fixed native right of property in land. Abundance makes land of little value. 3. Land reclaimed from the bush by an individual, and the site of a town founded on the garden ground of an individual, has a character of its own. 4. Fruit-trees planted by one man on another's land remain the property of the planter and his heirs. In a true sale the accurate and particular knowledge of property in land and trees is remarkably shown. 5. Personal property is in money, pigs, canoes, ornaments, &c. *B.* 1. The regular succession to property is that by which it passes to the sister's son, or to others who are of kin through the mother. 2. But that which a man has acquired for himself he may leave to his sons, or his sons and their heirs may claim. This is the source of many quarrels, the character of a piece of land being forgotten, or disputed by the father's kin. 3. Hence a tendency to succession to the father's property by his sons follows on the assertion of paternity, and the occupation of new ground. 4. A man's kin still hold a claim on his personal property, but his sons, who are not his kin, will generally obtain it.—In the absence of the author, Dr. Edward B. Tylor read a paper by Mr. A. W. Howitt on Australian message sticks and messengers. The use of message sticks is not universal in Australian tribes, and the degree of perfection reached in conveying informarion by them differs much. Some tribes, such as the Dieri, do not use the message stick at all, but make use of emblematical tokens, such as the net carried

by the *pinya*, an armed party detailed by the council of headmen of the tribe to execute its sentences upon offenders. Other tribes, such as the Kurnai, use pieces of wood without any markings. Others, again, especially in Eastern Queensland, use message sticks extensively, which are often elaborately marked, highly ornamented, and even brightly painted. No messenger, who was known to be such, was ever injured. The message stick was made by the sender, and was kept by the recipient of the message as a reminder of what he had to do. For friendly meetings the messenger of Kurnai, of Gippsland, carried a man's kilt and a woman's apron hung on a reed; but for meetings to settle quarrels or grievances by a set fight, or for hostile purposes generally, the kilt was hung upon the point of a spear. Among the Wotjoballŭk of the Wimmera River in Victoria, the principal man among them prepares a message stick by making certain notches upon it with a knife. The man who is to be charged with the message looks on, and thus learns the connection between the marks upon the stick and his message. A notch is made at one end to indicate the sender, and probably notches also for those who join him in sending the message. If all the people of a tribe are invited to attend a meeting, the stick is notched from end to end; if part only are invited, then a portion only of the stick is notched; and if very few people are invited to meet or referred to in the verbal message, then a notch is made for each individual as he is named to the messenger. The messenger carried the stick in a net-bag, and on arriving at the camp to which he was sent, he handed it to the headman at some place apart from the others, saying to him, "So-and-so sends you this," and he then gives his message, referring, as he does so, to the marks on the message stick. The author gives an explanation of the method adopted for indicating numbers, which fully disposes of the idea that the paucity of numerals in the languages of the Australian tribes arises from any inability to conceive of more numbers than two, three, or four. A messenger of death painted his face with pipe-clay when he set out, but did not in this tribe carry any emblematical token. Among the Wirajuri of New South Wales, when the message was one calling the people together for initiation ceremonies, the messenger carried a "bull-roarer," a man's belt, a man's kilt, a bead string, and a white head band, in addition to the message stick. In New South Wales, the Kaiabara tribe use message sticks cut in the form of a boomerang, to one end of which a shell is tied. As a rule the notches on a message stick are only reminders to the messenger of the message he is instructed to deliver, and are unintelligible to a man to whom they have not been explained; but certain notches appear to have a definite meaning and to indicate different classes; and among the Adjadura there is an approach to a fixed rule, according to which these sticks are marked, so that they would convey a certain amount of meaning definitely to an Adjadura headman independently of any verbal message.

Mathematical Society, December 13.—J. J. Walker, F.R.S., President, in the chair.—Dr. Glaisher, F.R.S., communicated a geometrical note by Mr. H. M. Taylor.—Mr. Love read a paper on the equilibrium of a thin elastic spherical bowl.—The President (Prof. Greenhill, F.R.S., in the chair) contributed some illustrations of a former paper on a method in the analysis of ternary forms.—The Secretary read an abstract of a paper on a method of transformation with the aid of congruences of a particular type, by Mr. J. Brill.

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EDINBURGH.

Royal Society, December 3.—Sir Douglas Maclagan, Vice-President, in the chair.—The Chairman gave an opening address.—Dr. John Murray communicated a paper by Mr. H. B. Brady on the Ostracoda collected in the South Sea Islands. One fresh-water specimen obtained in New Zealand is described. The rest were collected between the tide-marks or at depths of not more than 6 fathoms. The internal structure is not described, as the specimens were preserved in the dry state. Fifty new species and two new genera occur.—Dr. Murray communicated also a paper by Dr. O. von Linstow on *Pseudalius alatus*, Leuck., collected by Mr. Robert Gray in the Arctic Seas, and other species of the genus. A detailed description of this Entozoon is given, it having been only once previously described, and that imperfectly. Six other species of the same genus are described—Prof. Patrick Geddes read the first part (botanical and zoological) of a restatement of the theory of organic evolution. He drew attention to the two tendencies—vegetative and reproductive—which exist in organic nature, and asserted that evolution is the result of the universal subordination of the former to the latter.

STOCKHOLM.

Royal Academy of Sciences, December 12.—Contributions to our knowledge of the habits of solitary wasps, by Prof. Chr. Aurivillius.—On the singular points of such functions as are defined by non-linear differential equations, by Prof. Mittag-Leffler.—On the influence of the woods on the climate of Sweden, by Dr. Hamberg.—Singular generatrices in algebraic rule surfaces, by Prof. Björling.—On the systematic value of the varieties of herring, by Prof. F. A. Smitt.—On dinitronaphthalin-sulphon acid and some of its derivatives, by Herr P. Hellström.—On naphthoë acids, by Dr. Ekstrand.—On the action of fuming sulphuric acid on amido-naphthalin-sulphon acids, by Herr Forsling.—On the structure of the auricles in the Echinococonidæ, by Prof. S. Lovén.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Puff: Mrs. Macquoid (S.P.C.K.).—Rides and Studies in the Canary Islands: C. Edwardes (Unwin).—Elementos de Estatika Grafica: J. Schloske, traducidos del Alemán por V. Balbin (Buenos Aires).—Tratado de Geometria Analitica: J. Casey; traducido del Inglés por V. Balbin (Buenos Aires).—Carl von Linné's Ungdomsskrifter, 1, 2 (Stockholm).—*Challenger* Report—Zoology, vol. xxviii. (Eyre and Spottiswoode).—Elementary Building Construction and Drawing: E. J. Burrell (Longmans).—Atlas of Chemistry, part 1: V. V. Branford (Edinburgh, Livingstone).—Visitors' Guide to Salem (Salem, Mass.).—Bibliography of Astronomy for the Year 1887: W. C. Winlock (Washington).—The Beginning of American Science—The Third Century: G. Browne Goode (Washington).—On the Variation of Decomposition in the Iron Pyrites, 2 parts: A. A. Julien.—Journal of the Royal Microscopical Society, December (Williams and Norgate).—Essex Institute Historical Collections, vol. xxiv., January to December 1887 (Salem, Mass.).—Botanische Jahrbücher für Systematik Pflanzengeschichte, und Pflanzengeographie, Zehnter Band, iv. Heft (Williams and Norgate).—Journal and Proceedings of the Royal Society of New South Wales, vol. xxii. part 1 (Trübner).—Beiblätter zu den Annalen der Physik und Chemie, 1888, No. 11 (Leipzig).—Transactions of the Leicester Literary and Philosophical Society, October (Leicester).—The Encyclopædic Dictionary, vol. vii. part 2 (Cassell).—Catalogue of the Marsupialia and Monotremata in the Collection of the British Museum, Natural History (O. Thomas, London).—Die Mechanik in Ihrer Entwickelung: Dr. E. Mach (Brockhaus, Leipzig).—Year-book of Pharmacy, 1888 (Churchill).—Les Stations de l'Age du Renne, fasc. 1 (Baillière et Fils, Paris).—Prace Matemalyczno-Fizyezue, tom. i. (Warszawa).

CONTENTS.

THURSDAY, JANUARY 3, 1889.

SCIENTIFIC WORTHIES.

XXV.—James Joseph Sylvester.

JAMES JOSEPH SYLVESTER, born in London on September 3, 1814, is the sixth and youngest son of the late Abraham Joseph Sylvester, formerly of Liverpool.[1] He was educated at two private schools in London, and at the Royal Institution, Liverpool, whence he proceeded in due course of time to St. John's College, Cambridge. In these early days he manifested considerable aptitude for mathematics, and so it was not matter for surprise that he came out in the Tripos Examination of 1837 as Second Wrangler; being incapacitated, by the fact of his Jewish origin, from *taking* his degree, he was not able to compete for either of the Smith's Prizes. In more enlighted times (1872) he had the degrees of B.A. and M.A., by accumulation, conferred upon him, and received therewith the honour of a Latin speech from the Public Orator. He himself says: "I am perhaps the only man in England who am a full (voting) Master of Arts for the three Universities of Dublin, Cambridge, and Oxford, having received that degree from these Universities in the order above given: from Dublin, by *ad eundem;* from Cambridge, *ob merita;* from Oxford, by decree." He is now D.C.L. of Oxford, LL.D. of Dublin and Edinburgh, and Hon. Fellow of St. John's College, Cambridge. It is still open for him to receive yet higher recognition from his own *alma mater.*

Prof. Sylvester became a student of the Inner Temple, July 29, 1846, and was called to the Bar on November 22, 1850.[2] He has been Professor of Natural Philosophy at University College, London; of Mathematics at the University of Virginia, U.S.A.;[3] then ten years later Professor at the Royal Military Academy, Woolwich; and again, after a five years' interval, Professor of Mathematics at the Johns Hopkins University, Baltimore, U.S.A., from its foundation in 1877. Finally, in December 1883, he was elected Savilian Professor of Geometry at Oxford, in succession to Prof. Henry Smith.[4] His first printed paper was on Fresnel's optical theory (in the *Phil. Mag.*, 1837).

We can here only briefly allude to a communication which was accompanied by many important results: we refer to the Friday evening address (January 23, 1874) to the Royal Institution, "On Recent Discoveries in Mechanical Conversion of Motion." He says:—"It would be difficult to quote any other discovery which opens out such vast and varied horizons as this of Peaucellier's,—in one direction, descending to the wants of the workshop, the simplification of the steam-engine, the revolutionizing of the mill-wright's trade, the amelioration of garden-pumps, and other domestic conveniences (the sun of science glorifies all it shines upon) and in the other, soaring to the sublimest heights of the most advanced doctrines of modern analysis, lending aid to, and throwing light from a totally unexpected quarter on the researches of such men as Abel, Riemann, Clebsch, Grassmann, and Cayley. Its head towers above the clouds, while its feet plunge into the bowels of the earth."

The only works that Prof. Sylvester has published, we believe, are: (1) "A Probationary Lecture on Geometry, delivered before the Gresham Committee and the Members of the Common Council of the City of London, December 4, 1854," a slight thing which had to be written and delivered at a few hours' notice; (2) "Laws of Verse," 1870; (3) several short poems, sonnets, and translations, which have appeared in our columns and elsewhere.

Our notice would be incomplete without some record of the honours that have been conferred upon Dr. Sylvester. He was elected a Fellow of the Royal Society on April 25, 1839; has received a Royal Medal (1860) and the Copley Medal (1880), this latter rarely awarded, we believe, to a pure mathematician. On this last occasion, Mr. Spottiswoode accompanied the presentation with the words, "His extensive and profound researches in pure mathematics, especially his contributions to the theory of invariants and covariants, to the theory of numbers and to modern geometry, may be regarded as fully establishing Mr. Sylvester's claim to the award." He is a Fellow of New College, Oxford; Foreign Associate of the United States National Academy of Sciences; Foreign Member of the Royal Academy of Sciences, Göttingen, of the Royal Academy of Sciences of Naples, and of the Academy of Sciences of Boston; Corresponding Member of the Institute of France, of the Imperial Academy of Science of St. Petersburg, of the Royal Academy of Science of Berlin, of the Lyncei of Rome, of the Istituto Lombardo, and of the Société Philomathique. He has been long connected with the editorial staff of the *Quarterly Journal of Mathematics* (under one or another of its titles), and was the first editor of, and is a considerable contributor to, the *American Journal of Mathematics;* and he was at one time Examiner in Mathematics and Natural Philosophy in the University of London. He was not an original member of the London Mathematical Society (founded January 16, 1865), but was elected a member on June 19, 1865, Vice-President on January 15, 1866, and succeeded Prof. De Morgan as the second President on November 8, 1866. The Society showed its recognition of his great services to them and to mathematical science generally by awarding him its De Morgan Gold Medal in November 1887. Wherever Dr. Sylvester goes, there is sure to be mathematical activity; and the latest proof of this is the formation, during the last term at Oxford, of a Mathematical Society, which promises, we hear without surprise, to do much for the advancement of mathematical science there.

The writings of Sylvester date from the year 1837; the number of them in the Royal Society Index up to the year 1863 is 112, in the next ten years 38, and in the forthcoming volume 81, making 231 for the years 1837 to 1883: the number of more recent papers is

[1] Foster's "Hand-book of Men at the Bar" [2] Foster, *l.c.*

[3] The late Prof. Key, of University College and School, was the first occupant of the Chair, founded by Mr. Jefferson, once President of the United States, in 1824.

[4] He commences his Oxford lecture (NATURE, vol. xxxiii. p. 222), of date December 12, 1885, with the words: "It is now two years and seven days since a message by the Atlantic cable containing the single word 'elected' reached me in Baltimore informing me that I had been appointed Savilian Professor of Geometry in Oxford, so that for three weeks I was in the unique position of filling the post and drawing the pay of Professor of Mathematics in each of two Universities."

also considerable. They relate chiefly to finite analysis, and cover by their subjects a large part of it: algebra, determinants, elimination, the theory of equations, partitions, tactic, the theory of forms, matrices, reciprocants, the Hamiltonian numbers, &c.; analytical and pure geometry occupy a less prominent position; and mechanics, optics, and astronomy are not absent. A leading feature is the power which is shown of originating a theory or of developing it from a small beginning; there is a breadth of treatment and determination to make the most of a subject, an appreciation of its capabilities, and real enjoyment of it. There is not unfrequently an adornment or enthusiasm of language which one admires, or is amused with: we have a motto from Milton, or Shakespeare; a memoir is a trilogy divided into three parts, each of which has its action complete within itself, but the same general cycle of ideas pervades all three, and weaves them into a sort of complex unity; the apology for an unsymmetrical solution is—symmetry, like the grace of an eastern robe, has not unfrequently to be purchased at the expense of some sacrifice of freedom and rapidity of action; and, he remarks, may not music be described as the mathematic of sense, mathematic as the music of the reason? the soul of each the same! &c. It is to be mentioned that there is always a generous and cordial recognition of the merit of others, his fellow-workers in the science.

It would be in the case of any first-rate mathematician—and certainly as much so in this as in any other case—extremely interesting to go carefully through the whole of a long list of memoirs, tracing out as well their connection with each other, and the several leading ideas on which they depend, as also their influence on the development of the theories to which they relate; but for doing this properly, or at all, space and time, and a great amount of labour, are required. Short of doing so, one can only notice particular theorems—and there are, in the case of Sylvester, many of these, "beautiful exceedingly," which, for their own sakes, one is tempted to refer to—or one can give titles, which, to those familiar with the memoirs themselves, will recall the rich stores of investigation and theory contained therein.

A considerable number of papers, including some of the earliest ones, relate to the question of the reality of the roots of a numerical equation: in the several connections thereof with Sturm's theorem, Newton's rule for the number of imaginary roots, and the theory of invariants. Sylvester obtained for the Sturmian functions, divested of square factors, or say for the reduced Sturmian functions, singularly elegant expressions in terms of the roots, viz. these were $f_2(x) = \Sigma(a - b)^2(x - c)(x - d) \ldots$, $f_3(x) = \Sigma(a - b)^2(a - c)^2(b - c)^2(x - d) \ldots$, &c.; but not only this: applying the Sturmian process of the greatest common measure (not to $f(x), f'(x)$, but instead) to two independent functions $f(x)$, $\phi(x)$, he obtained for the several resulting functions expressions involving products of differences between the roots of the one and the other equation, $f(x) = 0$, $\phi(x) = 0$; the question then arose, what is the meaning of these functions? The answer is given by his theory of *intercalations:* they are signaletic functions, indicating in what manner (when the real roots of the two equations are arranged in order of magnitude) the roots of the one equation are intercalated among those of the other. The investigations in regard to Newton's rule (not previously demonstrated) are very important and valuable: the principle of Sturm's demonstration is applied to this wholly different question: viz. x is made to vary continuously, and the consequent gain or loss of changes of sign is inquired into The third question is that of the determination of the character of the roots of a quintic equation by means of invariants. In connection with it we have the noteworthy idea of *facultative* points; viz, treating as the coordinates of a point in n-dimensional space those functions of the coefficients which serve as criteria for the reality of the roots, a point is facultative or non-facultative according as there is, or is not, corresponding thereto any equation with real coefficients: the determination of the characters of the roots depends (and, it would seem, depends only) on the bounding surface or surfaces of the facultative regions, and on a surface depending on the discriminant. Relating to these theories there are two elaborate memoirs, "On the Syzygetic Relations &c.," and "Algebraical Researches &c.," in the Philosophical Transactions for the years 1853 and 1864 respectively; but as regards Newton's rule later papers must also be consulted.

In the years 1851–54, we have various papers on homogeneous functions, the calculus of forms, &c. (*Camb. and Dub. Math. Journal*, vols. vi. to ix.), and the separate work "On Canonical Forms" (London, 1851). These contain crowds of ideas, embodied in the new words, *cogredient*, *contragredient*, *concomitant*, *covariant*, *contravariant*, *invariant*, *emanant*, *combinant*, *commutant*, *canonical form*, *plexus*, &c., ranging over and vastly extending the then so-called theories of linear transformations and hyperdeterminants. In particular, we have the introduction into the theory of the very important idea of *continuous* or *infinitesimal* variation: say that a function, which (whatever are the values of the parameters on which it depends) is invariant for an infinitesimal change of the parameters, is absolutely invariant.

There is, in 1844, in the *Philosophical Magazine*, a valuable paper, "Elementary Researches in the Analysis of Combinatorial Aggregation," and the titles of two other papers, 1865 and 1866, may be mentioned: "Astronomical Prolusions; commencing with the instantaneous proof of Lambert's and Euler's theorems, and modulating through the construction of the orbit of a heavenly body from two heliocentric distances, the subtended chord, and the periodic time, and the focal theory of Cartesian ovals, into a discussion of motion in a circle and its relation to planetary motion"; and the sequel thereto, "Note on the periodic changes of orbit under certain circumstances of a particle acted upon by a central force, and on vectorial coordinates, &c., together with a new theory of the analogues of the Cartesian ovals in space."

Many of the later papers are published in the *American Mathematical Journal*, founded, in 1878, under the auspices of the Johns Hopkins University, and for the first six volumes of which Sylvester was editor-in-chief. We have, in vol. i., a somewhat speculative paper entitled "An application of the new atomic theory to the graphical representation of the invariants and covariants of binary quantics," followed by appendices and notes relating to various special points of the theory; and in

the same and subsequent volumes various memoirs on binary and ternary quantics, including papers (by himself, with the aid of Franklin) containing tables of the numerical generating functions for binary quantics of the first ten orders, and for simultaneous binary quantics of the first four orders, &c. The memoir (vols. ii. and iii.) on "Ternary cubic-form equations" is connected with some early papers relating to the theory of numbers. We have in it the theory of residuation on a cubic curve, and the beautiful chain-rule of rational derivation; viz. from an arbitrary point 1 on the curve it is possible to derive the singly infinite series of points ($1, 2, 4, 5, \ldots 3p \pm 1$) such that the chord through any two points, m, n, again meets the curve in a point $m+n$, $m \sim n$ (whichever number is not divisible by 3) of the series; moreover, the coordinates of any point m are rational and integral functions of the degree m^2 of those of the point 1.

There is in vol. v. the memoir, "A Constructive Theory of Partitions arranged in three acts, an Interact in two parts, and an Exodion," and in vol. vi. we have "Lectures on the Principles of Universal Algebra" (referring to a course of lectures on multinomial quantity, in the year 1881). The memoir is incomplete, but the general theories of nullity and vacuity, and of the corpus formed by two independent matrices of the same order, are sketched out; and there are in the *Comptes rendus* of the French Academy later papers containing developments of various points of the theory,—the conception of "nivellators" may be referred to.

The last-mentioned paper in the *American Mathematical Journal* was published subsequently to Sylvester's return to England on his appointment as Savilian Professor of Mathematics at Oxford. In December 1886, he gave there a public lecture containing an outline of his new theory of reciprocants (reported in NATURE, January 7, 1887), and the lectures since delivered are published under the title, "Lectures on the Theory of Reciprocants" (reported by J. Hammond), same *Journal* vols. viii. to x.; thirty-three lectures actually delivered, entire or in abstract, in the course of three terms, to a class in the University, with a concluding so-called lecture 34, which is due to Hammond. The subject, as is well known, is that of the functions of a dependent variable, y, and its differential coefficients, $y', y'', \ldots$, in regard to x (or, rather, the functions of $y', y'', \ldots$), which remain unaltered by the interchange of the variables x and y: this is a less stringent condition than that imposed by Halphen ("Thèse," 1878) on his differential invariants, and the theory is accordingly a more extensive one. A passage may be quoted:—"One is surprised to reflect on the change which is come over Algebra in the last quarter of a century. It is now possible to enlarge to an almost unlimited extent on any branch of it. These thirty lectures, embracing only a fragment of the theory of reciprocants, might be compared to an unfinished epic in thirty cantos. Does it not seem as if Algebra had attained to the dignity of a fine art, in which the workman has a free hand to develop his conceptions, as in a musical theme or a subject for painting? Formerly, it consisted in detached theorems, but nowadays it has reached a point in which every properly-developed algebraical composition, like a skilful landscape, is expected to suggest the notion of an infinite distance lying beyond the limits of the canvas." And, indeed, the theory has already spread itself out far and wide, not only in these lectures by its founder, but in various papers by auditors of them, and others,—Elliott, Hammond, Leudesdorf, Rogers, Macmahon, Berry, Forsyth.

Sylvester's latest important investigations relate to the Hamiltonian numbers: there is a memoir, *Crelle*, t. c. (1887), and, by Sylvester and Hammond jointly, two memoirs in the Philosophical Transactions. The subject is that of the series of numbers 2, 3, 5, 11, 47, 923, calculated thus far by Sir W. R. Hamilton in his well-known Report to the British Association, on Jerrard's method. A formula for the independent calculation of any term of the series was obtained by Sylvester, but the remarkable law by means of a generating function was discovered by Hammond, viz. $E_0, E_1, E_2, \ldots$, being the series $3, 4, 6, \ldots$ of the foregoing numbers, each increased by unity; then these are calculated by the formula $(1-t)^{E_0} + t(1-t)^{E_1} + t^2(1-t)^{E_2} + \ldots = 1 - 2t$, equating the powers of t on the two sides respectively: observe the paradox, $t = \frac{1}{2}$, then the formula gives $0 =$ sum of a series of positive powers of $\frac{1}{2}$.

Enough has been said to call to mind some of Sylvester's achievements in mathematical science. Nothing further has been attempted in the foregoing very imperfect sketch.

A. CAYLEY.

THE CREMATION OF THE DEAD.

The Cremation of the Dead. By Hugo Erichsen, M.D. (Detroit: D. O. Haynes and Co., 1887.)

THIS book is an appeal to the general public on the propriety of introducing the practice of cremation, universally, into civilized communities; or, as the author puts it, "it is a plea for the burning of the dead." He considers, and we are inclined to think he is right, that the period of fanatic and fierce opposition to cremation has passed, and has made way for a calm consideration of the subject. In 1874, he tells us, a Persian gentleman then resident in one of the Eastern States of the free and great Republic of America, who wanted to have his wife cremated, was compelled by an ignorant mob to resort to interment; but now the feeling has changed.

In our own country the same sensible desire to discuss the question of cremation, fully and freely, is fairly established at the present time; and so greatly has prejudice disappeared, that now the act of cremation has been carried out over fifty times at the Woking Cemetery alone. As Sir Spencer Wells shows, in an introductory chapter which he has written for the work before us, the obstacle of law in England against cremation has been removed, and relatives may resort to the cremation of their dead without any unreasonable impediments.

In saying so much in favour of freedom in regard to cremation, we must, however, in this country confine the freedom to the voice of the living. The wishes of the dead, though they may have been delivered up to the last moment in favour of cremation, and may even have been ordered in the will of the deceased, have no legal weight with the survivors. The writer of this article was called, quite recently, to see a lady who had rather suddenly died,

to determine that life was actually extinct. She had been haunted with the most terrible fears that she might be buried alive, fears much intensified by the existence in her family of a tradition that one of her relatives had actually been subjected to this awful ordeal. She had directed in her will that she was to be cremated, but her legal adviser, who had himself drawn out the document, discovered that it had no force in regard to the direction of cremation, and two of the nearest relatives having a determined and conscientious objection to the process, the body had to be interred. In this instance every precaution was taken that the body was absolutely dead, and even decomposed, before it was laid in the earth, and to this extent the wishes of the deceased were fulfilled; but the fact that the law does not respond to the wishes of the dead is a point to be remembered by all who would be cremated. The same failure of law seems to be operative in Italy, for we all remember that the final request of the great Garibaldi as to the disposal of his body by fire remains to the present moment disregarded.

Dr. Erichsen must at once receive the credit of having written the best book that has issued from the press on the subject of cremation. It is short and yet full, concise and yet complete. There are eight chapters: the first, a history of cremation; the second, the evils of burial, and the sanitary aspects of incineration; the third, cremation in times of war; the fourth, the processes of modern cremation; the fifth, the medico-legal aspect of incineration, and the objections to cremation; the sixth, burial alive, cremation from an æsthetic and religious point of view; the seventh, the present state of the cremation question.

The introductory letter by Sir Spencer Wells, to which reference has already been made, is an excellent prelude to the chapters above recorded. Sir Spencer Wells has for many years been a staunch and consistent advocate of cremation, and has put it on record that, when the time comes—may it be long delayed!—his body is to be destroyed by fire. He writes, therefore, with authority, as one who has well considered the subject in all its details, and has learned the best and most forcible answers to the many objections that have from time to time been raised against cremation. He quotes the late Dr. Parkes's statement "that neither affection nor religion can be outraged by any manner of disposal of the dead which is done with proper solemnity and respect to the earthly dwelling-places of our friends. The question should be placed entirely on sanitary grounds. Burying in the earth appears certainly to be the most insanitary plan." On the religious side of the question, Wells also adds a strong sentence from the late Lord Shaftesbury, who remarked to him that, if cremation were wrong, "what has become of the blessed martyrs who were burned at the stake in ancient and modern persecutions?"

We turn naturally, as scientific readers, to the section of Dr. Erichsen's work which treats on the sanitary aspect of the subject. This is not, in our view, the strongest part. In it the author has collated the widely reported instances of the spread of epidemics on the opening of burial-places where persons who died of contagious diseases, similar to maladies which have broken out, have been interred. But here three fallacies are suggested. In the first place, it is impossible to accept all the illustrations as illustrations strictly in point and entirely trustworthy—or, for the matter of that, any of the evidence as absolutely trustworthy—seeing that other causes which might have been at work to produce the effects named are not duly eliminated. In the second place, if the instances cited may be accepted as *primâ facie* evidence, they accord imperfectly with other instances, not of exceptional, but of every-day life, in which cemeteries and graveyards holding the remains of those who have died of contagious maladies have been partly or largely opened without any manifestation of the dangers referred to; or in which persons have lived for long series of years in close proximity to graveyards and cemeteries receiving the dead from infectious diseases, and yet have not suffered from those diseases more than others in other localities. There is at the present moment a cemetery near London, from which at times, emanations of the worst kind proceed, indicating that the cemetery is overcharged with dead, and ought at once to be closed; but no epidemic has broken out from it as a centre of contagion. In the third place, the evidence collected by the author, if it were accepted as mainly trustworthy, is not quite *ad rem.* It would be correct in so far as old burial-grounds and old modes of burial are concerned, but it would have no bearing whatever on the earth-to-earth system of burial which our countryman, Mr. Seymour Haden, has done so much to introduce and to perfect.

Dr. Erichsen's answer to these objections would be: Why linger at all over the bodies of the dead? they feel not, neither do they know. "It is of no consequence to the dead whether they rot in earth and originate miasmata, or are transformed by fire into pure white ashes. They feel as little of the process of decay as they do of the flame: their eye is surrounded by the same darkness, whether down in the deep grave or in the glowing light of the crematory furnace. But it is of greatest consequence to us, the living; and the only way to protect ourselves from poisonous infection by our dead is to burn them."

In this one sentence lies, in a scientific sense, the gist of the whole question. If it were true and demonstrable that the only way by which the living can be preserved from the dead is to burn the dead, every true man of science would support the principle of cremation out and out, and the practice would become universal in a very short space of time. Moreover, as Science, like Nature herself, has no morbid sentiments, but goes straight to and for the truth, she would not tarry long in making herself heard. It is just because the voice of Science cannot be so absolute that it demurs or hesitates. Her scholars inquire amongst the living of the day to see if they afford an answer to the important question. They ask: Are the persons whose duty it is to be nearest to the dead immediately after death—the upholsterers and the servants of the cemeteries and graveyards—more liable than others to the infectious diseases from their special occupations? and the answer which comes back is certainly negative. They ask other and similar questions:—How many times has it been known that a medical man in conducting the autopsy of a person who has died of a contagious affection has contracted the disease? How many women of the death-chamber have contracted disease from the dead? These questions also

receive a negative answer; and as a matter of course the man of science is, therefore, unable to be dogmatic or to strain a necessity: he cannot clearly recognize, on a "not proven" verdict, the duty of wounding the extremely sensitive feelings of millions of his fellow-men, on a subject that is amongst the most tender of all that pertains to humanity.

Our idea is that in the current state of public opinion, and in the current state of scientific knowledge, it is best to let the public feeling towards cremation work its own way, and to let earth-to-earth burial also have its free course.

Cremation will come partly by necessity, partly by a gradual sentiment in its favour. To force it by conjuring up dangers which do not exist is the very means of arresting it in its progress. We do not say that the work we have had under review is open to too severe criticism on these grounds; on the contrary, it adduces such a number of sound arguments in support of its case, and, on the whole, shows such a just and good weight on its own side, that we commend it as an excellent treatise—we should not improperly say standard treatise—on cremation.

ASSAYING.

Practical Metallurgy and Assaying: a Text-book for the use of Teachers, Students, and Assayers. By Arthur H. Hiorns. Pp. 471, with 91 Illustrations, Appendix, and Index. (London: Macmillan and Co., 1888.)

ASSAYING was a term originally used to denote the estimation, by the agency of heat, of a particular metal in an ore, alloy, or other metallic compound. Since the publication of Agricola's work in 1556, numerous English translations of foreign treatises on the subject have been published. Amongst these may be mentioned the translations of the works of Erker (1629), Barba (1674), and Cramer (1774). Assaying by the dry way has changed so little that the methods and instruments described in these old books might still be successfully used. Since the introduction, however, of the rapid and accurate wet processes, improvements have quickly followed each other, and from a particular ore a larger yield is now obtained than was formerly the case, so that the dry methods are, with a few exceptions, rapidly falling into disuse, as in many cases they do not indicate with sufficient precision the amount of metal actually present in the ore. The modern English literature of assaying is confined to Mitchell's large treatise, and to the chapters given in Percy's works and in Phillips's "Elements of Metallurgy." No small text-book, in which full cognizance is taken of wet processes, has hitherto been published, and a gap in our metallurgical literature has now been well filled by Mr. Hiorns's useful book, which is based on the course of instruction organized at the Royal School of Mines by Prof. W. C. Roberts-Austen, to whom the author, as an old pupil, dedicates his work. In all the Continental Schools of Mines, the instruction is conducted in a most unsatisfactory manner. Large classes rapidly pass through the various assaying processes, all the students working together with military precision at the Professor's word of command. In London, on the other hand, each student works independently, and is not permitted to pass from one metal to another until he can prove that he is able to constantly produce trustworthy results. As a student of the Royal School of Mines, Mr. Hiorns has thus had an excellent training for the task he has undertaken. Besides this, as Principal of the School of Metallurgy at the Birmingham and Midland Institute, he has had ample opportunity of ascertaining the wants of the average student.

Like so many of the text-books of science now published, Mr. Hiorns's book has been arranged to meet the requirements of the Science and Art Department Syllabus. The first part contains a number of experiments for the student to perform in order to elucidate the principles upon which metallurgy is based; the second part contains an account of the methods of assaying by dry methods; whilst the third deals with assaying by wet methods, and includes volumetric analysis and the analysis of furnace gases. The course is very systematically arranged, and it is certain that any student who has performed the experiments enumerated would be thoroughly well grounded in practical metallurgy. And the fact that such a book is now required by a large number of students in evening classes shows what excellent service the Science and Art Department is doing for practical metallurgy throughout the country.

The author discusses several of the newer methods, such as Turner's method of estimating carbon in iron, and alludes to recent researches, such as those of Beringer on the accuracy of the volumetric estimation of copper. He appears, however, to be unacquainted with the newer methods in use on the Continent, and it is to be regretted that he has not consulted the standard works of Balling and of Bruno Kerl, or the careful abstracts of foreign papers published in the Journals of the Chemical Society and of the Iron and Steel Institute. It is to be regretted, too, that there is a want of uniformity in the weights and measures adopted. Grains and grammes, ounces and cubic centimetres, are used indiscriminately. For industrial purposes, it was perhaps necessary that the "grains" should be retained. But, with regard to the "ounces," many assayers, who are familiar with metric measures, have no idea how many ounces make a pint. In the nomenclature and notation, there is also an unfortunate want of uniformity, as is shown by the indiscriminate use of the terms, oil of vitriol and sulphuric acid, carbonate of soda and sodium carbonate, hæmatite and hematite, OH_2 and H_2O, SO_4H_2 and H_2SO_4. The book is remarkably free from typographical errors. The name Fresenius is, however, spelt wrong in places (pp. 174, 183) and "oxide of silica" (p. 312) is a compound unknown to the chemist.

On the whole, the work is an excellent one, and will, no doubt, prove of great service to the teachers and students of classes in practical metallurgy. Chemists generally, accustomed to ordinary laboratory manipulation, will be interested to see how many operations there are, which, while differing from those with which they deal, are capable of affording very trustworthy results. The illustrations are of a very effective character, and are well executed from drawings that have been prepared with an amount of care not usual in figures of this class. Mr. Hiorns's literary style is far from faultless, but his instructions are always perfectly clear, and, to use the words of an old metallurgist, "he writes like one who hath black'd his Fingers and sing'd his Beard in metallick Operations."

B. H. B.

THE ORCHIDS OF THE CAPE PENINSULA.

The Orchids of the Cape Peninsula. By Harry Bolus, F.L.S. With Thirty-six Plates, partly coloured. Off-print from the Transactions of the South African Philosophical Society, 1888, Vol. V., Part 1. (Cape Town, 1888.)

"THIS," as the author informs us, "is an attempt to describe the Orchids growing on the peninsula of the Cape of Good Hope; to give their names and synonyms; to arrange them as far as possible in groups; to adduce the stations where they have been found, and their further distribution so far as known. To this is added a list of collectors; and of books and papers already published upon the subject of South African Orchidology." Mr. Bolus's name as an authority on Cape Orchids is already well known through his papers in the Journal of the Linnean Society; and the thoroughness with which his work has been done is vouched for by the fact that it has occupied a great part of his leisure time for several years, and embodies the results of a comparison of the Orchids of Thunberg's Herbarium, by Mr. N. E. Brown, A.L.S.; also by the fact that Lindley's Type Herbarium, and the General Herbarium, at Kew, where Mr. Bolus has been staying for several months, have been fully consulted.

The Cape peninsula is a tract of land about forty miles long, varying in width from about three to eleven miles, and has a total area of 197½ square miles; and it is interesting to note that in an area about one-fourth larger than the Isle of Wight, no less than 102 species, belonging to ten genera are found, thirty-three of which, so far as at present known, are endemic. The order is considered to take a position the fourth in importance in the flora (after Compositæ, Leguminosæ, and Ericaceæ), and to constitute 5·8 of the whole. The altitudinal range of the species is very interesting. The greater part of the area in question is occupied by a central mountain range, of which Table Mountain, which attains an elevation of 3562 feet, is the highest part. From Mr. Bolus's tables, it appears that fifty-nine species never descend into the plains to a lower elevation than 500 feet, twenty others are always found below this elevation, while the twenty-three remaining ones are indifferent in this respect. He also remarks that fifteen species have a vertical range of from 2000 to 3000 feet, and six species a range of more than 3000 feet. This large vertical range, which is shared in common with many of the flowering plants, Mr. Bolus attributes to the equability of the temperature, and of the moisture of the atmosphere at different elevations, owing to the close proximity of the sea on nearly every side.

One of the species is of such great beauty that there has been some danger of its ultimate extinction; on which points the following will be read with interest. "The peerless *Disa uniflora* is in its glory on the rivulets of Table Mountain in February. . . . This beautiful flower is the object of universal admiration, and the name which has been given to it, the 'Pride of Table Mountain,' indicates the honour in which it is held. It is, indeed, the queen of terrestrial Orchids in the southern hemisphere, as *Cypripedium spectabile* may be said to reign, though with less magnificence, in the northern. . . . It is still abundant on Table Mountain, although of late years large quantities of the tubers have been annually exported to Europe, and much needless destruction, arising from wasteful gathering by unskilled hands, resulted. But the summit of the mountains being Crown land, the Government has recently intervened, and restricted the removal of tubers within reasonable limits, so that, if this supervision be continued, there will be little reason to fear the extinction of this truly noble species."

The thirty-six, partly coloured, plates, which represent the rarer or least-known species, are drawn by the author, and the dissections and botanical details are admirably portrayed; though in some cases the outline only is given, and a little shading would have enhanced their effect.

Respecting the structure and homologies of Orchideæ, largely cited from Darwin, and with a plan of the flower from the same source, it may be pointed out that the so-called union of the two lateral stamens of the outer staminal whorl with the median petal, to form the lip, was disputed, and, I think, satisfactorily disproved, by Crueger; a view which has been since confirmed by Dr. Masters, in the case of *Cypripedium*. The papers in question appear to have been overlooked, but the oversight detracts little from the value of this admirably executed work. To those who wish to procure copies, the omission of the publisher's name is unfortunate. Messrs. West, Newman, and Co., of Hatton Garden, E.C., are the printers, and may be able to supply the work.

R. A. ROLFE.

OUR BOOK SHELF.

Carl von Linné's ungdomsskrifter. Samlade af Ewald Ährling, och efter hans död med statsunderstöd utgifvna, af K. Vetenskaps-Akademien. Första serien, första häftet. (Stockholm: P. A. Norstedt & Söner, 1888.)

THIS is the first part of the youthful writings of Linnæus, collected by the late Dr. Ewald Ährling, and published, under a State grant, by the Royal Swedish Academy of Science. The work as a whole is to be divided into two series of several parts each, the first series including a record of the life of Linnæus up to the year 1734, with botanical addenda. The second will contain the author's account of his journeys in Lapland (1732), hitherto printed only in English; in Dalabergslagen (1733); in Dalecarlia (1734); and notes on his sojourn abroad (1735). In the first series are the following purely botanical works:—"Hortus Uplandicus," after Tournefort's system, from the original in the possession of the Linnean Society; "Hortus Uplandicus," after Tournefort's system, with an addendum, and a new division of Umbellatæ (1730), from the original in the Leufsta Library; "Hortus Uplandicus," after the author's method the sexual system (1731), original in possession of the Rev. J. Johansson, at Ifvetofta; and "Adonis Uplandicus," after the sexual system (1731), original in the Leufsta Library.

The majority of readers will find that the most interesting of the papers in the part before us is the great botanist's diary. The original of this, wholly in the handwriting of Linnæus, is one of the two autographs in the possession of the Linnean Society. It contains thirteen closely-written pages, and we must conclude, from a remark in the diary, that it was written between 1730 and 1735. To judge from difference of writing and ink, additions were made at a later date.

The diary is followed by "Catalogus Plantarum

Rariorum Scaniæ item Catalogus Plantarum Rariorum Smolandiæ" (1728), in the possession of the De Geer family (Leufsta Library); and by "Spolia Botanica" (1729), the original of which is in the possession of the Linnean Society, and is considered to have been finished towards the end of 1729. This seems, however, improbable, the date of dedication (to Prof. Roberg, one of Linnæus's teachers at Upsala) being May 5, 1729. The work is accompanied by twelve facsimile drawings of the principal representatives of the Lapland flora.

This part of the first series contains copious and explanatory notes by the late Dr. Ährling, a work which must have entailed very great labour. After his death, his editorial duties were undertaken by Dr. M. B. Swederus. The second series will be edited by Prof. G. Lindström.

First Principles of Physiography. By John Douglas. (London: Chapman and Hall, 1889.)

THE ever-increasing number of text-books on this subject is evidence that the study of physiography is gaining in popularity. The object of the book before us, as the author states in his "Prologue on the Beach," is to give a systematic statement of the nature of the forces at work in the world, and of the changes which the matter of the world undergoes. The book is obviously designed to cover the syllabus issued by the authorities at South Kensington, although no mention of this fact is made.

The first part of the book deals with force, but for some reason or other, force is not defined until p. 26, and there only in an obscure place. The author's notion of treating elementary chemical ideas is somewhat peculiar; to make statements about positive and negative elements without explaining the meanings of those terms, and to use formulæ like NH_3 and H_2SO_4 (p. 36) without naming the compounds they represent, is scarcely the way to inspire a student with confidence in his teacher.

No less than 23 pages are devoted to tables, all of more or less interest to students of physiography.

Perhaps the chief novelty of the book is the introduction of copious quotations from, and references to, standard works. Their introduction as footnotes, however, is rather objectionable, as it tends to discontinuity. A good deal of information is undoubtedly given, but the style is not such as to commend it to those who are just commencing the study of science, and these, it must be remembered, constitute the majority of those who take up the subject of physiography.

LETTERS TO THE EDITOR.

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"Engineers" *versus* "Professors and College Men."

PROF. GREENHILL is, himself, one of many proofs that the distinction between "Engineers" and "Professors and College Men" is a Cross Division. Every "Engineer" ought to be a "highly-trained College man." If he were, he would know at once, from the very first sentence of the Principia (*Quantitas materiæ est mensura ejusdem* &c., &c.) that mass is the personal property of a body, one of the invariable things in nature:—and not an accidental property dependent, for its amount and even for its very existence, on the momentary surroundings. The letter M has hitherto been used by Newtonians in this sense. If anyone has since attached to it another and different sense, *he* is responsible for the consequent confusion. Would it not be well if Prof. Greenhill, and the School to which he has attached himself, would kindly leave to Newtonians their M, as defined for them by their Master; and (with severely logical consistency) turn it upside down (thus, W) when they wish to embody their own revolutionary definition? No Newtonian will refuse to recognize $Wv^2/2g$ as a correct expression for so much energy:—though he will probably think it both clumsy and complex, and will prefer to write as usual his $Mv^2/2$.

I am curious to know how Prof. Greenhill would deal with physical Astronomy. What is his measure of the earth's mass? According to the analogy of his "*units of* g *pounds*" the earth's mass is at present (near perihelion) to be spoken of as if it were some 6 or 7 per cent. greater than it was six months ago!

The whole of this attempt to improve on Newton is caused by unwillingness to face, once for all, the small amount of labour and thought requisite for learning or teaching how to pass from one system of units to another. A properly taught student learns, very early in his career, that this is no awful and mysterious process:—in fact that it is, throughout, quite as simple in principle as is the passing from miles per hour to feet per second.

And I venture to assert that such a student would attack with ease and confidence any *fair* question (*i.e.* one free from mere tricks or traps) connected with the subject. This one, for intance:—

"How many of the following quantities (taken in order) can, by selection of the requisite system of units, be simultaneously expressed by one and the same *number*. *First*, when that number is given? *second*, when it is not?

(*a*) The weight of a ton, at sea-level, at the equator.

(*b*) The speed of light *in vacuo*.

(*c*) The average kinetic energy of a particle of hydrogen at 0° C.

(*d*) The minimum compressibility of water at low pressures.

(*e*) The mean angular velocity of the earth about the sun.

Express the requisite units in C.G.S. measure, when the common numerical value, above mentioned, is $\log_e\pi$; and also when it is not assigned."

Of course it is understood, and this is my answer to Prof. Greenhill's first question, that *the student would be furnished with all the necessary data*, experimental or otherwise, expressed in definite *assigned* units.

In answer to Prof. Greenhill's second question I need only say that it is no part of my case to assert that all statements, made by "College men," are *necessarily* characterized by definiteness, by accuracy, or even by common-sense.

December 21, 1888. P. G. TAIT.

The Sun-spot Cycle.

IT may interest some of the readers of NATURE to learn that an expected change has just been observed upon the solar surface.

It is a well-established fact that in each new series of sun-spots the first spots of the cycle are seen in high solar latitudes, and that as the number of spots increases there is a common drift towards the sun's equator, the spot area becoming most extensive as the sixteenth parallel of heliographic latitude is reached. During late years the spots have been diminishing in number and size, and approaching the solar equator; and in the past twelve months very few spots have been seen on the sun's surface, and all in low latitudes, that recorded on December 21 being 4° south of the solar equator. The close of the year has, however, witnessed a change, as a small spot is recorded on the Stonyhurst drawing of December 30 at 36° south latitude. Spots near the equator will probably continue to be observed for some time, but, whilst they are diminishing, those in higher latitudes will be on the increase. S. J. PERRY.

Stonyhurst Observatory, Lancashire, December 31, 1888.

"Renaissance of British Mineralogy."

MR. FLETCHER'S admirable address on a "Renaissance of British Mineralogy," of which a report was published in a recent issue of your paper, calls timely attention to the present condition of the science. Mineralogy as a popular study seems dead: the chemists have deserted it for a study of complex organic compounds, so that it has become a mere hanger-on of geology. The science is now not thoroughly taught in any institution in this country, and teachers therefore have no means of acquiring knowledge, in the only really useful way, by working under the acknowledged masters. This is especially the case with

crystallography, which must at present, unfortunately, be regarded as a branch of mineralogy. And if they cannot acquire sound knowledge, how can they teach well? I feel quite sure that if the Science and Art Department would institute a summer course for teachers, where they would not have to waste their time over the merest rudiments, but could study practically the methods of crystal measurements, and the higher branches of mineralogical research, it would be largely taken advantage of by teachers and those who intend to become teachers. Failing this, I for one should be glad to know of any institution in this country or in Germany where such instruction could be obtained during the summer months. A TEACHER.

Glasgow, December 31, 1888.

Ventilating Bees.

I DO not know whether it is generally known that here, and I believe in other tropical countries, there are in every hive what one can only describe as "ventilating bees." I mean that during the hot season two or three bees post themselves, on their heads, at the entrance of the hive, and fan the interior with the incessant motion of their wings. They are relieved at intervals by fresh bees, who carry on the process. They are kept to their duty by a sort of patrol of bees to insure their incessant activity. This is a well-authenticated and known fact, and as such may interest your readers. EVA M. A. BEWSHER.

Mauritius, November 21.

Sonorous Sand at Botany Bay.

REFERRING to the correspondence with regard to the so-called "musical sands," which has appeared in your columns, it may be of interest to record the fact that sand with similar properties is found in Botany Bay, New South Wales, not far from the spot where Captain Cook first landed. When displaced by pressure from above, or disturbed with the hand or stick, this sand emits a musical sound, which appears to vary in intensity according to the degree of moisture which it contains. Should any of your correspondents wish for specimens, I shall be happy to forward a small quantity to them. A. SIDNEY OLLIFF.

Australian Museum, Sydney, November 16, 1888.

HOW RAIN IS FORMED.[1]

IN certain villages in the Indian Central Provinces, besides the village blacksmith, the village accountant, the village watchman, and the like, there is an official termed the *gápogári*, whose duty it is to make rain. So long as the seasons are good and the rain comes in due season, his office is no doubt a pleasant and lucrative one. It is not very laborious, and it is obviously the interest of all to keep him in good humour. But if, as sometimes happens, the hot dry weather of April and May is prolonged through June and July, and week after week the *ryot* sees his young sprouting crops withering beneath the pitiless hot winds, public feeling is wont to be roused against the peccant rain-maker, and he is led forth and periodically beaten until he mends his ways and brings down the much-needed showers.

You will hardly expect me, and I certainly cannot pretend, to impart to you the trade-secrets of the professional rain-maker. Like some other branches of occult knowledge which Madam Blavatsky assures us are indigenous to India, this art of rain-making is perhaps not to be acquired by those who have been trained in European ideas; but we can at least watch and interrogate Nature, and learn something of her method of achieving the same end; and if her scale of operations is too large for our successful imitation, we shall find that not only is there much in it that may well challenge our interest, but it may enable us to some extent to exercise prevision of its results.

Stated in the most general terms, Nature's process of rain-making is extremely simple. We have its analogue in the working of the common still. First, we have steam or water vapour produced by heating and evaporating the water in the boiler; then the transfer of this vapour to a cooler; and finally we have it condensed by cooling, and reconverted into water. Heat is communicated to the water to convert it into vapour, and when that heat is withdrawn from it, the vapour returns to its original liquid state. Nature performs exactly the same process.

In the still, the water is heated until it boils; but this is not essential, for evaporation may take place at all temperatures, even from ice. A common little piece of apparatus, often to be seen in the window of the philosophical instrument maker, and known as Wollaston's cryophorus, is a still that works without any fire. It consists of a large glass tube with a bulb at each end, one of which is partly filled with water: and, all the air having been driven out of the tube by boiling the water, it is hermetically sealed and allowed to cool. It then contains nothing but water and water vapour, the greater part of which re-condenses when it cools. Now, when thus cold, if the empty bulb be surrounded by ice, or, better, a mixture of ice and salt, the water slowly distils over, and is condensed in the colder bulb, and this without any heat being applied to that which originally contained the water. And this shows us that all that is necessary to distillation is that the condenser be kept cooler than the evaporater.

Nevertheless, at whatever temperature it evaporates, water requires heat, and a large quantity of heat, merely to convert it into vapour; and this is the case with the cryophorus; for if the evaporating bulb be wrapped round with flannel, and so protected from sources of heat around, the water cools down until it freezes. That is to say, it gives up its own heat to form vapour. A simple experiment that anyone may try with a common thermometer affords another illustration of the same fact. If a thermometer bulb be covered with a piece of muslin, and dipped into water that has been standing long enough to have the same temperature as the air, it gives the same reading in the water as in the air. But if when thus wetted it be lifted out and exposed to the air, it begins to sink at once, owing to the evaporation of the water from the wet surface, and it sinks the lower the faster it dries. In India, when a hot wind is blowing, the wet bulb sometimes sinks 40° below the temperature of the air.

Now this is a very important fact in connection with the formation of rain, because it is owing to the fact that water vapour has absorbed a large quantity of heat—which is not sensible as heat, but must be taken away from it before it can be condensed and return to the liquid state—that vapour can be transported as such by the winds for thousands of miles, to be condensed as rain at some distant part of the earth's surface.

I have said that the quantity of absorbed heat is very large. It varies with the temperature of the water that is evaporating, and is the greater the lower that temperature. From water that is on the point of freezing it is such that one grain of water absorbs in evaporating as much heat as would raise nearly 5½ grains from the freezing to the boiling point. This is called the latent heat of water vapour. As I have said, it is quite insensible. The vapour is no warmer than the water that produced it, and this enormous quantity of heat has been employed simply in pulling the molecules of water asunder and setting them free in the form of vapour, which is merely water in the state of gas. All liquids absorb latent heat when they evaporate, but no other known liquid requires so much as water.

Many things familiar in everyone's experience find their explanation in this absorption of latent heat. For instance, we feel colder with a wet skin than with a dry one, and wet clothes are a fruitful source of chills when the body is in repose; although, so long as it is in active exercise and producing a large amount of heat, since the evaporation

[1] A Lecture delivered by H. F. Blanford, F.R.S., at the Hythe School of Musketry on November 19.

only carries off the excess, no ill consequence may ensue. Again, if a kettle be filled with ice-cold water and put on a gas stove, suppose it takes ten minutes to bring it to boil. In that ten minutes the water has absorbed as much heat as raises it from 32° to 212°, an increase of 180°. Now, if it be left boiling, the gas-flame being kept up at the same intensity, we may assume that in every succeeding ten minutes the same quantity of heat is being absorbed by the water. But it gets no hotter: it gradually boils away. And it takes nearly an hour, or more than five times as long as it took to heat it, before the whole of the water has boiled away, since all this heat has been used up in converting it into steam. It was by an experiment of this kind that Dr. Black, in the last century, discovered the fact of latent heat, and determined its amount; and it was the knowledge of this fact that led James Watt to his first great improvement in the steam-engine.

One more example I may give, which those who have been in India will be able to appreciate, and which those who intend to go there may some day find useful to know. Nothing is more grateful in hot dry weather than a drink of cold water. Now, ice is not always to be had, but when a hot wind is blowing, nothing is easier than to get cold water, if you have a pot or bottle of unglazed earthenware, such as are to be had in every bazaar, or, what is better, a leather water-bottle, called a *chhàgal*, or a water-skin. All these allow the water to soak through and keep the outside wet; and if any one of them be filled with water and hung up in a hot wind, in the course of half an hour or an hour, the evaporation from the outside will have taken away so much heat that the contents may be cooled 20° or 30°, notwithstanding that the thermometer may stand at 110° or 115° in the shade. Soda-water may be cooled in the same way if wrapped in straw and kept well wetted while exposed to the wind. But it is of little use to do as I have seen natives do sometimes, viz. put the bottles into a tub of water in a closed room. It is the evaporation that carries off the heat, otherwise the water is no cooler than the air around.

Now to return to our subject. The atmosphere always contains some vapour which the winds have taken up from the ocean, lakes, rivers, and even from the land, for there are but few regions so dry and devoid of vegetation that there is no moisture to evaporate. The quantity of water thus evaporated from large water surfaces is a question of some importance to engineers, who have to take account of the loss from reservoirs and irrigation tanks, and a good deal of attention has been given to measure the amount lost by evaporation. In England it has been found to vary in different years from 17 to 27 inches in the year, or say from $1\frac{1}{2}$ to $2\frac{1}{4}$ inches per month on an average. Now, since in the east of England the rainfall is only about 24 inches in the year, it follows that in that part of the Kingdom the loss by evaporation from a water surface is not very much less than the rain falling directly on the surface.

In dry countries the evaporation may exceed the local rainfall. In the tropics it has been found to average from $3\frac{1}{2}$ to 6 inches per month in the dry season. In the case of a large tank at Nagpur, constructed to supply the city with water, it was found that the loss by evaporation, in the hottest and driest weather, was two and a half times as great as the quantity supplied for consumption.

These statistics will give some idea of the enormous evaporation that goes on from the water surfaces of the globe, and to this must be added all that takes place from the land. In the case of light showers, nearly the whole of the rain is re-evaporated; and probably, on an average, half of the total rainfall on the land is thus lost sooner or later, leaving not more than half for the supply of springs and rivers.

The quantity of vapour in the air is very variable. To us, in England, the west and south-west winds are the dampest, coming direct from the Atlantic, and north-east winds are the driest. The cause of their extreme dryn es I shall endeavour to explain presently. It is no doub partly due to the fact that they reach us from the land surface of Europe, but partly also to another cause to which I shall have to advert later on.

The quantity of vapour in the air is usually ascertained by the hygrometer, the ordinary form of which is a pair of thermometers, one having the bulb wet, the other dry, and observing the depression of the wet bulb. The principle of this I have already explained. But the same thing may be ascertained more directly by passing a measured quantity of air through a light apparatus containing sulphuric acid, or some other substance that absorbs water vapour greedily, and weighing the whole before and afterwards. The increase of the second weighment gives the weight of water absorbed. By such means it has been ascertained that air at 60° can contain as much as $5\frac{3}{4}$ grains of vapour in each cubic foot, and that air at 80° can contain rather less than 11 grains in the same space. The quantity that air can hold increases therefore very rapidly with the temperature. But it is seldom that it contains this maximum amount, especially at the higher temperatures.

In order to condense any part of this vapour we must take away its latent heat. It is not sufficient merely to cool it till it reaches the temperature of condensation, but we have further to abstract $5\frac{1}{2}$ times as much heat as would raise the condensed water from the freezing to the boiling point. Before, however, proceeding to consider how this cooling is effected, the question arises, What is the condensing point? For, obviously, since water can evaporate at all temperatures, so we should expect that it may condense at all temperatures. On what, then, does the condensing point depend?

I mentioned just now that air at the temperature of 60° can contain as much as $5\frac{3}{4}$ grains of vapour, and at 80° rather less than 11 grains in each cubic foot. Obviously, then, if air at 80°, containing this maximum quantity, be cooled to 60°, it must get rid of more than 5 grains, or nearly half its vapour, and this excess must be condensed. I speak of air containing these quantities, but in point of fact it makes no appreciable difference whether air be present or not. An exhausted glass vessel of one cubic foot capacity can hold $5\frac{3}{4}$ grains of vapour at 60° and no more, and nearly 11 grains at 80° and no more; and if, when thus charged at 80°, its contents be cooled to 60°, more than 5 grains will be condensed. If, however, it contain only $5\frac{3}{4}$ grains at 80°, none will condense until the temperature falls to 60°, but any further cooling produces some condensation. Thus, then, the condensing point depends on the quantity of vapour present in the air, and is the temperature at which this quantity is the maximum possible for that temperature.

This preliminary point being explained, we may now proceed to inquire what means Nature employs to condense the vapour in the air, producing at one time dew and hoar-frost, at another time fog and cloud, and at another rain, hail, and snow.

Let us take the case of dew and hoar-frost first, as they are comparatively simple. And in connection therewith I may relate a little incident that took place at Calcutta some years ago. A gentleman, who had not much acquaintance with physical science, was sitting one evening with a glass of iced brandy and water before him. It was in the rainy season, when the air, though warm, is very damp, and he had a large lump of ice in his tumbler. On taking it up, he noticed to his surprise that the glass was wet on the outside, and was standing in quite a little pool of water on the table. At first he thought his tumbler was cracked, but putting his finger to his tongue he found the fluid tasteless. "Very odd!" he remarked; "the water comes through the glass but the brandy doesn't."

Now, however with our present knowledge we may be inclined to smile at the simplicity of this remark, it so

happens that up to the end of the last century very much the same explanation was popularly held to account for dew. It was supposed to be a kind of perspiration emitted from the earth, and no satisfactory explanation of the phenomenon had been arrived at by the physical philosophers of the day. It remained for Dr. Wells to prove, by a long series of observations and experiments, which have been quoted by Sir John Herschel and Mr. John Stewart Mill as a typical instance of philosophical inquiry, that the cold surface of grass and shrubs condenses the vapour previously held in suspension in the air, these surfaces being cooler than the air, and below its point of condensation. And such, of course, is also the case of the glass tumbler containing ice. Anyone may try the experiment for himself. To produce hoar-frost, it is only necessary to cool the condensing surface below the freezing point, which may be done by crushing some ice and mixing it with salt. A tin pot is better than a glass to make this experiment.

When not only the ground, but also the air to a considerable height above it, is cooled in like manner, we have the production of fog, fog being the form in which the vapour is first condensed, and consisting of water in drops too minute to be separately visible. The formation of fog is very much aided if the air be laden with smoke. Smoke consists of extremely minute particles of unburnt coal or other fuel, and these cool faster than the air at night, and so cool the air in contact with them. Each one of them, too, condenses water on its surface, and being thus weighted they sink and form that dense fog that Londoners know so well.

Clouds are essentially the same as fog, but formed high up in the air. But in their case, and that of rain, snow, and hail, another and different cooling agency comes into play, and this will require some preliminary explanation.

I dare say that some of you may at some time or other have charged an air-gun. And if so, you will be aware that when so charged the reservoir becomes pretty warm. Now this heat is produced, not, as might be supposed, by the friction of the piston in charging, but is due to the fact that work has been done upon the air by compressing it into a very small space; in other words, work has been converted into heat. If the compressed air be allowed to escape at once, its heat is re-converted into work. It has to make room for itself by thrusting aside the atmosphere into which it escapes, and when thus expanded it is no warmer than before it was compressed. Indeed, not so warm, for it will already have parted with some of its heat to the metal chamber which contained it. And if when compressed it is allowed to cool down to the ordinary temperature, and then to escape, it will be cooled below that temperature just as much as it was heated by compression. Thus, if in being compressed it had been heated 100°, say from 60° to 160°, and then allowed to cool to 60°, on escaping it will be cooled 100° below 60°, or to 40° below zero, which is the temperature at which mercury freezes. This is the principle of the cold air chambers now so extensively employed on ship-board for the transport of frozen provisions from New Zealand and Australia.

Bearing in mind, then, this fact—that air in expanding and driving aside the air into which it expands is always cooled—let us see how this applies to the case before us, the production of cloud and rain.

The volume of a given weight of air—in other words, the space it occupies—depends on the pressure to which it is subject: the less this pressure the greater its volume. If we suppose the atmosphere divided into a number of layers superimposed on each other, the bottom layer is clearly subject to the pressure of all those that rest on it. This is equal to about $14\frac{3}{4}$ pounds on every square inch of surface. Another layer, say 1000 feet above the ground, will clearly be under a less pressure, since 1000 feet of air are below it; and this 1000 feet of air weighs slightly less than half a pound for every square inch of horizontal surface. At 2000 feet the pressure will be less by nearly one pound per square inch, and so on. If, then, any mass of air begins to ascend through the atmosphere, it will be continually subject to less and less pressure as it ascends; and therefore, as we have already seen, it expands, and becomes cooler by expansion. Cooling from this cause is termed dynamic cooling. Its rate may be accurately computed from the work it has to do in expanding.

It amounts to 1° for every 183 feet of ascent if the air be dry or free from vapour, and if, as is always the case, it contains some vapour, the height will not be very much greater so long as there is no condensation. But so soon as this point is passed, and the vapour begins to condense as cloud, the latent heat set free retards the cooling, and the height through which this cloud-laden air must ascend to cool 1° is considerably greater, and varies with the temperature and pressure. When the barometer stands at 30 inches, and at the temperature of freezing, the air must rise 277 feet to lose 1°, and if the temperature is 60° nearly 400 feet.

Conversely, dry air descending through the atmosphere and becoming denser as it descends, since it is continually becoming subject to an increased pressure, is heated 1° for every 183 feet of descent; and fog and cloud-laden air at 30 inches of pressure and the freezing point will be warmed 1° in 277 feet only, or if at 60° nearly 400 feet of descent, owing to the re-evaporation of the fog or cloud and the absorption of latent heat.

Now let us see how these facts explain the formation of cloud; and first I will take the case of the common cumulus or heap-cloud, which is the commonest cloud of the day-time in fine weather.

When after sunrise the air begins to be warmed, the lowest stratum of the atmosphere, which rests immediately on the ground, is warmed more rapidly than the higher strata. This is because the greater part of the sun's heat passes freely through a clear atmosphere without warming it, and is absorbed by the ground, which gives it out again to the air immediately in contact with it. So soon as the vertical decrease of temperature exceeds 1° in 183 feet, the warm air below begins to ascend, and the cooler air above to descend, and this interchange gradually extends higher and higher, the ascending air being gradually cooled by expansion, and ceasing to rise when it has fallen to the same temperature as the air around it. This ascending air is more highly charged with vapour than that which descends to replace it, since, as was mentioned before, most land surfaces furnish a large amount of moisture, which evaporates when they are heated by the sun. This process goes on until some portion of the ascending air has become cooled to the point of condensation. No sooner does it attain this, than a small tuft of cumulus cloud appears on the top of the ascending current, and the movement which was invisible before now becomes visible. In a calm atmosphere each tuft of cloud has a flat base, which marks the height at which condensation begins, but it is really only the top of an ascending column of air. No sooner is this cloud formed than the ascent becomes more rapid, because the cooling which checked its further ascent now takes place at a much slower rate, and therefore the cloud grows rapidly.

On a summer afternoon when the air is warm and very damp, such cumulus cloud ascends sometimes to very great heights, and develops into a thunder-cloud, condensing into rain. Rain differs from fog and cloud only in the size of the water drops. In fog and cloud these are so minute that they remain suspended in the air. But as the cloud becomes denser, a number of them coalesce to form a rain-drop, which is large enough to overcome the friction of the air. It then begins to fall, and having to traverse an enormous thickness of cloud below, it grows larger and larger by taking up more and more of the

cloud corpuscules, so that when finally it falls below the cloud it may have a considerable size.

Such, then, is the mode in which rain is formed in an ordinary summer shower; and the more prolonged rainfall of stormy wet weather is the result of a similar process, viz. the ascent and dynamic cooling of the moist atmosphere. But in this case the movement is on a far larger scale, being shared by the whole mass of the atmosphere, it may be, over hundreds or thousands of square miles; and to understand this movement we shall have to travel somewhat further afield, and to inquire into the general circulation of the great atmospheric currents set in movement by the sun's action in the tropics, and modified by the earth's diurnal rotation and the distribution of the continents and oceans on its surface.

Before, however, entering on this subject, which will require some preliminary explanation, and in which we shall have to take account both of ascending and descending currents on a large scale, I will draw your attention to another and simpler case, in which both these classes of movements are prominently illustrated, and in which they exhibit their characteristic features in a very striking manner.

In the valleys of the Alps, more especially those to the north of the central chain, in Switzerland and the Tyrol, there blows from time to time a strong warm dry wind, known as the Föhn. It blows down the valleys from the central chain, melting the snows on its northern face, and although there is more or less clear sky overhead, all the southern slopes of the mountains are thickly clouded, and heavy rain falls on the lower spurs and the adjacent plain, replaced by snow at the higher levels up to the passes and the crest of the range. Cloudy weather also prevails to the north in Germany, and the weather is stormy over some part of Western Europe.

It is only since the general introduction of telegraphic weather reports and the construction of daily weather charts have enabled us to take a general survey of the simultaneous movements of the atmosphere over the greater portion of Europe, that this Föhn wind has been satisfactorily explained.[1] It is found that when a Föhn wind blows on the north of the Alps, the barometer is low somewhere to the north or north-west, in Germany, Northern France, or the British Isles, and high to the south-east, in the direction of Greece and the Eastern Mediterranean. Under these circumstances, since the winds always blow from a place of high barometer to one of low barometer, a strong southerly wind blows across the Alps. On their southern face it is forced to ascend, and therefore, as just explained, it is cooled and gives rain in Lombardy and Venetia, and snow at higher elevations. But having reached the crest of the mountains, it descends to the northern valleys, and being by this time deprived of a large part of its vapour, it becomes warmed in its descent, owing to compression. absorbs and re-evaporates the cloud carried with it, and is then further warmed at the rate of 1° for every 183 feet of descent. Thus it reaches the lower levels as a warm dry wind, its warmth being the effect of dynamic heating.

Other mountain chains afford examples of the same phenomenon. A very striking instance, which much impressed me at the time, is one that I witnessed many years ago in the mountains of Ceylon; and it was afterwards mentioned to me by Sir Samuel Baker, who had been equally struck by it. My own experience is as follows:—In June 1861, I paid a week's visit to the hill sanitarium of Newara Eliya, at an elevation of 6200 feet, on the western face of Pedro Talle Galle, the highest mountain in the island. The south-west monsoon was blowing steadily on this face of the range; and during the whole time of my stay it rained, as far as I am aware, without an hour's intermission, and a dense canopy of cloud enveloped the hill face, and never lifted more than a few hundred feet above the little valley in which Newara Eliya is built. But on leaving the station by the eastern road that leads across the crest of the range to Badulla, at a distance of five miles one reaches the *col* or dip in the ridge near Hackgalle, and thence the road descends some 2000 feet to a lower table-land which stretches away many miles to the east. No sooner is this point passed than all rain ceases and cloud disappears, and one looks down on the rolling grassy hills bathed in the sunshine of a tropical sun, and swept by the dry westerly wind that descends from the mountain ridge. In little more than a mile one passes from day-long and week-long cloud and rain to constant sunshine and a cloudless sky.

As an almost invariable rule, or at least one with few exceptions, ascending air currents are those that form cloud and rain, and descending currents are dry and bring fine weather. And this holds good whatever may be the immediate cause of these movements. We may now proceed to consider these greater examples to which I have already referred.

In the great workshop of Nature, in so far at least as concerns our earth, with but few exceptions, all movement and all change, even the movements and energies of living things, proceed either directly or indirectly from the action of the sun. Nowhere is this action more direct and more strikingly manifested than in the movements of the atmosphere. Were the sun extinguished, and to become, as perhaps it may become long ages hence, a solid cold sphere, such as Byron imagined, "wandering darkling in eternal space," a few days would suffice to convert our mobile and ever-varying atmosphere into a stagnant pall, devoid of vapour, resting quiescent on a lifeless earth, held bound in a more than Arctic frost. From such a consummation, despite the supposed decaying energy of our sun, we may, however, entertain a reasonable hope that we are yet far distant.

Bearing in mind the all-embracing importance of the sun, let us see how the great movements of the atmosphere are determined by the way in which the earth presents its surface to the solar rays.

Since the quantity of solar heat received on each part of the earth's surface depends on the directness or obliquity of his rays—in other words, on the height to which the sun ascends in the heavens at noon—being greatest where he is directly overhead, as in summer in the tropics, it follows that the hottest zone of the earth is that in the immediate neighbourhood of the equator, and the coldest those around the poles.

Did time allow, and were the necessary appliances at hand, it would be easy to show you that both as a matter of experiment, and also as a deduction from physical laws, there must be under such circumstances a flow of air from the colder to the warmer region in the lower atmosphere, and a return current above. And to a certain extent we have these constant winds prevailing for about 30° on either side of the equator, in the trade-winds, which blow towards the equator in the lower atmosphere, and the anti-trades blowing in the opposite direction at a great height above the earth's surface.

In the neighbourhood of the equator there is a zone extending right round the earth in which the barometer is lower than either to the north or the south. It is due to the greater heat of the sun, and it is towards this that the trade winds blow. It shifts to some extent with the seasons, being more northerly in the summer of the northern hemisphere, and more southerly in that of the southern hemisphere; and its average position is rather to the north of the equator, owing to the fact that there is more land in the northern than in the southern hemisphere, and that land is more heated by the sun than the ocean.

This simple wind system of the trades and anti-trades does not extend right round the earth, nor beyond 30° or

[1] The explanation was originally given by Prof. J. Hann, of Vienna.

40° of latitude in either hemisphere. Were the earth's surface uniformly land or uniformly water, there probably would be a system of trade-winds all round the globe, blowing from both hemispheres towards the equator; but even in that case they would not extend much, if at all, beyond their present limits. In the first place, every great mass of land sets up an independent system of air currents, since the land is hotter than the ocean in the summer, and colder in the winter. In the summer, therefore, there is a tendency to an indraught of air from the sea to the land in the lower atmosphere, and an outflow above, and in the winter the opposite; and this tendency modifies or interrupts the system of the trades and anti-trades. We have this tendency shown most distinctly in the monsoons of South-Eastern Asia, where, both in the India and China seas, a south-west wind in the summer takes the place which in the absence of the Asiatic continent would be held by a north-east trade-wind. And it is only in the winter that a north-east wind blows, and this is then termed the north-east monsoon.

In the second place, as I have said, the system of trade-winds could not in any case extend far beyond their present limits in latitude, owing to the fact that the earth is a sphere and not a cylinder. Let us fix our attention for a moment on the anti-trades—the upper winds which blow from the equator towards the poles. The equator, from which they start, is a circle about 24,900 miles in circumference; the poles are mere points, and, therefore, the whole of the air that blows towards the poles must turn back in any case before it reaches the pole, and must begin to turn back before it has gone very far on its journey. And, as a fact, a great part of it does turn back between 30° and 40° of latitude, which I have already mentioned as being the limit of the trade-winds. A part of the remainder descends to the earth's surface, and sweeps the Northern Atlantic and the North Pacific as a south-west wind.

On the chart which represents the average distribution of atmospheric pressure in January, there are two somewhat interrupted zones of high pressure over the ocean in these latitudes. These mark the regions in which the anti-trades descend to the earth's surface, and from which the trade-winds start. Over the ocean in all higher latitudes, both in the northern and southern hemispheres, the barometer is low—for the most part, indeed, much lower than over the equator; and the region intervening between the zones of high pressure and the seat of lowest pressure is that of predominant south-west, or at all events westerly, winds. Since our islands are situated on the border of this region of low pressure, south-west are our prevailing winds.

But now two questions arise: first, Why are these winds westerly, and not simply south winds? and second, How is it that the barometer is so low over the North Atlantic and North Pacific Oceans, and also in the southern hemisphere in high latitudes, seeing that in these latitudes, at least in winter, the sun's heat is so much less than at the tropics? The chart represents the state of things in midwinter of the northern hemisphere, and yet everywhere to the north of latitude 40° the deep blue tint indicates that the pressure is lower than even in the southern tropic, where the sun shines vertically overhead. Clearly this low pressure must be due to some other cause than the warmth of the air.

The explanation of this remarkable distribution of the atmospheric pressure, of the existence of two zones of high pressure in latitudes 30° to 40°, and of very low pressure in higher latitudes, except in so far as they are modified by the alternations of land and water, was first given by the American physicist, Prof. Ferrel. Its full demonstration is to be obtained only from the consideration of somewhat recondite mechanical laws, but a general idea of the causes operating may be gathered from very simple considerations, which may be demonstrated with a terrestrial globe.

Starting with the well known fact that the earth revolves on its axis once in the twenty-four hours, let us see what will be the consequence, if we suppose a mass of any ponderable matter—that is, any substance having weight, no matter whether light or heavy—to be suddenly transferred from the equator to latitude 60°.

As the circumference of the earth at the equator is about 24,900 miles, any body whatever, apparently at rest at the equator, is carried round the earth's axis at the rate of 1036 miles an hour. But in latitude 60°, where the distance from the axis is only half as great as at the equator, it is carried round at only half the same rate, or 518 miles an hour; and at the pole it simply turns round on its own axis. Supposing, then, a mass of air to be suddenly transferred from the equator to latitude 60°, with the eastward movement that it had at the equator, it would be moving twice as fast to the east as that part of the earth, and, to any person standing on the earth, would be blowing from the west with a force far exceeding that of a hurricane. It would be moving eastwards 518 miles an hour faster than the earth. Indeed, its movement would really be far greater than this. In virtue of a mechanical principle known as the law of the conservation of areas, which means that any body revolving round a central point, under the influence of a force that pulls it towards that point, describes equal areas in equal times, instead of only 518 miles, it would be revolving round the earth's axis 1554 miles an hour faster than that part of the earth. I need not, however, specially insist on this point, because, as a matter of fact, the air which constitutes the anti-trades is not suddenly transferred, but takes a day or two to perform its journey, and in the meantime by far the greater part of its eastward movement is lost by friction against the trade-wind which blows in the opposite direction underneath it. The point on which we have to fix our attention is that, when the anti-trades descend to earth, they still retain some of this eastward movement, and blow, not as south, but as south-west or west-south-west winds.

On the other hand, the trade-wind, which blows towards the equator, is coming from a latitude where the eastward movement is less than at the equator, and its own movement eastward is therefore less than that of the surface over which it blows. A person, therefore, standing on the earth, is carried eastward faster than the air is moving, and the wind seems to blow against him from the north-east. Similarly, to the south of the equator, the trade-wind, instead of blowing from the south, comes from the south-east.

Thus, then, we have in both hemispheres a system of westerly winds in all higher latitudes than 40°, and a system of easterly winds—viz. the trade-winds—between about 30° and the equator; and if the globe were either all land or all water, these systems would prevail right round the earth.

Now, it is the pressure of these winds, under the influence of centrifugal force, that causes the two zones of high barometer in latitudes 30° to 40°, and the very low pressure in higher latitudes. It is not difficult to understand how this comes about. You are probably aware that the earth is not an exact sphere, but what is termed an oblate spheroid—that is, it is slightly flattened at the poles and protuberant at the equator, the difference of the equatorial and polar diameters being about 26 miles. It has acquired this form in virtue of its rotation on its axis. If you whirl a stone in a sling, the stone has a tendency to fly off at a tangent, and, so long as it is retained in the sling, that tendency is resisted by the tension of the cord. In the same way, every object resting on

the earth, and the substance of the earth itself, has a tendency to fly off at a tangent, in consequence of its rotation on its axis, and this tendency is resisted and overcome by gravity. Were the earth not revolving, its form, under the influence of gravity alone, would be a true sphere. If it were to revolve more rapidly than at present, it would be still more oblate, flatter at the poles, and more bulging in the tropical zone; if less rapidly, the flattening and bulging would be less.

This is precisely what happens with the west and east winds of which we have spoken. West winds are revolving faster than the earth, and tend to make the atmosphere more protuberant at the equator than the solid earth; hence they press towards the equator, to the right of their path in the northern hemisphere, and this tendency increases rapidly in high latitudes. Easterly winds, on the other hand, tend to render the form of the atmosphere more nearly spherical, and they, too, press to the right of their path in the northern hemisphere or towards the pole. In the southern hemisphere, for the same reason, both press to the left. The result of these two pressures in opposite directions is to produce the two zones of high barometer in the latitudes in which we find them—viz. between the easterly trade-winds and the westerly winds, which are the anti-trades that have descended to the earth's surface. And the low barometer of higher latitudes is produced in like manner by the westerly winds pressing away from those regions.

Thus, then, we find that all this system of winds, and the resulting distribution of atmospheric pressure as indicated by the barometer, is the result of the sun's action in equatorial regions. It is this that gives the motive power to the whole system, so far as we have as yet traced it, and it is this that produces those great inequalities of atmospheric pressure that I have so far described.

It remains now to see how storms are generated by these westerly winds. In so far as they retain any southing, they are still moving towards the pole in the northern hemisphere—that is to say, they are advancing from all sides towards a mere point. Some portion of them must therefore be continually turning back as the circles of latitude become smaller and smaller. But they are now surface-winds, and in order so to return they must rise and flow back as an upper current. This they do by forming great eddies, or air-whirls, in the centre of which the barometer is very low, and over which the air ascends, and these great air-whirls are the storms of the temperate zone and of our latitudes. It is the ascent and dynamic cooling of the air in these great eddies that cause the prolonged rainfall of wet stormy weather. How the eddies originate, or, rather, what particular circumstance causes them to originate in one place rather than another, we can scarcely say, any more than we can say how each eddy originates in a rapidly-flowing deep river. Some very small inequality of pressure probably starts them, but, when once formed, they often last for many days, and travel some thousands of miles over the earth's surface.

Two such storms are represented on the charts of February 1 and 2, 1883, one on the coast of Labrador, the other to the south-west of the British Isles. The first of these appears on the chart of January 28, in the North Pacific, off the coast of British Columbia. On the 29th it had crossed the Rocky Mountains, and was traversing the western part of the Hudson's Bay Territory. On the 30th it had moved to the south-east, and lay just to the west of the Great Lakes, and on the 31st between Lake Superior and Hudson's Bay. On February 1 it had reached the position on the coast of Labrador shown in the chart, and on the 2nd had moved further to north-east, and lay across Davis's Straits, and over the west coast of Greenland. After this it again changed its course to south-east, and on February 4 passed to the north of Scotland, towards Denmark, and eventually on to Russia.

The second storm had originated off the east coast of the United States between January 28 and 29, and on the following days crossed the Atlantic on a course somewhat to north of east, till, on February 2, it lay over England.

These storms always move in some easterly direction, generally between east and north-east, and often several follow in rapid succession on nearly the same track. It is this knowledge that renders it possible for the Meteorological Office to issue the daily forecasts that we see in the newspapers. Were it possible to obtain telegraphic reports from a few stations out in the North Atlantic, these storm warnings could be issued with much more certainty, and perhaps longer before the arrival of the storm than at present. In the case of such storms as that which reached our islands on February 2, we often have such warnings from America, but their tracks are often more to the north-east, in the direction of Iceland, in which case they are not felt on our coasts, and hence the frequent failure of these American warnings.

It is the region of low pressure in the North Atlantic that is the especial field of these storms. As they pass across it, they produce considerable modifications in the distribution of pressure, but some of its main features remain outstanding. Thus there is always a belt of high barometer between the storm region and the trade-winds, and in the winter there is almost always a region of high barometer over North America, and another over Europe and Asia, however much they may shift their places, and be temporarily encroached on by the great storm eddies.

These regions of high pressure are the places where the winds descend, and, as I mentioned in the earlier part of this lecture, these winds are dry, and generally accompany fine weather. On the contrary, the eddies, where the air ascends, are damp and stormy, and especially that part of the eddy that is fed by the south-west winds that have swept the Atlantic since their descent, and so have become charged with vapour.

And now we are prepared to understand why east, and especially north-east winds are generally so dry. They are air that has descended in the area of high barometer that, especially in the winter and spring, lies over Europe and Asia, and has subsequently swept the cold land-surface, which does not furnish much vapour, and therefore they reach us as dry cold winds. To begin with, the air comes from a considerable height in the atmosphere, and in ascending to that height in some other part of the world, it must have got rid of most of its vapour in the way that has been already explained. In descending to the earth's level it must, of course, have been dynamically heated by the compression it has undergone, but all or nearly all this heat has been got rid of by radiation into free space on the cold plains and under the clear frosty skies of Northern Asia and Northern Europe, and it then blows outwards from this region of high barometer over the land, towards the warmer region of low barometer on the North Atlantic Ocean.

Thus we see that, in all cases, rain is produced by the cooling of the air, and that in nearly all, if not all, this cooling is produced by the expansion of the air in ascending from lower to higher levels in the atmosphere, by what is termed dynamic cooling. This last fact is not set forth so emphatically as it should be in some popular text-books on the subject, but it is an undoubted fact. It was originally suggested by Espy some forty years ago, but the truth is only now generally recognized, and it is one of the results which we owe to the great advance in physical science effected by Joule's discovery of the definite relation of equivalence between heat and mechanical work.

THE SOARING OF BIRDS.[1]

So much for sails. Now I want to make some suggestions, or suggest some queries, as to the *skimming* flight of birds, in reference to which a good deal of fresh observation has been possible during the voyage.

You perhaps recollect that when the British Association was at Glasgow, you asked me to put into writing, briefly, as a paper for your Section, some remarks on this subject which I had made to you in conversation, but that, owing to my hasty departure to attend the trial of H.M.S. *Shah*, I omitted to do this.

I had better briefly recite the above particulars here in order to make more clear the bearing of the new observations we (I and Tower) have made.

The view was that when a bird skims or soars on quiescent wings, without descending and without loss of speed, the action must depend on the circumstance that the bird had fallen in with, or selected a region where the air was ascending with a sufficient speed. In still air the bird, if at a sufficient height, could continue to travel with a steady speed, using his extended wings as a sort of descending inclined plane, the propelling force depending on the angle of the plane and on the equivalent of "slip,"—that is to say, on the excess of the angle of actual descent compared with the angle of the inclined plane. The steady speed would be attained when the weight of the bird and the sines of the angle of the plane = the bird's *air resistance*, including skin friction of wings—in fact one might say = simply the skin friction of the whole area, for the bird's lines are fine enough to justify this statement, since there is no wave-making to be done, and indeed experiment shows that the statement is true for "fish-formed" bodies moving wholly and deeply immersed in water. Of course the bird's angle of actual descent is greater than that of the quasi-inclined plane, owing to the equivalent of "slip" in the wings. Under these simultaneously acting and correlated conditions there is of course—or probably—some total angle of descent which enables the bird to minimize his rate of approach to the earth in still air. If when there is a wind the configuration of the ground or any other circumstances can produce a local ascent of air more rapid than the bird's minimum rate of descent when soaring in still air, he may continue to soar indefinitely by keeping in the region where the air is thus ascending.

Now, in most cases where one sees birds "soaring," it is easy to see that they have plainly selected such a region, and for a long time I felt confident that the only two even apparent exceptions I had encountered were such as to *prove* not to *invalidate* the rule. One of these exceptions was that once, when the sea in Torbay was in a state of glassy calm, I noticed a large gull thus soaring at some distance from the shore,—watching it with a pair of binoculars, so that I was sure of the quiescence of the wings. But here the riddle was at once solved by the observation of what I had not at first noticed—the dark trace of the front line of a fresh sea-breeze advancing all across the bay. Such an advance with a definitely marked front, encountering an extended body of quiescent air, involved of course an ascent of air in the region of the encounter, and this was where the bird was soaring. The other exception was that when at sea I had often noticed birds thus soaring near the ship. The solution was that, so far as I had then noticed, the birds always selected a region to leeward of the ship, where the eddies created by the rush of air past her hull, &c., might readily have created local ascending currents.

The new exceptions we have seen since we have approached the Cape entirely set these two solutions at defiance.

[1] Extract from a letter of the late William Froude to Sir W. Thomson, of February 5, 1878, received after Mr. Froude's death. Reprinted from the Proceedings of the Royal Society of Edinburgh, March 19, 1888.

The first exception we noticed was in the flight of some albatrosses. We were sailing, and steaming (at low speed, being short of coal), nearly due east in the latitude of the Cape, with the wind light and variable abaft the beam, and with a well-marked south-west swell of about 8 to 9 seconds period, and varying from 3 or 4 feet to 8 or 9 feet from hollow to crest. The speed of such waves would be from 24 to 27 knots.

Under these conditions the birds *seemed* to soar almost *ad libitum* both in direction and in speed; now starting aloft with scarcely, if any, apparent loss of speed; now skimming along close to the water, with the tip of one or other wing almost touching the surface for long distances, indeed now and then actually touching it. The birds were so large that the action could be clearly noted by the naked eye even at considerable distances; but we also watched them telescopically, and assured ourselves of the correctness of our observations. The action was the more remarkable owing to the lightness of the wind, which sometimes barely moved our sails, as we travelled only 5 knots before it, by help of the screw.

After long consideration the only explanation of at all a rational kind which presented itself was the following, which indeed presents the action of a *vera causa*, and one which was very often certainly in accordance with the birds' visible movements, though it was often also impossible either to assert or to deny the accordance; and anyhow the question arises, Is the *vera causa* sufficient? I will try to trace its measure

When a wave is say of 10 feet in height and say 10 seconds period (a case near enough to ours to form the basis of a quantitative illustration) the length of the wave from crest to crest is just 500 feet, the half of which space, or 250, the wave of course traverses in 5 seconds, and assuming the wave to be travelling in a calm, it must happen approximately that during the lapse of this 5 seconds the air which at the commencement of the interval lay in the lowest part of the trough has been lifted to the level of the crest, or must have risen 10 feet, so that its mean speed of ascent has been 2 feet per second (10 feet in 5 seconds). And since (as is well known) the maximum speed of an harmonic motion is $\frac{\pi}{2}$ times, or nearly $1\frac{1}{2}$ times its mean speed, it follows that along the side of the wave at its mid-height the air must approximately be ascending at the rate of 3 feet per second, and if the bird were so to steer its course and regulate its speed as to conserve this position he would have the advantage of a virtual upward air current having that speed.

NOTES.

The Berlin Academy of Sciences has presented 2000 marks (£100) to Prof. Leopold Auerbach (Breslau), and the same amount to Dr. Franz Schütt (Kiel), to aid them in their physiological researches. Dr. Freudenthal, Professor of Philosophy at Breslau, and Herr von Rebaur-Paschwitz, the astronomer, have received 1500 marks (£75) each.

At the last meeting of the Scientific Committee of the Royal Horticultural Society, Mr. Henslow called attention to the fact that the year 1889, besides being the centenary of the chrysanthemum in Europe, is that of the dahlia in England. It was introduced by the Marchioness of Bute in 1789, and figured with single and double forms in the *Botanical Magazine*, vol. xliv., t. 1885, and the *Botanical Register*, vol. i. t. 55.

The death of Mr. J. J. Coleman, F.R.S.E., is announced. He died at the age of fifty. For some time he was manager of the works of Young's Paraffin and Mineral Oil Company, Glasgow; and in this capacity he carried out some important experimental investigations for the utilization of so-called waste

products. Afterwards he invented the refrigerating machine which bears his name, a machine which has increased and cheapened the available supply of fresh meat.

On Tuesday, the 8th inst., the second of the series of one-man photographic exhibitions at the Camera Club will be open to visitors on presentation of card. The Exhibition will continue for about six weeks. The object of this series of Exhibitions is to bring together in turn representative collections of the work of the best photographic artists. By the co-operation of Mr. Harry Tolley, of Nottingham, the Camera Club is enabled to exhibit a representative set of his photographs. These pictures are large direct work, and are printed in the permanent platinum process.

A course of six lectures on "The Science of Brewing" will be given at the Finsbury Technical College by Dr. E. R. Moritz, commencing Wednesday, January 23, and being continued on successive Wednesday evenings. Other courses of special lectures will be given during the present term by Prof. Perry, on "The Differential Calculus and its Application to Problems of Electrical and Mechanical Engineering"; by Prof. S. P. Thompson, on "Optical Principles and Practice"; and a special laboratory course on "Electro-deposition" (plating and typing), by Mr. Rousseau.

According to the *Standard* of December 29, 1888, Hampshire was visited, at 11 o'clock on the morning of the 28th, with what was believed to be an earthquake. There were, it is reported, a severe subterranean rumbling and a concussion in the neighbourhood of Emsworth Common. A horse and cart passing at the time were visibly shaken, and two men were nearly knocked off their legs. There was a violent rustling of the trees in the neighbourhood, and the shock appeared to extend over a wide area.

On Sunday, December 23, a severe shock of earthquake was experienced in Calcutta and throughout Bengal. In Rajshahye large fissures opened, whence hot liquid mud was ejected. The Calcutta Correspondent of the *Times*, reporting these facts by telegraph on December 30, said no loss of life had been heard of.

Several severe shocks of earthquake were felt in Bosnia, on December 18, especially at Rogatica, Cajnica, Pleolje, and Poljancic.

The University of Edinburgh continues to attract a very large number of students. During the past year, according to the Edinburgh Correspondent of the *Times*, the total number of matriculated students was 3532, as against 3459 last year, 2667 in 1878, and 1564 in 1868. Of this total, 1008 were enrolled in the Faculty of Arts, 108 in the Faculty of Divinity, 474 in the Faculty of Law, and 1942 in the Faculty of Medicine. Of the students of medicine 832 (or 43 per cent.) belonged to Scotland, 705 (or fully 36 per cent.) were from England and Wales, 36 from Ireland, 79 from India, 247 (or nearly 13 per cent.) from British colonies, and 43 from foreign countries.

At the Bath meeting of the British Association, a Report was presented by the Committee which had been appointed to consider the advisability and possibility of establishing in other parts of the country observations upon the prevalence of earth tremors similar to those now being made in Durham. Considering that much is being done with the object of securing suitable forms of instruments, and that these investigations are still incomplete in many ways, the Committee felt that it would be premature for them to select and recommend any special recorder at present. They, however, emphasized the view that, whilst carefully finished, highly sensitive, and necessarily expensive seismoscopes, made to record with as much accuracy as possible the time, form, and intensity of each set of tremors, are very desirable, and indeed indispensable, yet only a comparatively small number of such instruments would be required in a general scheme of seismographical observatories. Such instruments, moreover, could only be used with effect in carefully selected situations, and otherwise under very special conditions. On the other hand, comparatively rough, cheap, and easily used instruments, which could do little more than afford fairly accurate time-records, would be required in large numbers, and must form a most important portion of such a scheme. The Committee hoped that at the next meeting of the Association they might be in a position to present a Report containing definite recommendations.

The Chief Signal Officer of the United States reports that in the reorganization of the Record Division of the Signal Office he found a large amount of valuable rainfall data, furnished by voluntary observers prior to 1874. With a view of making these records available, he has published them in an atlas of rain charts of the United States, for each month of the years 1870–74; these will be found especially useful in the study of weather conditions over that country. He also states that an examination of the records of the voluntary observers shows that it will be possible to further utilize them in the preparation of normal temperature charts, which he hopes to be able to issue with the Monthly Weather Review at an early date. An eighteen-year normal monthly rain-chart is now being regularly issued in this Review.

In December 1877, Prof. F. E. Nipher established a volunteer weather service in Missouri, the object being primarily to investigate the rainfall of that State, and he has now published the results in a paper entitled "Missouri Rainfall." It contains maps and tables showing the average monthly amounts for ten years ending 1887, at thirty-one stations, together with the maximum and minimum monthly and yearly falls. The yearly averages vary from 31·4 inches to 45·7 inches.

In a recent lecture on Bacteria, delivered at Brooklyn, Dr. George M. Sternberg pointed out that the rapid progress of bacteriology in Germany has been due, to a very considerable extent, to the enlightened policy of the Government. Dr. Sternberg is of opinion that if, during the past ten years, the Americans had had a well-equipped laboratory, under proper direction, the medical corps of the army and navy could easily have supplied men who would have done good work in this department of research. He thinks it is not creditable to the United States as a nation that Americans have contributed so little to the advance of bacteriology. "Let us hope, however," he added, "that we are entering upon a new era. Here in Brooklyn private munificence has provided the means of research which the National Government should have provided long since; and here, at least, the fault will rest with the profession, if active workers are not found to avail themselves of the facilities provided for making original researches in bacteriology, in physiology, and in experimental pathology." The "private munificence" referred to is that of Dr. Hoagland, who has equipped a laboratory at Brooklyn. This laboratory, which has been organized in accordance with the best models, is to be devoted exclusively to scientific research, and to instruction in physiology, pathology, histology, and bacteriology.

The tenth volume of the Proceedings of the United States National Museum has been issued. The series to which this volume belongs consists of papers prepared by the scientific corps of the National Museum; of papers by others, founded upon the collections in the National Museum; and of brief records of interesting facts from the correspondence of the Smithsonian Institution.

The new number—the sixth—of the *Internationales Archiv für Ethnographie* completes the first volume of this excellent

periodical. Among the plates is a coloured representation of the deerskin mantle, ornamented with shell work, recorded to have belonged to the Virginian Chief, Powhattan. Dr. E. B. Tylor contributes, in English, an account of this interesting object, which forms one of the treasures of the Ashmolean. It belongs to the Tradescant collection, which was the nucleus of the museum of Elias Ashmole. It seems that there were in use among the Powhattans three kinds of mantles, viz. of dressed skins embroidered with beads (including shells), of furs, and of feather work. In the original collection of Tradescant there were specimens of all three kinds. Of these, the shell-embroidered mantle alone remains. It measures about 2·2 m. in length by 1·6 m. in width. The two deerskins forming it are joined down the middle; no hair remains. The ornamental design consists of an upright human figure in the middle, divided by the seam; a pair of animals; thirty-two spirally formed rounds (two in the lowest line have lost their shells); and the remains of some work in the right lower corner. Dr. Tylor says that the decorative shell-work is of a kind well known in America. The shells used are *Marginella*: so far as Mr. Edgar A. Smith is able to identify them in their present weathered state, *M. nivosa*. Among the other contents of the number are some notes, in German, on the ethnography of Mexico, by Carl Breker, and an attempt, by M Messikommer, to describe some elements of what may have been the intellectual life of the inhabitants of ancient lake-dwellings.

In the new number of the Transactions of the Leicester Literary and Philosophical Society, there is a useful and interesting paper, by Mr. Montagu Browne, Curator of the Town Museum, Leicester, on "Evidences of the Antiquity of Man in Leicestershire." He begins with objects of bone and horn, then examines the remains of pottery, and finally deals with articles in bronze and stone. Palæolithic implements have not yet been discovered in Leicestershire, but Mr. Evans, in a passage quoted by Mr. Browne, is of opinion that they may be found there. "It is by no means impossible," writes Mr. Evans, "that you may succeed in finding them. It is a little far north, but I doubt whether the glaciers persisted so long in that part of England as they did in the Lake District, and in Wales."

Messrs. Cassell and Co. are issuing, in monthly parts, a popular edition of "The Story of the Heavens," by Sir Robert S. Ball, the Royal Astronomer of Ireland. The work, which is well printed on good paper, will be completed in eighteen parts. With Part I., which we have just received, a star map is given.

The "Educational Annual" for 1889, compiled by Edward Johnson, has been issued. Messrs. G. Philip and Son are the publishers. The work is designed to place within reach of the general public a concise summary of authentic information, drawn from official or other trustworthy sources, relative to primary and secondary education, in a form convenient for reference. Information relating to training colleges for teachers and teachers' associations has been included.

A book, entitled "Rides and Studies in the Canary Islands," by Mr. Charles Edwardes, has just been published (Fisher Unwin). The author quaintly explains that it is written "for the entertainment both of those who visit the Canary Islands and those who do not." A considerable part of the book has already appeared in the form of articles in magazines and journals. Those who have already read these fragments will not object to find them again among Mr Edwardes's lively and pleasant sketches.

We have received a diary called "The Perennial Diary," which a good many people may find useful. It is not intended to supersede ordinary diaries. Each page is devoted to a single day of the year, and events occurring on that day in different years may all be entered on the same page. The volume is issued by Mr. John Heywood, of Manchester and London.

The Free Libraries Committee of Manchester are able to give, in their thirty-sixth Annual Report, a most favourable account of all departments of the institutions under their charge. The number of the buildings in which the work of the Committee is carried on has been increased to ten by the establishment of the Hyde Road Reading Room, which was opened some time ago by the Mayor, in the presence of a large and enthusiastic meeting of the inhabitants of the district. In the course of the last twelve months the number of readers at the various libraries and reading-rooms (*i.e.* the number of visits they have made) reached an aggregate of nearly four millions and a half, or about a quarter of a million more than in the previous year. There has been an increase also in the number of books read. The number used for home reading and for perusal in the reading-rooms has been 1,606,874, against 1,462,028 volumes read in the preceding twelve months. The daily average of volumes used in all the Libraries was 4464.

In the letter on "Nose-blackening as preventive of Snow-blindness," by Mr. A. J. Duffield (vol. xxxviii. p. 172), *for* "New Zealand" *read* "New Ireland."

The additions to the Zoological Society's Gardens during the past week include a Common Kestrel (*Tinnunculus alaudarius*) captured at sea, presented by Mr. Thomas Austin; a Pyxis Tortoise (*Pyxis arachnoides*) from Durban, Natal, presented by Colonel J. H. Bowker, F.Z.S.; a Rat-tailed Snake (*Trigonocephalus lanceolatus*) from St. Lucia, W.I., presented by the West Indian (Natural History) Exploration Committee; two Concave-casqued Hornbills (*Buceros bicornis*) from India, deposited; a —— Squirrel (*Sciurus* sp. inc.) from Burmah, two Ceylonese Hanging Parrakeets (*Loriculus asiaticus*) from Ceylon, purchased.

OUR ASTRONOMICAL COLUMN.

Detection of New Nebulæ by Photography.—Prof. Pickering gives a brief account, in No. 6 of the Annals of Harvard College Observatory, vol. xviii., of some experiments he has recently conducted as to the advantages of a photographic doublet over an ordinary astronomical object-glass for astronomical work, and especially in photographing nebulæ. A number of plates were exposed upon the region of Orion, the instrument used being the Bache telescope, which has a photographic doublet with an aperture of 8 inches and a focal length of 44 inches; each plate covered a region 10° square, the definition being good within $3\frac{1}{2}°$ of the centre of the plate. The result of the experiments was the detection of twelve new nebulæ; fourteen nebulæ were seen on the photographs that were also given in Dreyer's Catalogue, and four nebulæ in the Catalogue were not represented on the plates. A similar proportion of discovery over the entire sky would mean some 4000 or 5000 new nebulæ, and 400 plates would be sufficient for a complete survey, provided here was no overlapping, and no plates proved defective.

Comets Faye and Barnard, October 30.—The following ephemeris for Faye's comet is in continuation of that given in Nature (vol. xxxix. p. 186). The ephemeris for Barnard's comet is by Herr Spitaler (*Astr. Nach.*, No. 2871). Both are for Berlin midnight:—

	Comet 1888 *d* (Faye).		Comet 1888 *f* (Barnard, Oct. 30).	
1888.	R.A.	Decl.	R.A.	Decl.
	h. m. s.	° ′	h. m. s.	° ′
Jan. 4	7 58 38	0 11·7 N.	10 25 49	4 42·3 N.
6	7 56 57	0 14·2	10 24 55	5 45·1
8	7 55 15	0 17·7	10 23 51	6 49·1
10	7 53 32	0 22·3	10 22 40	7 54·4
12	7 51 49	0 27·9	10 21 22	9 0·6
14	7 50 6	0 34·5	10 19 58	10 7·6
15	7 48 26	0 42·1 N.	10 18 24	11 15·2 N.

Both comets are slowly diminishing in brightness.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *JANUARY* 6–12.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 6

Sun rises, 8h. 7m.; souths, 12h. 6m. 17·2s.; sets, 16h. 6m.: right asc. on meridian, 19h. 11·3m.; decl. 22° 26′ S. Sidereal Time at Sunset, 23h. 12m.

Moon (at First Quarter January 9, 1h.) rises, 11h. 2m.; souths 16h. 22m.; sets, 21h. 53m.: right asc. on meridian, 23h. 27·9m.; decl. 8° 22′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	8 39 ...	12 30 ...	16 21 ...	19 35·5 ...	23 46 S.
Venus.....	10 7 ...	15 2 ...	19 57 ...	22 7·2 ...	13 15 S.
Mars......	10 0 ...	14 55 ...	19 50 ...	22 1·0 ...	13 16 S.
Jupiter....	6 32 ...	10 28 ...	14 24 ...	17 32·8 ...	22 56 S.
Saturn....	18 55*...	2 24 ...	9 53 ...	9 27·8 ...	16 6 N.
Uranus...	0 54 ...	6 17 ...	11 40 ...	13 21·6 ...	7 55 S.
Neptune..	13 2 ...	20 45 ...	4 28*...	3 52·0 ...	18 27 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Saturn, January 6.—Outer major axis of outer ring = 45″·1: outer minor axis of outer ring = 11″·0: southern surface visible.

Variable Stars.

Star.	R.A. (1889·0)	Decl. (1889·0)			
	h. m.	° ′		h. m.	
U Cephei	0 52·5 ...	81 17 N. ...	Jan.	8, 21 53	*m*
R Tauri	4 22·3 ...	9 55 N. ...	,,	9,	*M*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,,	7, 19 0	*m*
			,,	12, 19 0	*M*
R Canis Majoris...	7 14·5 ...	16 11 S. ...	,,	11, 18 10	*m*
			,,	12, 21 26	*m*
U Geminorum ...	7 48·5 ...	22 18 N. ...	,,	10,	*M*
X Boötis	14 18·9 ...	16 50 N. ...	,,	7,	*m*
U Boötis	14 49·2 ...	18 9 N. ...	,,	9,	*m*
δ Libræ	14 55·1 ...	8 5 S. ...	,,	12, 22 53	*m*
R Herculis	16 1·2 ...	18 40 N. ...	,,	10,	*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,,	11, 4 58	*m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,,	12, 6 0	*M*
R Aquilæ	19 0·0 ...	8 4 N. ...	,,	6,	*M*
T Vulpeculæ ...	20 46·8 ...	27 50 N. ...	,,	8, 20 0	*M*
			,,	12, 4 0	*m*
Y Cygni	20 47·6 ...	34 14 N. ...	,,	6, 5 40	*m*
		and at intervals of		36 0	
δ Cephei	22 25·0 ...	57 51 N. ...	Jan.	7, 2 0	*M*
			,,	10, 20 0	*m*
S Aquarii	22 51·3 ...	20 56 S. ...	,,	7,	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ξ Virginis	173° ...	9° N. ...	Swift; streaks. January 11.
,, ζ Boötis	218 ...	14 N. ...	Very swift; streaks.
,, β Boötis	222 ...	42 N. ...	,, ,,

NOTES ON METEORITES.[1]

VII.

Possible Connection between the Jets and Envelopes seen in Cometary Swarms.

THE jets observed in comets when near the sun are very various in form. The concentric envelopes seen at times are much more regular; an idea of their appearance will be gathered from the accompanying illustration of Donati's comet.

It has not yet been clearly ascertained whether the jets and envelopes are connected phenomena—that is, whether the jets are true whirls of the meteorites themselves—or whether they represent volatilization of the vapours of the nucleus in a particular direction, which vapours subsequently assume a concentric form. In Halley's comet, at all events, this was not

Fig. 21.—Concentric envelopes as illustrated by Donati's comet.

observed. Sir John Herschel writes concerning this: "The bright smoke of the jets, however, never seems to be able to get far out towards the sun, but always to be driven back and forced into the tail, as if by the action of a violent wind rolling against them—always from the sun—so as to make it clear that this tail is neither more nor less than the accumulation of this

Fig. 22.—Combination of jets and envelopes (comet of 1861).

sort of luminous vapour, darted off in the first instance towards the sun, as if something raised it up, as if it were exploded by the sun's heat, out of the kernel, and then immediately and forcibly turned back and repelled from the sun."

The Concentric and Excentric Envelopes.

While in Donati's comet we get perhaps the finest exhibition of concentric envelopes successively thrown off from the nucleus towards the sun, in Coggia's comet, on the other hand, we had the most striking instance which has been yet observed in which the envelopes put on an appearance as if they belonged to two different systems of concentric envelopes cutting each other.

It is important here to enter into some details. In Coggia's comet (as observed with Mr. Newall's 25-inch refractor, with a low power), next to the nucleus the most brilliant feature was an object resembling a fan opened out some 160°. The nucleus, marvellously small and definite, was situated a little to the left of the pin of the fan—not exactly, that is, at the point held in the hand. If this comet, outside the circular outline of the fan, offered indications of other similar concentric circular outlines, astronomers would have recognized in it a

[1] Continued from p. 142.

great similarity to Donati's comet with its "concentric envelopes." But it did not do so. Envelopes there undoubtedly were, but instead of being concentric they were excentric, and of an entirely unique arrangement.

To give an idea of the appearance presented by these excentric envelopes, still referring to the fan, let us imagine a circle to be struck from the left-hand corner with the right-hand corner as a centre, and make the arc a little longer than the arc of the fan. Do the same with the right-hand corner. Then with a gentle curve connect the end of each arc with a point in the arc of the fan half-way between the centre and the nearest corner. If these complicated operations have been properly performed, the reader will have superadded to the fan two ear-like things (as of an owl), one on each side. Such "ears," as we may for convenience call them, were to be observed in the comet, and they at times were but little dimmer than the fan. It will be observed that there is a central depression between the ears.

At first it looked as if these ears were the parts of the head furthest from the nucleus in advance along the comet's axis, but careful scrutiny revealed, still further forwards, a cloudy mass, the outer surface of which was convex, while the contour of the inner surface exactly fitted the outer outline of the ears and the intervening depression. This mass was at times so faint as to be almost invisible. But at other times it was brighter than all the other details of the comet which remain to be described, now that I have sketched the groundwork. Occasionally to be seen outside all was still another fainter mass, both the surfaces of which were convex outwards, the inner one having a greater radius. This exterior envelope or "umhullung" was the faintest part of the head.

In the root of the excessively complex tail were to be observed prolongations of all the curves to which I have referred. Thus, behind the brightest nucleus was a region of darkness which opened out 45° or 60°, the left-hand boundary of which was a continuation of the lower curve of the right ear. All the boundaries of the several different shells which showed themselves, not in the head in front of the fan, but in the root of the tail behind the nucleus, were continuous in this way—the boundary of an interior shell on one side of the axis bent over in the head

FIG. 23.—Rough outline sketch of head and envelopes of Coggia's comet as seen in Mr. Newall's 25-inch refractor on the night of July 12, 1874 (perihelion passage, August 27).

to form the boundary of an exterior shell on the other side of the axis.

I next draw attention to the kind of change observed. To speak in the most general terms, any great change in one "ear" was counterbalanced by a change of an opposite character in the other; so that, when one ear was thinned or elongated, the other widened; when one was dim, the other was bright; when one was more "pricked" than usual, the other at times appeared to lie more along the curve of the fan and to form part of it. Another kind of change was in the fan itself, especially in the regularity of its curved outline and in the manner in which the straight sides of it were obliterated altogether by light, as it were, streaming down into the tail.

There was nothing which in the slightest degree resembled the giving off of vapour.

The only constant feature in the comet was the exquisitely soft darkness of the region extending for some little distance behind the nucleus. Further behind, where the envelopes, the prolongation of which formed the tail, were less marked, the delicate veil which was over even the darkest portion became less delicate, and all the features were merged into a mere luminous haze. Here all structure, if it existed, was non-recognizable, in striking contrast with the region round and immediately behind the fan.

Next, it has to be borne in mind that the telescopic object is, after all, only a projection, from which the true figure has to be built up, and it is when this is attempted that the unique character of this comet becomes apparent. There were no jets, there were no concentric envelopes; but, in place of the latter, excentric envelopes indicated by the ears and their strange backward curvings, and possibly also by the fan itself.

It seems impossible that we can be here dealing with the mere volatilization of the materials of which the nucleus is composed; for, assuming that it is possible, as has hitherto been imagined, that shells of vapours can be thrown off to form concentric envelopes, and that the heads of comets like Donati's are thus built up, it is difficult at first to see how such appearances as here described could be thus produced.

On the Forces which produce the Various Forms and Parts of Comets.

Before we proceed further with any detailed description, it is necessary to inquire into the causes of the cometary phenomena with which we have so far become acquainted—namely, nucleus, jets, envelopes concentric and excentric, and tails.

We shall best do this by referring to the various memoirs with which Roche, of Montpellier, has enriched science. He dealt first with the atmospheres of planets; and, in concluding the third part of a memoir on the figure of a fluid mass subjected to the attraction of a distant point,[1] remarked that the inquiry might possibly apply to the theory of comets, if we suppose such an object, fluid and homogeneous, falling in a straight line towards the sun.

We have seen that a comet when it first makes it appearance at its greatest distance puts on a form resembling a planetary nebula. It is at this point that M. Roche closes with it in order to see what its change of form must be supposing it to be as above stated fluid and homogeneous.

As it approaches the sun, a tidal action will be set up, as the solar attraction will be greater on the particles nearest to it; hence there will be an elongation of the swarm, and possibly even one or more separations along a radius vector.

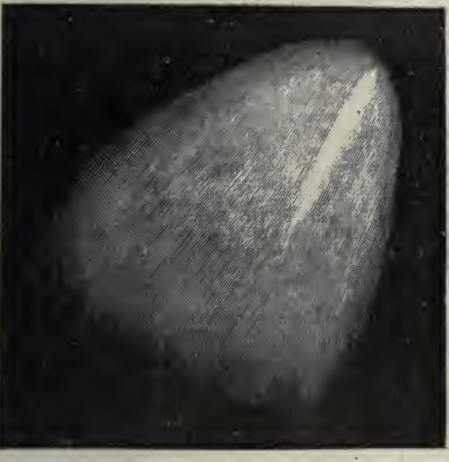


FIG. 24.—Elongation of a cometary swarm. Comet 1882 *b*, Washington equatorial.

If gravitation alone is concerned, the comet will remain symmetrical, it will reduce its size as it approaches the sun,[2] and part of its outer portions will be successively lost along the radius vector both towards and away from the sun; there, in fact, will be two outpouring streams—one directed towards the sun, the other away from it. There will be the greatest elongation and the greatest loss at perihelion.

M. Roche makes this out by considering the form of the envelope in which particles will be equally attracted by the sun and the general mass of the comet.

One chief point of the mathematical investigations was, in fact, to determine the surface on which the gravity of a small particle was *nil* in consequence of the solar and cometary attractions. This is called the limiting surface. On this point I quote from M. Faye:—[3]

"There exists, for every body placed within the sphere of action of our sun, a surface limit beyond which its matter may not pass, under pain of escaping to that body and falling within the domain of the solar action. This surface limit depends on two things—the mass of the body, and its distance from the sun. For a planet like the earth, whose mass is so considerable, this surface limit is very distant, and yet, within the still terrestrial region of its satellite, the moon, a child could lift, without much difficulty, a body which would weigh for us 36,000 kilogrammes, so feeble does the attraction of our globe become at that distance of 60 terrestrial radii. A little beyond the lunar orbit, a body would cease to belong to the earth, and would enter the exclusive domain of the sun. But for a comet this surface limit is much nearer the nucleus, and, moreover, it draws nearer and nearer in proportion as the comet approaches the sun. . . . The surface which so limits a body in the vicinity of the sun presents two singular points in the direction of the radius vector, setting out from which this surface is widened out into a conical network, in such a manner that the dissolution of a body the matter of which reaches or passes beyond these boundaries is effected principally in the vicinity of the points referred to, flying, so to speak, into two pieces, thus obeying at once the attraction of the comet and especially, the thenceforth preponderating attraction of the sun. . . .

FIG 25.—Showing how a comet approaching the sun, gravity alone being in question, loses its constituent particles beyond its free surface, which is constantly diminishing, by an outflow in both directions along the radius vector.

"All the conditions of instability are found united in comets. Their mass is extremely small, and, consequently, the surface limit is very near the centre of gravity. Their distance from the sun diminishes rapidly in the descending branch of their trajectory; consequently this surface limit becomes more and more contracted. Finally, their enormous volume tends unceasingly to dilate, because of the increasing heat of the sun, and to cause the cometary matter to shoot out beyond this surface limit.

"What becomes of this matter after it is set free by the action of the sun? Having escaped from that of the comet, it will none the less preserve the original speed, *i.e.* the speed which the comet itself had at the moment of separation; this speed will scarcely be altered by the feeble attraction of the cometary nucleus, or by the internal movements of which I have spoken, since these are measured by a few metres per second, while the general motion round the sun takes place at the rate of 10, 15, 20 leagues and more per second. The molecules, separated and thenceforward independent, then describe isolated orbits around the sun, differing very little from that of the comet. Those which are found in advance go a little faster and take the lead; those which are behind remain a little in the rear; so that the abandoned materials are divided along the trajectory of the comet in front and in rear of the nucleus In time these materials are separated considerably from the body from which they emanate, and are more and more disseminated; but, considered at the moment of emission, they will form two visible appendages, two sorts of tails opposed and stratified on the orbit of the comet."

So much for the state of things if gravitation alone is in question.

But is gravitation alone concerned in building up a comet's form? That this is not so was fully recognized long ago, and it was suggested by the fact that the tails always appeared to be driven away from the sun; Seneca, indeed, was possibly acquainted with this fact, as he wrote: "Comæ radios solis effugiunt."[1] Kepler was the first to suggest that the matter of the tails was transported to the regions opposite the sun by the impulsion of the solar rays; Euler and Laplace accepted this explanation; and Newton was the first to give a complete explanation of the curve of the tail.

Olbers, whose researches dealt with the phenomena presented by the comet of 1811, considered that the approach of a comet to the sun might develop electricity in one or the other of these bodies, and to this were ascribed both the repulsive action of the

[1] *Mémoires* of the Academy of Montpellier, vol. ii. p. 23.
[2] *Annales de l'Observatoire de Paris*, vol. v. p. 376.
[3] "Forms of Comets," NATURE, vol. x p. 247.

[1] See Pliny, Book II. chap. xxii. *et seq.*, for many references to more ancient authorities.

sun on the materials of the comet, and that of the comet on the nebulous atmosphere by which it was surrounded.

Olbers was driven to consider the repulsive action of the comet on its atmosphere in order to explain the many luminous sectors visible in the comet in question. To this he also ascribed the gradual rise of successive envelopes, so well illustrated subsequently by the comet of Donati.

The energy of electrical repulsion depends upon the amount of surface of the bodies concerned, whereas the attraction of gravity depends upon the masses of the bodies. Small things have more surface in proportion to their masses than large ones, and there will therefore be attraction or repulsion between the sun and the particles composing comets according as the differential effect of the two opposite forces is repulsive or attractive. In the very small particles, the electrical repulsion will be stronger than the attraction due to gravitation, while in the larger particles the two forces may balance each other, or gravitation may preponderate. Only the finest particles composing the head of a comet are therefore repelled to form the tails.

Bessel[1] considerably modified this hypothesis. He considered that the action of the sun on the comet represented a polar force.

M. Faye has more recently held that this repulsive action is due to the radiant energy of the sun, and that it has an intensity inversely as the square of the distance, and proportional to the surface and not to the mass of the moving particles. Its action would therefore be in the inverse ratio of the density of the particles upon which it acted; it would vary with every difference of cometary constitution; it would be inappreciable on the nucleus itself; (the idea being, of course, that the nucleus was a solid body); and it would be most effective in the case of the rarest vapours. The important part of M. Roche's later memoir consists in testing these views of repulsion, to determine whether the forms of comets could be explained by its introduction.

One result is very striking: the tail towards the sun demanded by gravitation alone at once disappears. The limiting surfaces which Roche's calculations demand are so very like some of the surfaces actually observed in the head of a comet, where they can be best seen, that it is suggested that the movement of the particles takes place in the precise direction where they would flow according to M. Roche's mathematical investigations.

Hence we are justified in attributing some cometary phenomena to the flow of matter acting under the influence of attraction and solar repulsion.[2] In concluding his memoir Roche points out (p. 393) that the hypothesis of a repulsive force acting along a radius vector, and varying inversely as the square of the distance, and only acting on matter reduced to a state of great rarefaction, gives figures identical with those observed. We see the germ of the tail is the part of the atmosphere the furthest removed from the sun, and it is easy to explain the enormous development of the emission of cometary particles near perihelion. The existence of a repulsive force which counterbalances the solar attraction M. Roche therefore considers established by his researches.

It must, however, be at once stated that much remains to be done before all the help that M. Roche's work can afford can be

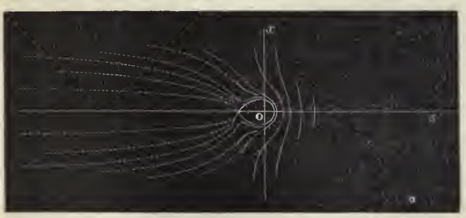


Fig. 26.—M. Roche's theoretical construction of the head of a comet, a repulsive force being taken into account.

utilized, and there is little question that the outflow in the solar direction has not been so entirely abolished as his figures indicate. This, however, may to a certain extent depend upon the fact that the observations of comets have been made at some distance from perihelion. But there may be another reason. If the outflow along the limiting surface is an outflow of solid particles, the solar repulsion will not be effective until collisions have reduced this dust to vapour. We shall still therefore have the quasi-conical surface *turned towards the sun*,[1] though it will be soon destroyed. Many of the phenomena presented by jets and excentric envelopes may be thus caused, and the very complicated phenomena presented by Coggia's comet, and others in which the section of the cone presents the appearance of birds with their wings more or less extended, do not seem opposed to this view.

J. Norman Lockyer.

(*To be continued.*)

[1] Bessel's paper "On the Physical Constitution of Halley's Comet" is printed in the *Connaissance des Temps*, 1840.

[2] See *Annales de l'Observatoire de Paris*, vol. v.

[1] Although this does not figure in Roche's diagrams, Faye gives it in his lectures on the "Forms of Comets."

PRELIMINARY NOTE ON KEELING ATOLL, KNOWN ALSO AS THE COCOS ISLANDS.

Mr. John Murray, of the *Challenger* Expedition Office, has forwarded to us the following letter, which he has received from Dr. Guppy:—

Dear Mr. Murray,—

During my sojourn of nearly ten weeks in these islands, I was able to make a fairly complete examination of them. Here, I can only refer to some of the new features of this atoll which my investigations have disclosed, and must leave the details to be subsequently worked into a general description of the islands. Regarding myself as very fortunate in being able to examine the only atoll visited by Mr. Darwin—the atoll, in fact, which gave rise to the theory of subsidence—I at once set about making observations, without reference to any particular view of the origin of coral-reefs. I examined all the islands and islets, more than twenty in number, making a separate description of each, and reaped the benefit of the fact that this atoll has been occupied for more than half a century by residents interested in their surroundings. The result has been to convince me that several important characters of these islands escaped the attention of Mr. Darwin, partly owing to his limited stay, partly also due to his necessarily defective information of the past changes in the atoll. The features, in fact, that escaped his notice, throw considerable light on the mode of origin of these lagoon islands, and give no support to the theory of subsidence.

In the first place, I have ascertained that Keeling Atoll consists essentially of a ring of horse-shoe or crescentic islands inclosing a lagoon and presenting their convexities seaward. The crescentic form is possessed in varying degrees by different islands: some of the smaller ones are perfect horse-shoe atollons, and inclose a shallow lagoonlet; others, again, exhibit only a semi-crescentic form; whilst the larger islands have been produced by the union of several islands of this shape. The whole land-surface, however, is subject to continual change. The extremities of islands are often being gradually swept away or extended. Some islands are breached during heavy gales, others are joined, so that by the repetition of these changes the island in the course of time loses its original form. Hence it is that, although the crescent is the primitive shape of each island this structure is partly disguised in the case of some of the larger islands by the union of several of smaller size. The Admiralty chart gives but an imperfect idea of the true shape of the islands; but, notwithstanding, its inspection will prove very instructive.

In truth, Keeling Atoll exhibits in an incomplete manner the features of the large compound atoll of the Maldive Group. If it was considerably larger and possessed a less protected lagoon, so that open-sea conditions prevailed in its interior, it would have all the features of a compound Maldive atoll—that is, an atoll consisting of a circle of small atolls or atollons. In its original condition, however, it was an atoll consisting of a circle of crescentic islands. Such it is essentially now, but extensive changes have often partly disguised this feature.

Before proceeding to explain the origin of the incompleted compound atoll of the Keeling Islands, it will be necessary to dwell on the exaggerated prevailing notion of an atoll. This kind of coral-reef is usually described as a circular reef inclosing a deep basin or lagoon; but this description only applies to very small atolls less than a mile across. By drawing a section on a true scale of an atoll of average size, like Keeling Atoll, it will at once become apparent that such a description

gives a very misleading idea of the real nature of this class of reef. A section of Keeling Atoll, drawn from the 1000-fathom line on a true scale of an inch to the mile, and intended to illustrate a breadth of six miles, and a depth in the lagoon of 9 or 10 fathoms, would represent to the naked eye a flat-topped mountain, the depth of the so-called basin on the summit being merely represented by a slight central depression of about 1/100 of an inch. If the lagoon possessed a depth of 30 fathoms, the inclosed basin so-called would only be indicated in this section by a central depression of about 3/100 of an inch. So trifling a proportion does the depth of an atoll of ordinary size bear to the breadth, that such a reef can only be accurately described as possessing a broad level surface, with very slightly raised margins. A correct model of Keeling Atoll would at once convey a just idea of the true relative dimensions of a reef of this class. The lagoon would be there only represented by a film of water occupying a slight hollow in the level mountain-top. By thus grasping these facts, we at once perceive that by reason of our failing to view an atoll in relation to its surroundings, and through our misconceptions of its dimensions, we have been led to introduce a great cause to explain a very small effect. The slightly raised margins can be easily explained by causes dwelt upon by Murray, Agassiz, and others. No movement of the earth's crust is necessary for this purpose. The mode of growth of corals, the action of the waves, and the influence of the currents, afford agencies quite sufficient to produce the slightly raised margins of an atoll.

The development of the islands of an atoll into horse-shoe or crescentic islands, as in the instance of Keeling Atoll, or into perfect small atolls or atollons, as in the Maldive Group, is a subsequent process to be shortly explained. These small atolls and horse-shoe islands only assume their characteristic forms *after the island has been thrown up by the waves.* Such was the conclusion I arrived at concerning small atolls and crescent-shaped coral islands in the Solomon Islands (Proc. Roy. Soc. Edin., 1885–86, p. 900); and as just stated I have formed the same opinion concerning the islands of Keeling Atoll. There is in the first place the island from which "lateral extensions grow out on either side so as to ultimately form a horse-shoe reef," which itself under favourable conditions may develop into a small atoll. In the Solomon Islands I imperfectly grasped the method by which these changes in form are effected. In Keeling Atoll I saw the process in operation, and I arrived at the conclusion that wherever a coral island stems a constant surface-current, the sand produced by the breakers on the outer edge of the reef will mostly be deposited by the current on each side of the island in the form of two lateral banks or extensions, giving the island ultimately a horse-shoe form, with the convexity presented against the current. The process may be aptly compared to the formation of a V-shaped ridge of sand when a stake or some other obstacle is placed in a river-bed. The stake represents the original small island thrown up by the waves. The V-shaped ridge of sand represents the arms of the horse-shoe island which are subsequently formed. The back-wash or eddy may in the river-bed join the arms of the V-shaped ridge of sand. In a similar manner a horse-shoe island may have a bank thrown up across the mouth, and thus a small atoll is formed. Such is the process, imperfectly disclosed to me in the Solomon Islands, that I found illustrated in all its stages in Keeling Atoll. In the Keeling Islands, however, it was necessary to satisfy myself of the reality of the agencies chiefly concerned in this process. For instance, I had to ascertain how and to what extent the surface-currents acted, and to discover the source of the sand. It was also necessary to observe what changes in the form and extent of the islands had occurred in the experience of the residents during the half-century of their occupation.

The westerly equatorial drift or south-east trade current, striking the south-east angle of the atoll, there divides and sweeps around the coasts, the two branches meeting and forming an eddy off the north-west island, a spot where drift timbers are often detained and stranded after having been swept around half the circumference of the atoll. Advantage of this current is taken by the proprietor of the islands, who directs his men to mark any logs of valuable timber thrown up on the weather or south-east coast, and then to launch them again outside the breakers. In this way huge logs are transported by the current to any particular island. If left alone, the logs, whether drifted around the north or south side of the atoll, arrive finally in the eddy off the north-west angle. This current finds its way into the lagoon through the several passages between the islands its rate there varying usually from half a knot to two knots in the hour. Only rarely is there any check to the inflow of water through the passages, as, for instance, during north-west gales.

The current in these passages carries daily a large amount of sand into the lagoon. I discovered this accidentally whilst using the tow-net for catching the pelagic animals brought in by the current. The source of this sand is the weather edge of the reef on the outer side of the islands, where the breakers are unceasingly at work in keeping up the supply. After several measurements under varying conditions of current, tide, and depth, I estimated that during every day of ordinary weather at least 10 tons of sand are carried through the passages into the lagoon. During gales and cyclones this amount is greatly increased; and probably the estimate for an ordinary year would not be less than 5000 tons. The bulk of this sand is deposited by the current near the inner mouths of the passages and on the margins of the lagoon, where it goes to extend the islands in the form of banks stretching into the lagoon. In this manner an island obtains a horse-shoe shape, just as the V-shaped ridge is formed by placing a stake in a river-bed. The first stage is represented by an island with two sand-banks extending into the lagoon, one from each extremity. The second stage is that in which the island has attained a semi-crescentic shape by the encroachment of its vegetation on the newly formed banks. In the course of time, when the vegetation of the island has entirely occupied the banks, the third stage, that of the horse-shoe island, is reached. In some instances, there is yet a further stage, when during a long continuance of westerly winds another bank is thrown up across the mouth of the horse-shoe, and a small atoll with a shallow lagoonlet is produced. Thus the currents are the principal agencies in forming the horse-shoe islands of Keeling Atoll. In large atolls, where more open-sea conditions prevail in the lagoon, and especially where, as in the Maldives, there are two opposite sets of winds and surface-currents, each prevailing in its own half of the year, we should expect to find the horse-shoe island replaced by an atollon. Keeling Atoll, however, lies for eleven months out of the twelve within the region of the constant trade-wind and westerly drift current, so that the situation is only one favouring the formation of horse-shoe islands facing to the southward and eastward. The protected character of the lagoon, also, is not a condition that would assist the growth of a circular island or atollon.

Another important feature in this atoll is to be found in the existence outside the seaward edge of the present reef of a series of submerged lines of growing corals separated from each other by sandy intervals. Unfortunately, I was not able to examine these to the extent I desired, since it can only be satisfactorily done later in the year, when the sea is sufficiently smooth to allow boats to approach the breaker edge of the reef. This feature, however, is familiar to the residents, who have supplied me with information on the subject. It would seem that all around the circumference of this atoll there is a space outside the present edge of the reef varying from 200 to 500 or 600 yards in width, where ships have anchored, and where boats in the calm season go with fishing parties. Here the submarine slope slopes gradually down to 20 or 30 fathoms; but beyond this the descent is precipitous. It is on this gradual slope that the lines of growing coral occur, separated by sandy intervals from each other. There may be two or three of these lines, the innermost covered by 4 or 5 fathoms, and the outer by from 20 to 30 fathoms.

We are thus able to perceive that the outward extension of the reef is effected, not so much by the seaward growth of the present edge of the reef, as by the formation outside it of a line of growing corals, which when it reaches the surface reclaims, so to speak, the space inside it, which is soon filled up with sand and reef-*débris.* The evidence, in fact, goes to show that a reef grows seaward rather by jumps than by a gradual outward growth. This inference is of considerable importance, since it connects all classes of reefs together in the matter of their seaward growth, the degree of inclination of the submarine slope being the chief determining factor.

Following Le Conte, I have previously shown (Proc. Roy. Soc. Edin. 1885–86, p. 884) that where there is a very gradual submarine slope the deposition of sand and the presence of much sediment in the water will prevent the growth of corals in the shallow water outside the seaward edge of the reef, and that in consequence a line of living corals will spring up in the clearer and deeper waters a considerable distance beyond. The appearance of this line of coral at the surface will result in the production of a barrier-reef with a lagoon-channel inside. In

a similar manner the submerged line of growing corals immediately outside the weather-edge of the reef of Keeling Atoll would form a barrier-reef, if it was removed some miles from the shore instead of being only about 100 yards distant. As it is now situated, it lies too close to the edge of the present reef to prevent the obliteration of the channel inside it after it has reached the surface. Its lagoon-channel would be very quickly filled with sand and reef-*débris*, and as a result we should merely have a permanent addition to the present reef-flat, which, when the process was complete, would be 100 yards wider. The process is the same as in the case of a barrier-reef, the difference in the result being due to the submerged line of corals being too close to the edge of the reef for the preservation of the interior channel; and this circumstance is due to the fact of the submarine slope being greater than in the case of a coast fronted by a barrier-reef. These remarks are merely intended to be suggestive. They may, perhaps, direct the attention of of other observers to the examination of the outer slopes of atolls and to their mode of seaward growth. This can only be done during unusually calm weather.

I have discovered many other new features of minor interest in connection with Keeling Atoll, to which I will refer in my full description of these islands. The island of North Keeling, lying fifteen miles to the north, is a small atoll connected with Keeling Atoll by a bank. I hope to describe it at some future time.

In conclusion, I may state that most of my observations in these islands were directed towards estimating the age of Keeling Atoll. These data have yet to be worked up, and I am fairly confident of getting a satisfactory estimate. The lagoon is rapidly filling up with sand and coral, but it is almost impossible to state in precise terms the changes since the visit of the *Beagle*, as the survey then made was little more than a sketch. The present Admiralty chart is of but little service in inquiring into past changes, for in it the original survey of the *Beagle* in 1836 has received several later additions, and there is nothing to distinguish the one from the other. For the purpose of navigation, and for the advantage of science, a complete examination of these islands should be made. The best season for surveying is during the calm weather of the months of January and February, when boats can venture close to the edge of the reef, and a satisfactory examination of the outer shores, as well as the interior of the atoll, can then be made. In collecting information from the residents, it will be necessary to remember that no records are kept in the islands; and in studying past changes the observer will have to receive what may at first sight appear to be very interesting facts with scientific caution. Some corroboration of such facts should always be looked for.

Yours faithfully,

Batavia, November 8. H. B. GUPPY.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, December 20, 1888.—"Correlations and their Measurement, chiefly from Anthropometric Data." By Francis Galton, F.R.S.

Two organs are said to be co-related or correlated, when variations in the one are generally accompanied by variations in the other, in the same direction, while the closeness of the relation differs in different pairs of organs. All variations being due to the aggregate effect of many causes, the correlation is a consequence of a part of those causes having a common influence over both of the variables, and the larger the proportion of the common influences the closer will be the correlation. The length of the cubit is correlated with the stature, because a long cubit usually implies a tall man. If the correlation between them were very close, a very long cubit would usually imply a very tall stature, but if it were not very close, a very long cubit would be on the average associated with only a tall stature, and not a very tall one; while, if it were *nil*, a very long cubit would be associated with no especial stature, and therefore, on the average, with mediocrity. The relation between the cubit and the stature will serve as a specimen of other correlations. It is expressed in its simplest form when the relation is not measured between their actual lengths, but between (*a*) the deviation of the length of the cubit from the mean of the lengths of all the cubits under discussion, and (*b*) the deviation of the mean of the corresponding statures from the mean of all the statures under discussion. Moreover these deviations should be expressed on the following method in terms of their respective variabilities. In the case of the cubit, all the measures of the left cubit in the group under discussion, and which were recorded in inches, were marshalled in the order of their magnitude, and those of them were noted that occupied the first, second, and third quarterly divisions of the series. Calling these measures Q_1, M, and Q_3, the deviations were measured from M, in terms of inches divided by $\frac{1}{2}(Q_3 - Q_1)$, which divisor we will call Q. Similarly as regards the statures. [It will be noted that Q is practically the same as the probable error.] This having been done, it was found that, whatever the deviation, y, of the cubit might be, the mean value of the corresponding deviations of stature was $0 \cdot 8y$; and, conversely, whatever the deviation, y', of the stature might be, the mean value of the corresponding deviations of the cubit was also $0 \cdot 8y'$. Therefore this factor of 0·8, which may be expressed by the symbol r, measures the closeness of the correlation, or of the reciprocal relation between the cubit and the stature. The M and Q values of these and other elements were found to be as follow:[1] left cubit, 18·05 and 0·56; stature 67·2 and 1·75; head length, 7·62 and 0·19; head breadth, 6·00 and 0·18; left middle finger, 4·54 and 0·15; height of right knee, 20·50 and 0·80; all the measures being in inches. The values of r in the following pairs of variables were found to be: head length and stature, 0·35; left middle finger and stature, 0·70; head breadth and head length, 0·45; height of knee and stature, 0·9; left cubit and height of right knee, 0·8. The comparison of the observed results with those calculated from the above data showed a very close agreement. The measures were of 350 male adults, containing a large proportion of students barely above twenty-one years of age, made at the laboratory at South Kensington, belonging to the author.

These results are identical in form with those already arrived at by the author in his memoir on hereditary stature (Proc. Roy. Soc., vol. xl, p. 42, 1886), when discussing the general law of kinship. In that memoir, and in the appendix to it by Mr. J. D. Hamilton Dickson, their *rationale* is fully discussed. In fact, the family resemblance of kinsmen is nothing more than a special case of correlation.

The general result of the inquiry was that, when two variables that are severally conformable to the law of frequency of error, are correlated together, the conditions and measure of their closeness of correlation admits of being easily expressed. Let x_1, x_2, x_3, &c., be the deviations in inches, or other absolute measure, of the several "relatives" of a large number of "subjects," each of whom has a deviation, y, and let X be the mean of the values of x_1, x_2, x_3, &c. Then (1) $y = r$X, whatever may be the value of y. (2) If the deviations are measured, not in inches or other absolute standard, but in units, each equal to the Q (that is, to the probable error) of their respective systems, then r will be the same, whichever of the two correlated variables is taken for the subject. In other words, the relation between them becomes reciprocal; it is strictly a correlation. (3) r is always less than 1. (4) r (which, in the memoir on hereditary stature, was called the ratio of regression) is a measure of the closeness of correlation. Other points were dwelt upon in the memoir, that are not mentioned here: among these was as follows: (5) The probable error, or Q, of the distribution of x_1, x_2, x_3, &c., about X, is the same for all values of y, and is equal to $\sqrt{(1 - r^2)}$ when the conditions specified in (2) are observed.

It should be noted that the use of the Q unit enables the variations of the most diverse qualities to be compared with as much precision as those of the same quality. Thus, variations in lung-capacity which are measured in volume can be compared with those of strength measured by weight lifted, or of swiftness measured in time and distance. It places all variables on a common footing.

"Preliminary Account of the Morphology of the Sporophyte of *Splachnum luteum*." By J. R. Vaizey, M.A., of Peterhouse, Cambridge. Communicated by Francis Darwin, F.R.S.

[1] The head length is here the maximum length measured from the notch below the brow. The cubit is measured with the hand prone, from the flexed elbow to the tip of the middle finger. The height of knee is taken from a stool, on which the foot rests with the knee flexed at right angles; from this the measured thickness of the heel of the boot is subtracted. All measures had to be made in the ordinary clothing. The smallness of the number of measures, viz. 350, is of little importance, as the results run with fair smoothness. Neither does the fact of most of the persons measured being hardly full grown affect the main results. It somewhat diminishes the values of M, and very slightly increases that of Q, but it cannot be expected to have any sensible influence on the value of r.

Royal Meteorological Society, December 19.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—On the prolonged spell of cold weather from September 1887 to October 1888, by Mr. C. Harding. During the fifty-nine weeks ending the third week in October, there were but four warm weeks in the north-west of England, and only five warm weeks in the south-west of England, whilst in the latter district there was not a single warm week between March 12 and October 22. The mean temperature for the whole period was dealt with for the twelve districts into which the Meteorological Office divides the whole area of the United Kingdom, and with the single exception of the north of Scotland the weather for the period ending in October this year was the coldest of any during the past ten years. At Greenwich the temperature during the fourteen months was below the average on 312 days out of 427, or 73 per cent., and in July there was not a single warm day, the temperature being continuously below the average from June 27 to August 6. The means for July 11 and 12 were colder by several degrees than those for March 9 and 10.—Report on the phenological observations for 1888, by the Rev. T. A. Preston. Vegetation was generally backward throughout the season. In the south west of England and south of Ireland plants were earlier than usual, but not elsewhere. In February they were from one to four weeks later, and gradually gained ground till June. In the south of Ireland they were slightly in advance of the average in June and July; in the south-west of England they just reached the average in July; whilst in Guernsey they were a fortnight later. Fruits generally were a failure; very few really ripened, and from want of sun were deficient in flavour. Haymaking was unusually late (as much as five weeks); it began in July or August, and was not entirely finished till late in September; much of it was spoilt or secured in bad condition. Straw was plentiful, and though the corn was not an average crop, the fine October enabled farmers to secure a better one than could have been expected. Roots were often a failure, and potatoes were much diseased.—A winter's weather in Massowah, by Captain D. Wilson-Barker. This paper gives the results of four-hourly observations during December 1887 to February 1888. The highest shade temperature was 95°, and the lowest 68°.

Zoological Society, December 18, 1888.—Mr. Howard Saunders in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of November 1888, and called attention to a specimen of the Small-clawed Otter (*Lutra leptonyx*), presented by Mr. W. L. Sclater, Deputy Superintendent, Indian Museum, Calcutta, new to the Society's Collection, and to a Monkey of the genus *Cercopithecus*, from South Africa, apparently referable to the Samango Monkey (*Cercopithecus samango*), also new to the Society's Collection.—Mr. G. B. Sowerby read descriptions of fourteen new species of Shells from China, Japan, and the Andaman Islands, chiefly collected by Deputy Surgeon-General R. Hungerford.—A communication was read from Mr. Herbert Druce, in which he gave an account of the Lepidoptera-Heterocera collected by Mr. C. M. Woodford in Guadalcanar Island, Solomon Islands. The collection was stated to contain examples of 53 species, 18 of which were described as new to science.—Mr. J. H. Leech read the second portion of a paper on the Lepidoptera of Japan and Corea, comprising an account of the Sphingidæ, Bombycidæ, Notodontidæ, and Cymatophoridæ, in all 352 species. Of these, 38 species were now described as new to science.—Dr. Hans Gadow read a paper on the numbers and on the phylogenetic development of the remiges of Birds. The author showed that the number of primaries is of very limited taxonomic value, as was proved by the numerous exceptions mentioned in the lists contained in the paper. A comparison of the remiges of the Penguins with those of other Carinatæ seemed to indicate an extremely low stage in the Penguins, which, however, was not borne out by other anatomical features. The Ratitæ were most probably descendants of birds which formerly possessed the power of flight and had lost it. This view was strengthened by an examination of the structure of the wings and of the feathers of their nestlings. The paper concluded with general remarks upon the probable gradual development of the organism of flight in birds.

PARIS.

Academy of Sciences, December 17, 1888.—M. Janssen in the chair.—On the analytical theory of heat, by M. H. Poincaré. In a previous note (*Comptes rendus*, civ. p. 1754) the author studied the problem connected with the cooling of a homogeneous and isotropous solid body; here a more satisfactory demonstration is given of the theorem growing out of that problem.—On the abruptly and slowly contracting muscles of the hare, by M. L. Ranvier. A recent experiment is described, which has been carried out for the purpose of studying in the hare the two species of muscles, which in the rabbit differ in colour, structure, and functions, but which in the hare are all alike red.—On M. Zédé's submarine boat, the *Gymnote*, by Admiral Paris. An account is given of the first trial of this boat, recently launched at Toulon, and constructed for the purpose of realizing the suggestions made by M. Dupuy de Lôme on the subject of submarine electric navigation. As this is an engine of warfare, the details of its mechanism are suppressed; but it is stated that the trial more than realized the expectations of its inventor. It works by electricity, with perfect ease, on, and at any desired depth below, the surface, obeys the helm in all positions, fully attains the hoped-for velocity, and its ventilation and lighting are all that can be expected down to a certain depth. By introducing sundry obvious modifications, boats of this description may be turned to the best account for the purpose of scientific marine exploration.—Eocene Echinidæ in the province of Alicante, Spain, by M. Cotteau. The recent explorations of the Eocene formations in this region have yielded as many as 76 species of fossil Echinidæ, grouped in 36 genera, and representing nearly all existing groups of this family. Of the species, 50 are new to science, and some of these are specially interesting, as they belong to extremely rare genera, well deserving the attention of palæontologists, and four of which are quite new. A striking feature of this Eocene Echinidian fauna is the enormous preponderance of irregular over regular forms, the former comprising as many as 67 out of the 76 species here described.—On the nutriment of castaways at sea, by Prince Albert of Monaco. The researches made during the *Hirondelle's* last expedition in the North Atlantic tend to show that the crew of a vessel short of provisions might support life indefinitely if supplied with the proper appliances for capturing the small marine fauna which is found to exist in great abundance in the Atlantic, and probably in all temperate and warm marine waters.—On the diurnal variation of the barometer, by M. Alfred Angot. It is shown that diurnal barometric variation results from the interference of two distinct waves. One of these is exclusively due to the diurnal variation of temperature in the given region, and subject like it to local influences. The other, of semi-diurnal periodicity, is produced by a general cause independent of all local influence; its phase is constant, approximating to 63°, and its amplitude for all regions and all seasons is determined by an equation, whose terms show a certain analogy with those corresponding to the theory of the tides.—On certain new properties and on the analysis of the fluoride of ethyl, by M. H. Moissan. In a previous communication the author showed that ethylfluorhydric ether (ethyl fluoride) was a gaseous body capable of being obtained in a very pure state, and causing ethyl iodide to react on the anhydrous fluoride of silver. Here he describes several other properties of the same substance. Heated to a dull red for several hours in a glass ball, the fluoride of ethyl yields a complex mixture of carburets containing traces only of the fluoride of silicium. Under the action of a weak induction spark the volume increases greatly, yielding hydrofluoric acid, a small quantity of acetylene, and especially ethylene, without depositing carbon. In the presence of a powerful spark, carbon is deposited with formation of acetylene, ethylene, propylene, &c.—On the employment of oxygenated water for the quantitative analysis of the metals of the iron group (continued), by M. Ad. Carnot. Here the author deals more especially with chromium and manganese.—On the reproduction of zircon, by MM. P. Hautefeuille and A. Perrey. Zircon, obtained at a very high temperature by Sainte-Claire Deville and Caron, by making the fluoride of zirconium to act on silica or on silicium fluoride, is here reproduced at a temperature not exceeding 700° C. by the action of the bimolybdate of lithin on a mixture of zircon and silica. This is the same process by means of which these chemists have obtained the emerald and phenacite.—Papers are contributed by M. Raoul Varet, on the action of the cyanide of mercury on the salts of copper; by M. Albert Colson, on a diquinolic base; by M. W. Louguinine, on the heats of combustion of the camphors and borneols; by M. Louis Crié, on the affinities of the Jurassic and Triassic floras of Australia and New Zealand; and by M. Michel Hardy, on the discovery of a Quaternary burial-place at Raymonden, in the commune of Chancelade, Dordogne.

December 24.—M. Janssen in the chair.—After the usual annual allocution pronounced by the President, M. Janssen, the names were announced of the successful competitors in the prize essays proposed for the year 1888. These were as under:—*Geometry:* Grand Prize of the Mathematical Sciences, M. Emile Picard; Prix Bordin, Madame Sophie de Kowalewsky; Prix Francœur, M. Emile Barbier; Prix Poncelet, M. E. Collignon. *Mechanics:* Extraordinary Prize of 6000 francs, MM. Banaré, Hauser, and Reynaud, 2000 francs each; Prix Montyon, M. H. Bazin; Prix Plumey, Madame Benjamin Normand and family; Prix Dalmont, M. Jean Resal. *Astronomy:* Prix Lalande, M. Joseph Bossert; Prix Valz, Mr. E. C. Pickering; Prix Janssen, Dr. William Huggins; Prix Damoiseau, not awarded. *Statistics:* Prix Montyon, M. Félix Faure, M. J. Teissier, and MM. Lallemand and Petitdidier. *Chemistry:* Prix Jecker, M. Maquenne and M. Cazeneuve. *Geology:* Prix Cuvier, M. Joseph Leidy. *Botany:* Prix Desmazières, M. V. Fayod; Prix Montagne, M. Gaston Bonnier. *Agriculture:* Prix Vaillant, not awarded. *Anatomy and Zoology:* Prix Savigny, not awarded; Prix Thore, Dr. Carlet; Prix da Gama Machado, not awarded. *Medicine and Surgery:* Prix Montyon, Dr. Hardy, Dr. Albert Hénocque, and MM. Follin and Duplay; Prix Bréant, Dr. Hauser; Prix Barbier, MM. Leroy, Raphael Dubois, and Dr. Ehrmann; Prix Godard, Dr. Maurice Hache; Prix Lallemand, MM. François-Franck and Paul Blocq. *Physiology:* Prix Montyon, Dr. Augustus D. Waller (London) and M. Léon Fredericq. *Geography:* Prix Gay, M. Simart. *General Prizes:* Prix Montyon (Unhealthy Industries), Dr. Paquelin and M. Fumat; Prix Trémont, M. Fénon; Prix Gegner, M. Valson; Prix Delalande-Guérineau, Père Roblet; Prix Jérome Ponti, M. Kœnigs; Prix Laplace, M. Paul-Louis Weiss.—The programme of prizes proposed for the year 1889 comprises the following:—*Geometry:* Prix Francœur (1000 fr.), discoveries or works useful to the progress of pure or applied mathematical sciences; Prix Poncelet (2000 fr.), same subject. *Mechanics:* Extraordinary Prize of 6000 francs for any invention tending to increase the efficacy of the French naval forces; Prix Montyon (700 fr.), invention or improvement of instruments useful to the progress of agriculture, the mechanical arts or sciences; Prix Plumey (2500 fr.), any invention or improvement tending most to the progress of steam navigation; Prix Fourneyron (500 fr.), theoretical and practical essay on the progress of aërial navigation since 1880. *Astronomy:* Prix Lalande (540 fr.), any essay or observation most useful to the progress of astronomy; Prix Valz (460 fr.), the most interesting astronomical observation during the year; Prix Janssen (gold medal), any discovery or work tending to the progress of physical astronomy. *Physics:* Prix L. La Caze (three of 10,000 fr. each), the best work on physics, chemistry, and Physiology. *Statistics:* Prix Montyon (500 fr.), the best work on the statistics of France. *Chemistry:* Prix Jecker (10,000 fr.), any work tending most to the progress of organic chemistry. *Geology:* Prix Delesse (1400 fr.), best work on geology or mineralogy. *Botany:* Prix Barbier (2000 fr.), most useful discovery in medicine, surgery, pharmacy, or botany; Prix Desmazières (1600 fr.), the most useful work on all or any section of Cryptogamy; Prix Montagne (1000 and 500 fr.), useful works on the anatomy, physiology, development, or description of the lower Cryptogamous plants; Prix de la Fons Mélicocq (900 fr.), best work on the botany of North France. *Agriculture:* Prix Vaillant (4000 fr.), best work on the diseases of cereals in general. *Anatomy and Zoology:* Grand Prix des Sciences Physiques (3000 fr.), the complete study of the embryology and development of any animal; Prix Bordin (3000 fr.), a comparative study of the auditory apparatus in mammals and birds; Prix Savigny (975 fr.), in aid of young zoologists studying the invertebrates of Egypt and Syria. *Medicine and Surgery:* Prix Montyon (one or more prizes not otherwise specified), for the best work on the healing art; Prix Bréant (100,000 fr.), for a specific against cholera; Prix Godard (1000 fr.), anatomy, physiology, and pathology of the genito-urinary organs; Prix Lallemand (1800 fr.), researches on the nervous system in the widest sense of the term; Prix Ballion (1400 fr.), any work most useful to the health and improvement of the human race; Prix Mège (10,000 fr.), to continue and complete the essay of Dr. Mège on the causes that have retarded or advanced the progress of medicine. *Physiology:* Prix Montyon (750 fr.), for the promotion of experimental physiology; Prix Pourat (1800 fr.), experimental researches on muscular contraction; Prix Martin-Damourette (1400 fr.), therapeutic physiology. *Physical Geography:* Prix Gay (2500 fr.), comparative study of the floras and faunas and relations existing between the Polynesian Islands and surrounding lands. *General Prizes:* Prix Montyon, one or more prizes for the best means of rendering unhealthy industries less dangerous; Prix Trémont (1100 fr.), for any work tending in any way to promote the interests of France; Prix Gegner (4000 fr.), to promote the positive sciences; Prix Petit d'Ormoy (10,000 fr.), researches in pure and applied mathematical sciences and the natural sciences; Prix Laplace (a complete collection of the works of Laplace), the first student leaving the Ecole Polytechnique.

Astronomical Society, November 7.—M. Moussette in the chair.—Colonel Laussedat read a paper on national time, in which he urged the adoption of Paris time throughout France.—M. Gunziger observed Barnard's comet on November 4. It was about the size of the nebula in Andromeda, with scarcely any tail, but a bright nucleus of about the sixth magnitude.—Rev. S. J. Perry, of Stonyhurst College, was elected an honorary member.—The Royal Astronomical Society and the Liverpool Astronomical Society were elected Corresponding Societies.

December 5.—M. Flammarion, President, in the chair.—The President announced that important gifts had been offered for the Society's proposed Observatory: M. Bardou offered a 4-inch equatorial, M. Secretan a transit instrument, M. Lütz spectroscopic and photographic apparatus, M. Lévy a set of binocular glasses. Thanks were voted to the above donors.—M. Flammarion read a paper on the changes observed in Mars, specially referring to Dawes's forked bay and Lake Mœris.—M. Gérigny read a paper on the aberration of light, showing the influence of the sun's motion upon that phenomenon by Yvon Villarceau's method.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Boilers: their Construction and Strength: T. W. Traill (Griffin).—Our Fishery Rights in the North Atlantic: J. I. Doran (Philadelphia).—Methods of Analysis of Commercial Fertilizers, Cattle Foods, &c. (Washington).—The Probable Cause of the Displacement of Beach Lines: A. Blytt (Christiania).—Bulletin of the New York State Museum of Natural History, Nos. 4, 5, 6 (Albany).

CONTENTS.

THURSDAY, JANUARY 10, 1889.

THE LATE WILLIAM DENNY.

The Life of William Denny, Shipbuilder, Dumbarton. By Alexander Balmain Bruce. With Portrait. (London: Hodder and Stoughton, 1888.)

THE late William Denny was in many ways a remarkable man. He was a prominent member of the modern school of naval architects; an ardent advocate of scientific progress in the design and construction of ships; a strong supporter of scientific education in naval architecture; a contributor of many papers and technical data to the various professional institutions of which he was a member; and an eloquent, indefatigable, and effective exponent of his views upon all subjects. He was also the managing partner of one of the largest shipbuilding firms in the world, and was distinguished for his intimate knowledge of the many and intricate details of the business; for his clear insight into, and close grip of, the questions with which he had to deal; for his eager desire to promote good relations among the members of the firm and the various grades of workpeople in their employ; and for what he did all round towards making the business with which he was connected—as he frequently said it was his ambition to do—"a model of efficiency on all sides." Both by practice and by precept, William Denny laboured strenuously and effectively, at all times and seasons, for the advancement of his profession, and for the good of those who wished to qualify themselves for the practice of that profession.

The description of Mr. Denny's work as a naval architect occupies only a comparatively small portion of the present book—108 pages out of 478. The remainder consists of general biographical details, and accounts of Mr. Denny's views upon the many local, and the various political, social, moral, and religious questions in which he had a strong—we may say a burning—and ever-increasing interest. This general record of his acts and words will be valued by those who knew the man, and who admired and loved him—as none who really knew him could help doing—and will give to others who may read it a good idea of a life which was full of activity, interest, and promise.

It is, however, the professional work of the subject of this memoir, and not his vigorous, cultured, many-sided, full, and keenly sympathetic mind and life, that we have to do with here. Prof. Bruce, the author, says that the five chapters (VI.–X.) which deal with this branch of his subject "present a popular account of Mr. Denny's technical work, written by one who possesses no knowledge of the technic of shipbuilding." But they do much more than that, as will be inferred from the fact that Prof. Bruce availed himself of assistance by such competent authorities as Mr. Robert Duncan, the well-known Port Glasgow shipbuilder; Mr. Martell, the Chief Surveyor of Lloyd's Register Society; and Mr. F. P. Purvis, the Chief of the Scientific Department in Messrs. Denny's shipyard.

The name of Mr. Denny is inseparably associated with modern progress in scientific naval architecture. Prof. Bruce says truly that "in naval architecture he was sometimes in fact, and always in spirit, a pioneer, . . . sagacious to discern quickly the value of a new suggestion or invention, prompt to give it generous recognition, energetic and enthusiastic in taking it up and developing it until it had gained a secure place in general thought and practice;" and he "was of that earnest temper that must and will improve where improvement is possible."

Mr. William Denny belonged to a family of shipbuilders. "He was the third in succession of three Williams, of whom the two first, his grandfather and his uncle, had been men of genius in the art of shipbuilding." His grandfather started shipbuilding on his own account in Dumbarton, in 1817, when there was no shipbuilding yard on the Clyde above Dumbarton. He gained renown, during the infancy of steam navigation, as the builder of the Thames passenger-steamer *Marjory*, and of the mail-steamer *Rob Roy*, the first sea-going steamer built, which was employed at first in the Glasgow and Belfast trade, and afterwards as a passenger-vessel between Dover and Calais. He also built the *Trinidad*, the first steamer sent to the West Indies. This William Denny had seven sons, all of whom became shipbuilders. The sole survivor of the seven is Mr. Peter Denny, the father of the subject of this memoir, and the head of the shipbuilding firm of Messrs. William Denny and Brothers, and of other important commercial undertakings, whose long and honourable career and high personal qualities have obtained for him, to an unusual degree, the confidence and esteem of all who know him. Mr. Peter Denny's brother William started the present firm of William Denny and Brothers in 1844. He applied himself with great skill and success to the use of iron in shipbuilding, and in ten years he created a prosperous business. His death occurred at the same early age as that of his nephew, viz. not quite forty years.

The late Mr. William Denny's contributions to the science of naval architecture relate mainly, though not entirely, to the resistance, speed, and propulsion of ships, the stability of ships, the use of steel in construction, and to improvements in structural details and arrangements He was struck by the report of the British Association Committee in 1869—and particularly by the separate report of Mr. Froude—upon the subject of resistance, and the best way of determining by experiment the relation between speed and power in ships. Mr. Froude enunciated in his separate report the law which connects speed with resistance in floating bodies of varying size but similar forms, and enables the resistance of a full-sized ship to be calculated from that of a small model. He also showed graphically, in the shape of curves, the true variation of resistance with speed, as determined by experiment, for ship-shape models of various forms. Both these points were seized hold of and utilized at once by Mr. Denny for practical application. In January 1870, he commenced to test the speeds of steamers progressively on the measured mile, *i.e.* to determine the relation between engine power and speed at several speeds from the lowest to the highest, and to plot curves, similar to those made by Mr. Froude from model experiments, that showed the true variation of power with speed over the whole practicable range of

speed, in the vessels so tried. Previous to that time it had been generally considered that it was sufficiently accurate for ordinary purposes to ascertain the power necessary to drive a ship at one or two different speeds only, and to assume that the resistance at other speeds would vary as the square of the speed. The errors often involved by this assumption were known to be considerable; but the practice was not improved upon until the introduction of Mr. Froude's curves of resistance for models, and Mr. Denny's corresponding curves for actual ships.

In 1873, Mr. Denny entered into correspondence with Mr. Froude, and communicated to him from that time forward the results of his experiments upon the speed of ships. Those results, when compared with what were given by model experiments, were of great assistance to Mr. Froude in his investigations. Mr. Froude said, at the Institution of Naval Architects, in 1876: "Mr. Denny's horse-power results, when closely scrutinized, were found at once to supply most important information on the subject of engine friction; and they have helped to corroborate and further elucidate certain general conclusions on the subject of the expenditure of power in propulsion, which other less crucial tests had enabled me to arrive at approximately." Mr. Froude read a paper, from which the foregoing is a quotation, in which he used the data referred to for determining the ratio of indicated to effective horse-power in ships. He also read a second paper, at the same meeting, in which he said that "the trial of Mr. Denny's ship *Merkara*, referred to in the paper I have already read, furnished materials for extending and giving practical completeness to a comparison [of the resistances of long ships of several types] which our series of experiments had already led us to institute between several types of form." This furnishes an excellent illustration of how progress may be facilitated by the close co-operation of the scientific investigator and experimentalist with the practical worker who requires to understand and apply the teachings of science; and it is one proof out of many of the scientific value of Mr. Denny's early speed trials, and of his readiness to communicate freely the results to others interested in the subject. Mr. Denny laboured with great enthusiasm to perfect the data obtained on measured mile trials of ships, and to collect and record it systematically; and he was always ready to place such information at the disposal of other workers in the same field.

Mr. Denny read a paper, in 1875, before the Institution of Engineers and Shipbuilders of Scotland, on "The Difficulties of Speed Calculations," in which he strongly insisted upon the uselessness of the ordinary speed formulas, and urged the desirability of having all steamers, if possible, tried progressively. He gave conclusive force to his arguments by exhibiting curves of power and speed for several ships, which showed large departures of the curves given by the standard formulas from the curves which had been deduced from actual trials of the ships. He never afterwards ceased to call attention to the great advantage of the improved system of speed trials; and he soon had the satisfaction of seeing it brought into general use. Mr. Froude said, in 1876, in one of the papers above referred to: "It is to Mr. Denny's honour that, finding the so-called constants [in the speed formulas then in use] were invariably variable and inconstant, he determined of himself to strike out a new line, and find out by trial what is fact, instead of contenting himself with assuming what ought to be the relation between indicated horse-power and speed."

In 1881, Mr. Denny, with the consent of his partners, took the important step of erecting an experimental tank in the shipyard at Dumbarton for the purpose of carrying out independent trials of the resistance of models such as Mr. Froude had long been doing at Torquay. So long before as 1877 he said at a meeting of the Institution of Naval Architects: "The attention of all mercantile naval architects should be called to the fact that all Mr. Froude's experiments bear strongly and directly on our work; and unless we follow them thoroughly and follow them accurately, and with an anxious spirit, we shall not succeed as we ought in taking the lead of those countries interested in shipbuilding." His biographer says that in 1881, when a large extension of the Dumbarton shipyard was commenced, "the erection of an experimental tank became a subject of serious consideration with Mr. William Denny. He had become convinced that the expenditure involved in the construction and maintenance of such a tank would be justified by its utility. The result was that the present Dumbarton tank, the only one either in existence or in contemplation under private control, was devised, constructed, and equipped." The magnitude and difficulty of the work will be understood when it is remembered that the water-space contained in the so-called tank is 300 feet long and 22 feet broad, and the depth of water 9 feet; that the models experimented upon are fashioned out of solid paraffin by mechanism originally devised by Mr. Froude for the purpose; and that the models are towed from end to end of the tank by means of an overhead carriage fitted with delicate apparatus for accurately measuring the speed of the model and the force applied to maintain it at that speed, and for automatically producing a graphic record, in a form suitable for subsequent measurement and calculation, of the results of each experiment. The outlay and thought demanded by such an extensive, complicated, and novel undertaking was very great; but the greatest difficulty of all would be the formation of a staff competent to make it fruitful and successful in results. Mr. Froude was a man of genius, and everyone could not work with his tools. However, Mr. Denny soon organized a staff of skilled assistants who have proved their fitness for the task to which they were put. Prof. Bruce says that "since the tank was opened some twenty thousand experiments have been made on models of ships previously built and tried on the measured mile, or of ships in process of design, or of ideal ships conceived for the purposes of experiment."

Mr. Denny did much to simplify and improve the methods of calculation for determining a ship's stability, and to apply the known science of the subject to the practical work of the designing office. In 1880 he commenced the practice of ascertaining by experiment the position of the centre of gravity of every ship built by his firm; and the data thus obtained for numerous ships were perfected and carefully systematized for guidance in the preparation of new designs. Amsler's mechanical

integrator, and other mechanical devices for reducing the labour of calculations made in a naval architect's office, were extensively used; and the time required for such work was reduced to a small fraction of that formerly expended. The integrator was modified by its inventor at Mr. Denny's suggestion, so as to be better fitted for the special work of stability, and other, calculations; while, on the other hand, the integrator was used so as to enable large simplifications to be effected in the systems of calculation. The manner in which the mechanical integrator was used in Mr. Denny's office to reduce the time and labour involved by laborious calculations, and to effect improvements in the methods of calculation themselves, is well illustrated by one of Mr. Denny's most valuable contributions to the science of naval architecture, which is contained in a paper on "Cross Curves of Stability, their Uses, and a Method of constructing them obviating the Necessity for the Usual Correction for the Differences of the Wedges of Immersion and Emersion."

Mr. Denny, as was his custom throughout, gave a practical direction to his work in connection with the subject of stability, by preparing for each steamer built by his firm general particulars of her technical qualities, "such as dead-weight capability, speed and power, stability," and other matters of importance, for the guidance of her owner and captain.

Mr. Denny was one of the most prominent advocates and pioneers of the recent change from iron to mild steel as the material of a ship's construction. He was the builder of the *Rotomahana*, the first mercantile ocean-going steamer constructed of the new material—a vessel which soon answered objections made to the use of steel by grounding on a rocky bottom and proving its superiority over iron when subjected to the roughest of treatment. Mr. Denny showed, in a paper read before the Iron and Steel Institute in 1881, that the extra cost of steel per ton would be more than counterbalanced by savings effected in the weight of structure, and by the additional weight of cargo that could thus be carried. The truth of this view was soon proved by the commercial results of the use of steel ships. He was always a consistent advocate of the use of steel, and a stout upholder of its merits against all attacks; and he often pointed out with great force and truth that defects arising from faulty design or bad work had been attributed to the material itself. Hence he strongly urged the necessity for more careful study of structural defects, and of what he called the "morbid anatomy of ships."

It is impossible, within the space at our disposal, to deal thoroughly with, or even to notice all of, the many subjects associated with Mr. Denny's name. He did much to improve the structural details of design, and to bring about the introduction of double bottoms into mercantile steamers. He was always an able and close critic of the rules by which Lloyd's surveyors are guided in the survey of ships for classification, and of the manner in which those rules are carried out. He proposed, in 1877, to frame new rules, by which the displacement of a vessel would be the standard for regulating the thickness of the plating and the sizes of the frames and other parts. Mr. Denny's arguments were met with what, at the time, may have been a sufficient answer, viz. that there were no fixed load-lines for ships, and therefore there was no definite amount of displacement. This answer does not now hold good, however, seeing that since the report of the Load-line Committee, in 1885, full means have been in operation for fixing the load-lines, and therefore the displacements, of ships. But there still remains much to be said, on both sides, about Mr. Denny's proposals.

Mr. Denny did some very valuable work as a member of the Load-line Committee, a description of which will be found in Chapter IX. In that Committee; at an interview with the President of the Board of Trade in 1883; in giving evidence before the Royal Commission on Loss of Life at Sea in 1885; and on all other suitable occasions, Mr. Denny advocated a reform of the Marine Department of the Board of Trade, in the direction of forming a Board whose members would have personal knowledge of the subjects dealt with and the interests affected by them, and of strengthening the executive staff of that Department by furnishing it with the best scientific assistance that could be procured.

The cause of technical education never had a stronger supporter than Mr. Denny. He advocated it, and worked in it, at all times and seasons. Everyone engaged in the scientific teaching of naval architecture had his eager encouragement and generous help. The writer owes much to him for practical assistance of every kind in connection with the commencement and carrying on of the work of the Chair of Naval Architecture in Glasgow University. Mr. Denny was always ready to apply the resources of his establishment to the benefit of others who were working in the cause he had at heart. He was an ardent advocate of technical education for all who were entering the shipbuilding profession. His own early training was very thorough, and he qualified himself, as the record of his work proves, to occupy the first rank among naval architects. Yet we find him dissatisfied, and saying in 1883 to one who consulted him as to sending a son to the Royal Naval College, "None of the rest of us [the exception referred to is his brother Mr. Archibald Denny, who now ably fills his place at the works] were at the College, and it will be a lifelong regret to me that I missed its advantages. . . . The work of these schools [of naval architecture] is the leaven which is slowly but profoundly inspiring and changing the character of our profession." The practical efforts made by Mr. Denny himself among his own people to improve their technical training are best described in his own words. In 1883, he said:—

"Our attempts at technical education in our shipyard and engine-works consist of the following:—

"(*a*) Rules as to the admission, by examination, of apprentices and others into the shipyard offices.

"(*b*) The same for our engine-works.

"(*c*) Rules to the Awards Committee to guide them in rewarding the workmen for inventions or improvements.

"A similar scheme of awards has been begun in our engine-works.

"From these papers and the private information given to you as to the awards made, you will observe that our attempts to stimulate the intelligence of our *employés* have developed in two forms, corresponding to the main divisions of these *employés*. First, by examination we have tried to secure a supply of apprentices and others for our offices elected by ability and steadiness, and with some knowledge suitable for the careers before them,

Second, we attempt by rewards to stimulate the minds of our workmen directly to invention and to a continual criticism of the methods of work, tools, and machines employed by them. We have not tried yet to induce them to attend technical classes, but a few of them do attend such classes in the town, conducted under the control of the Science and Art Department.

"All our draughtsmen attend such classes, and in addition have from us the use of our offices, with paper, &c., free, also of a very complete library of works on naval architecture and cognate subjects, every evening excepting Saturday and Sunday."

Mr. Ward, the managing partner of the firm, reported, in 1887, that, since the introduction, seven years before, of the system of awards to workmen for inventions and improvements, "claims have been considered valuable and worthy of award to the number of 196, while rather more than three times that number have been considered altogether."

Want of space prevents our saying more respecting the subject of this memoir, though much more might be said with advantage. The early termination of Mr. Denny's career is an irreparable loss to his profession and to the cause of scientific progress in ship construction. The last professional distinction conferred upon him was that of being elected President of the Institution of Engineers and Shipbuilders in Scotland; but he did not live to deliver his Presidential address. This circumstance in connection with his death is similar to what happened in the case of a celebrated predecessor in that office, also a Clyde shipbuilder. We refer to Mr. John Elder, who died in the prime of life almost immediately after the members of the Institution elected him as their President in 1869. FRANCIS ELGAR.

MEMORY.

Memory: its Logical Relations and Cultivation. By F. W. Edridge-Green, M.B., B.S. Durham. (London: Baillière, Tindall, and Cox, 1888.)

THE title of this book is somewhat misleading. "Memory: its physiological" or "organic conditions," would be more pertinent, for "logical relations" suggest a treatment of mental association more allied to that offered, for instance, by Dr. Bradley in his "Principles of Logic." This will probably appear a trifling remark to the author, niceties of terminology seemingly being of small importance in his eyes. Throughout his work the writer lightly passes from the corporeal to the mental sphere with a serene indifference to the needs of clear conception.

The author's stand-point may be understood from the following paragraphs of the introduction:—

"What is memory? It is the process by means of which impressions of the external world and ideas are retained for use on future occasions. . . .

"Memory must be clearly distinguished from remembrance and recollection. Recollection is the power of voluntarily recalling impressions. Remembrance is the term applied when the process is involuntary. Memory is the innate power to have an impression recalled if a proper stimulus be applied. . . .

"All the above appears at first sight to be strongly against the view that memory is a definite faculty occupying a limited portion of the brain. But in the following pages I shall endeavour to prove that memory *is* a definite faculty, and has its seat in the basal ganglion of the brain, separate from, but associated with, all the other faculties of the mind."

Mr. Edridge-Green evidently does not fear the reproach of heresy, for there is hardly a sentence of the foregoing that would not be condemned by the authorities of the day. Whether memory be defined as "the process by means of which impressions and ideas are retained," or "the innate power to have an impression recalled" (we leave it to the author to reconcile his own language), —to mark it off from "remembrance and recollection" would, by most psychologists, be regarded as making a distinction without a difference. And, further, to maintain that "memory is a definite faculty occupying a limited portion of the brain," with "its seat in the basal ganglion," undoubtedly is a proof of independent opinion, if not of scientific discretion.

We are treated in chapters v. and vi. to an account, at some length, of "the faculties of the mind," as well as —although we had been told that memory is *a* definite faculty occupying a limited portion of the brain—of "the special memories" appertaining to the same. The faculties turn out to be thirty-seven, the number being five short of those alleged in "the phrenological system," the items of which are in part rechristened, and also rearranged. Regarded as conformity to popular description, there may be no great harm in all this; but something more than language is at fault in the statement that "the mind is made up of a number of faculties, each of which responds to certain impressions, and influences the mind as a whole to seek after these impressions, and to avoid their negatives." Indeed, many of the author's perplexities, and much of the reader's difficulty in comprehending him, are clearly traceable to the adoption of this somewhat antiquated and crude way of regarding the mental constitution. The topic, however, must not detain us, and we proceed to consider the author's refutation of "the hypothesis that the perception and memory of any impression occupy the same portion of the brain."

Eight reasons are apparently offered. No. 1 had perhaps best be given in the author's own words:—

"They [*i.e.* perception and memory] are totally distinct functions; thus, the eye receives the impression in the first place, but no one supposes for an instant that the eye is the seat of the memory of impressions of sight. . . . Why should the brain, having manufactured ideas, &c., remember them? The cerebral hemispheres bear exactly the same relation to the basal ganglia as the external senses do, and there is no evidence to support the theory that the cerebral hemispheres are the seat of memory."

The reasoning apparently is: the cerebral hemispheres (which possess the property of *manufacturing ideas*) stand "exactly in the same relation to the basal ganglia as the external senses." Now it is admitted that the eye, or external sense, remembers nothing, therefore neither can the cerebral hemispheres. Is this meant for reasoning, or mere dogmatism? No one who has not a theory to support would press the analogy of the eye and the cerebral hemispheres; resting as it does on nothing better than a vague resemblance of the minute structure of retina and cortex; rather, if analogy is to count for anything, it is the "basal ganglia" that should be likened to

the sense-organ, for their part, if not whole, function in the perceptive act is mediatorial.

Argument 2: "The view that the memory of an impression occupies a part of the brain distinct from the perception is simpler and more consistent than superadding the function of memory to that of some of the faculties. Thus, why should the instinct to acquire and its special memory occupy different portions of the brain, whilst the perception of a form and its memory occupy the same portion?" But who but our author alleges that they do occupy different portions?

Most of the other arguments turn upon the implications of the faculty-hypothesis, and lose whatever force they seem to possess by the abandonment of that mode of conceiving mental phenomena. Thus, No. 5: "The absolute impossibility of understanding how an impression could be split up, so that each faculty might take its share of an impression." Material separation of faculty-areas apart, one does not readily see the point of this difficulty.

Our author says: "I will take for illustration the faculty of colour, as being the *very simplest* possible; but by no stress of imagination can I conceive how an impression of colour can exist, apart from the impression itself, to be of any definite use in remembrance." The reader's imagination will very likely be as much taxed as the writer's; indeed, he will probably vainly try to imagine what the author is exerting himself to imagine. After much straining, I seem to myself to seize the intended meaning in the following rendering. The mind being regarded as an aggregate of distinct faculties, the *matter* of any impression will be simultaneously apprehended by several. Now the energy of a faculty is a function of the hemispheres. But that which is common to several faculties cannot be the exclusive property of any one. In remembering, the perceiving faculty is dormant; therefore, in remembering, some other region of the brain must be excited.—But how if the initial assumption be denied, and memory of the perception be no other than the perception *minus* the outlook? Is this not sufficiently proved by the fact that there can be no memory where there never was presentation; and that remembered presentation can, in certain circumstances, attain the intensity of original presentation?

But if mental revival be not the subjective concomitant of renewed cerebral agitation of a definite kind, how ought we to conceive the state of the case?

Thus: "all sensory impressions, whether elaborated by the faculties of the mind situate in the cerebral hemispheres or by the sensory nerves, are permanently stored up in the optic thalami, and constitute the sensory memory"—the equivalent of psychical retentiveness. Then these stored-up impressions find a way out of their hiding-places through one of two opportunities. Either when some similar impression passes "through the sensory memory centre on its way up to the mind" (= remembrance); or "through the intervention of the mind" (=recollection). The writer shows no signs of having thought out all that his descriptions imply. As for the relegation of the function of memory to the basal ganglion, nothing deserving the name of evidence is forthcoming.

In his treatment of memory in the strictly subjective aspect, Mr. Edridge-Green shows to more advantage, although a certain want of lucidity here as elsewhere forms a serious defect. He instances three laws of remembrance, which must be given in his own words:—

First Law.—"All impressions tend to revive those of a similar character previously received; but an impression in the sensory memory will not be brought before the consciousness if its psychical intensity does not reach a certain definite standard. This psychical intensity is attained by the association of impressions representing similar members of a psycho-physical series contiguously combined in a similar manner."

The law seems to amount to this: As the points of similarity in two impressions, one old and one new, increase, is the chance of recognition increased. So interpreted, it is the equivalent of what is frequently called by psychologists the law of Similarity. The law as stated by our author involves, however, his third law, or that familiarly known as the law of Contiguity.

The similarity referred to in the first law detached from contiguous association is made the subject of the second law of remembrance, which runs—

"When an impression is received similar to one received previously, unless the previous impression be revived at the same time, both impressions will remain separate; whereas if the previous impression be brought before the consciousness and recognized as similar, an association of the two impressions will take place."

The implication of this law is that the similarity of impressions alone does not suffice for revival. Over and above the resemblance of the impressions themselves there must be a perception of the resemblance. One would have thought, indeed, that this went without saying. Physical similarity or even identity is not conscious perception of resemblance. The students who did not recognize the leaf of a plane-tree, although they had been staring at it every day of their lives, had never had in the psychological sense an "impression" of the plane-leaf. The image on the retina preceded no mental image; and when their attention was called to the plane-leaf, they may be said to have then *seen* it for the first time. But having cognized it once, they cognized it always, according to a well-known dictum that cognition and re-cognition are one and the same.

This second law is no pure law of remembrance, then, but a fundamental condition of knowing.

Third Law.—"The revival of a component of an impression tends to the revival of the remaining components, and the revival of any impression tends to the revival of other impressions received about the same time; but unless these reach the necessary standard of psychical intensity, they will not be brought before the consciousness."

This is a not unhappy wording of the great law of mental association, the "law of contiguity."

As a statement of principles, whether physiological or psychological, I cannot hold this book on the mysterious subject of Memory to be a valuable contribution to scientific literature. There is evidence throughout of first-hand observation and of genuine effort to acquire an original comprehension of both physical and psychical phenomena, but the materials are ill-arranged, and the theorizing crude or mistaken. It might be well if the author, before pursuing his inquiries in this field, made

himself more acquainted with the present condition of physiological and psychological research.

The most satisfactory part of the book is the concluding section, entitled "The Cultivation of Memory," wherein some excellent practical advice is given with regard to methods of acquisition and study, the adoption of which would save much commonly wasted time and labour.

W. C. COUPLAND.

THE SPECIES OF FICUS OF THE INDO-MALAYAN ARCHIPELAGO.

The Species of Ficus of the Indo-Malayan and Chinese Countries. Part II. Synœcia, Sycidium, Covellia, Eusyce, and Neomorphe. By George King, M.B., F.R.S., &c. *Annals of the Royal Botanic Garden*, Vol. I. Part 2, pp 67–185, tt. 87–225. (Calcutta: Printed at the Secretariat Press. London: L. Reeve and Co. 1888.)

THE first part of this excellent illustrated monograph of a very difficult genus was reviewed in NATURE, vol. xxxvi. p. 242, where some details are given of the classification adopted by Dr. King. The present part completes the volume; but it is intimated that a supplement is to follow, dealing with the new species recently collected by Mr. H. O. Forbes in New Guinea, and containing an account of the fertilization of *Ficus Roxburghii*, by Dr. D. D. Cunningham. A photograph of a tree in fruit of this remarkable species forms the frontispiece to the volume. It is one of those species which bear the fruit on the trunk; and in this particular tree the fruit is heaped up around the base of the trunk in such profusion as to suggest the idea of its having been placed there. Several species of the section *Covellia* exhibit this peculiarity; and some even go farther and bury their fruit in the ground, where it ripens, like the earth-nut *Arachis hypogæa*. *Ficus conglobata*, King, and *F. hypogæa*, King, belong to this group. The former bears enormous clusters of figs which are wholly or partially buried in the soil; and of the latter, Mr. H. O. Forbes, who collected it in Sumatra, at an altitude of 5000 feet, says "the fig-bearing branches issue from the stem very near the ground, and at once become sub-terrestrial, producing figs either entirely or partially buried. These figs when very young are devoid of colour in the upper half, but pinkish in the lower half. When a little older they become reddish-pink all over, and when mature they are of a greenish-grey colour."

Another highly curious species is *Ficus Minahassæ*, Miquel, a native of the province of Minahassæ in the Celebes. Miquel truly says this is "Omnium Ficuum maxime singularis." The figs (receptacles) are only from one-tenth to one-fifteenth of an inch in diameter, crowded together in little balls, about an inch in diameter, and borne on long slender pendulous leafless branches.

Ficus hispida, Linnæus, is one of the commonest species throughout tropical Asia, and extends to North Australia and Hong Kong. It is also very variable, the variability being in a great measure due to the different situations in which it grows. This species bears the receptacles (figs) in pairs in the axils of the leaves, or in clusters on the trunk, and sometimes they occur in both positions on the same tree at the same time. The fruit from the trunk sometimes burrows in the ground, and Roxburgh seems to have been the first to record the phenomenon. On the sandy lands near the sea on the coast of the Tanjore country, he states, the whole raceme and fruit are often entirely underground. Whether it was for this reason or not that this variety received the name of *F. Dæmonum* is not mentioned, but Vahl gave this name to specimens collected by Kœnig.

With regard to the execution of the second part of Dr. King's monograph, more especially the lithographs, the work of native artists, there is, if anything, an improvement on the first part; and the dissections are throughout on the same plate as the species to which they refer (instead of on separate plates as in the first part), which is much more convenient. The total number of species described is 207, many of them new; though, on the other hand, Dr. King has reduced a very large number of species founded by other authors, especially by Miquel, on fragmentary herbarium specimens. In several instances two species were founded on the different sexes of the same species, based upon differences in the shape and other characters of the receptacle. On this point it may be mentioned that Count Solms's and Fritz Müller's investigations of the sexual relations of the fig and caprifig, and the investigation of other species by the former, had led botanists to expect greater diversities in the male and female receptacles than would appear from Dr. King's researches to exist. He says:—"In by far the majority of cases these two kinds of receptacle [*i.e.* male and female], so physiologically distinct, are undistinguishable by external characters, and they are borne by the same individual plant. They look exactly alike until one opens and examines their contents. The most notorious of the few exceptions to this rule is the common eatable fig (*Ficus Carica*)." These differences have been fully discussed from time to time in NATURE. What is more surprising than this sexual similarity is that in certain species having dimorphic receptacles, the dimorphism Dr. King finds bears no relation to the separation of the sexes. For example, *Ficus trachycarpa*, Miquel, having spherical verrucose receptacles, Dr. King considers to be the same species as *F. clavata*, Wallich, which has larger ovate or obovate receptacles; and he says:—"There is no absolute sexual relation between the external form and the contents of the two kinds of receptacle which occur in this species; but, so far as I have observed, the large obovoid clavate receptacles invariably contain male and gall-flowers, and the males are not confined to a zone near the mouth, but are to be found on all parts of the interior of the receptacle. Of the small subglobular receptacles, however, some are exclusively filled with fertile female flowers, while others (like the large clavate receptacles) contain males and gall-flowers mixed together." One would almost suspect an error in the identification of these two forms as one species, though in foliage they are absolutely alike.

But, apart from all physiological considerations, it is a matter for congratulation that this useful work has been completed, and Dr. King has earned the thanks of all his fellow-botanists.

W. B. H.

OUR BOOK SHELF.

Questions and Examples on Elementary Experimental Physics. By Benjamin Lœwy. (London: Macmillan and Co., 1888.)

THIS book contains some 470 questions and examples in elementary physics, selected from the various papers set by the author for the examinations of the College of Preceptors. The questions are arranged under the four sections, sound, light, heat, and electricity and magnetism, and are further subdivided in each section into groups of five or six, with the suggestion that each group should form the subject of an ordinary school lesson. Problems involving a knowledge of mathematics beyond elementary arithmetic and geometry are avoided; in other respects the general standard of the questions is about that of the advanced stage of the Science and Art Department's examinations. The questions are well selected, and free from ambiguity or repetition. We notice under Heat, ix., 1, the question: "In the process of graduating a thermometer, why must the freezing-point be determined before the boiling-point?" This is the order of operations as usually given in the text-books, but it has been shown in the elaborate report of the Bureau des Poids et Mesures that the interval between the freezing and boiling points is most constant when the freezing-point is determined as soon as possible *after* the boiling-point.

We can recommend this little book to the attention of those teachers who have to prepare pupils for the public examinations in elementary physics. For success in such examinations it is not sufficient that the pupils should possess the requisite amount of knowledge: they must also acquire the power to express their knowledge clearly and concisely on paper, and such power it is one of the functions of such a book as this to impart.

H. H. H.

The Unknown Horn of Africa. By F. L. James, F.R.G.S. (London: G. Philip and Son, 1888.)

THIS is an extremely interesting record of an exploration from Berbera to the Leopard River, undertaken about four years ago. Various attempts had been made, before Mr. James's journey, to penetrate to the interior of Somali Land, but without success. Mr. James and his companions, more fortunate, or more skilful, than their predecessors, contrived to push their way to the goal for which they started; and the result is that the present volume is accompanied by a map embodying much new information regarding a district of considerable extent and importance. Some of the difficulties encountered by the party were formidable, but courage and perseverance enabled the travellers to overcome every obstacle. Mr. James has much to tell us about the flora and fauna of the country, as well as about its physical features; and he has many lively and instructive passages describing his relations with the natives, whose peculiarities he seems to have thoroughly understood. The story is itself so interesting, and is told in so bright and pleasant a style, that the book ought to be one of the most popular of recent works of travel. It is illustrated by a number of excellent coloured plates, and by various effective pictures, composed from photographs of natives and native scenery taken on the spot.

Seas and Skies in Many Latitudes. By the Hon. Ralph Abercromby. (London: Edward Stanford, 1888.)

THIS is not an ordinary book of travels. It has been written mainly for the purpose of calling attention to such phenomena of the sky and weather as Mr. Abercromby has observed in various parts of the world. The opening chapter describes the author's experiences in Canada and the United States in the year 1865. Then he gives an account of a voyage round the world, beginning with what he saw in Egypt, and passing on to descriptions relating to Australia, New Caledonia, Fiji, New Zealand, Cape Horn, and Rio Janeiro. Mr. Abercromby next takes his readers within the Arctic Circle, and afterwards he tells of a long journey, in the course of which he was at the Cape of Good Hope, Mauritius, Ceylon, the Himalayas, Borneo, Manilla, San Francisco, and Washington. He by no means confines his narrative to matters specially attractive to meteorologists. He takes interest in many different classes of subjects, and has something more or less memorable to record about almost all the places he has visited. It is, however, meteorology that he keeps chiefly in view, and we need scarcely say that on this subject, which he has so long and carefully studied, his book is always fresh and instructive. The value of the work is increased by good maps and illustrations.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Alpine Haze.

WITH the caution of a true man of science, Prof. Tyndall has given this name to a phenomenon observed by him in the Alps. Does not W. Clement Ley beg the question by calling it "dust-haze"? I should translate his *nebula arida* by "dry haze."

Two hundred years ago, Ludolf gave the best definition of *qobar* by translating it: "*opacitas aeris qualis solet esse tempore fervidissimæ æstatis.*"

In my last letter I quoted several names of it, in order to show that the vulgar eye has long distinguished this phenomenon. I have since learnt that, in the Basque dialect of Gipuzkoa, its proper name is *bisuntsa*, but that seafarers call it *lur-autsa, i.e.* "earth-haze," *lañoa*, meaning common fog. The Ethiopic name, *qobar*, comes from *qabbara*, a root extant also in Hebrew and Arabic, and meaning *to bury*. *Qobar* hides the landscape, and conceals stars of the third magnitude, even in the zenith. Gasparin observed it on Mount Ventouk, where he crossed a thick cloud which made no impression on his hygrometer. Humboldt, viewing *qobar* in Peru, says, "Quelle est cette vapeur qui est visible et qui ne mouille pas?" but leaves his question unanswered.

While travelling in Spain, Willkomm remarked *qobar* at a distance of 3 or 4 miles, yet, on reaching the actual spot, he saw nothing. He clearly distinguished it from the *landrauch* ascribed to smoke caused by turf burning in Westphalia, and thinks, like Spaniards, that *callina* increases with solar heat. Several German authors have spoken of this phenomenon as smoke, but Egen is the only one who has followed it up from place to place through an extent of 200 kilometres, and rendered it probable that it then covered a space of more than a thousand square myriametres. It seems, however, that particles of smoke should attract moisture, if there were any in the air, and then form real clouds or otherwise fall to the ground by increase of weight.

Bravais saw *qobar* on the Faulhorn, when his hygrometer was at 51, air saturated with moisture marking 100. In Ethiopia, where I have observed it so low as 20, the hygrometer's mean reading was 41 when *qobar* was conspicuous. Above 72 it disappeared. These figures apply to the place of observation. Conclusions can be safely drawn only when the air's moisture shall have been measured in several places along the line of sight up to the spot where *qobar* prevails, or, better still, in that spot itself when recognized from a distance.

Since I published my first account of *qobar*, Martins, who observed it in Auvergne, Switzerland, and from Mount Canigou, is the only author who has specially described it. He says that the Swiss call it *hâle*, and that he saw none of it in Lapland. In spite of Kaemtz's remark that, moisture being the essence of all fogs, a "dry fog" is an expression not less un-

meaning than a "triangular square," Martins divides his "*brouillards secs*" into four classes, viz. volcanic ashes as seen in the year 1783; smoke from turf or stubble fires; *callina* or *qobar;* and a fourth kind established on negative evidence which seems untrustworthy.

W. Clement Ley has described quite well the hues of *qobar*. It is light buff when near or slight. Otherwise, its colour is a lurid gray verging to blackness. Whatever may be its connection with cumuli in England, I could detect nothing of the sort in Ethiopia, where I have watched *qobar* for whole weeks without any ensuing rain or even cloud.

Four years ago the French transit of Venus expeditions agreed to investigate the amount of carbonic acid gas in the air of their several stations. Mine was in Hayti, where *qobar* was rife, and while observing for many consecutive hours the passage of air through caustic potash in prepared tubes, I regretted their not being made to receive plugs of loose cotton in order to collect smoke, dust, or microbes. All the tubes having been subsequently tested in Paris by Prof. Müntz, he obtained the unexpected result that air contains more carbonic acid in the southern hemisphere than on the north of the equator. Those tubes inclosed also fragments of pumice-stone previously steeped in sulphuric acid in order to collect moisture. With a little care and trouble next summer in a spot of Southern Europe where *qobar* abounds, meteorologists might soon get an insight into its true nature.

January 3. ANTOINE D'ABBADIE.

SEVERAL communications have appeared in NATURE on the subject of atmospheric haze. It would be interesting to know whether the writers consider the haze which they have described as identical in substance with that which I would call *ordinary* atmospheric haze. The haze of these writers is a haze taking the visible form of layers or bands. The haze to which I refer has under ordinary circumstances no visible form at all. We are conscious of its presence by its effect in diminishing the transparency of the air. Everyone knows that, quite apart from fog, or smoke, or dust, or low cloud, or falling rain, the transparency of the air varies very greatly at different times. In our climate there is nearly always more or less of atmospheric haze, the rare exceptions proving the rule; and the haze may be so dense as to render terrestrial objects invisible at a distance of a very few miles. Celestial objects may also be obscured by the same cause. Not to speak of the varying brightness and varying colour of the sun at sunset (in the production of which effects another cause may co-operate), there are occasions on which the sun long before sunset is shorn of his beams through the intervention of a low general haze, the hygrometric conditions at the time being such as to preclude the idea of fog, to which indeed the haze referred to bears little resemblance.

On July 24, 1868, I witnessed from the summit of Snowdon a curious effect of this diffused haze. The day was cloudless. Overhead the sky was clear and blue, but at lower altitudes it was hazy, and the haze gradually thickened towards the horizon, where it terminated in an opaque brown ring, which encircled the mountain and shut out from view all objects beyond a distance of about 15 miles.

The nature of atmospheric haze has not, I think, hitherto been satisfactorily elucidated, and it is much to be desired that advantage should be taken of some occasion when the haze is exceptionally dense, for the application of the various methods of research which modern science has rendered possible.

Clifton, December 25, 1888. GEORGE F. BURDER.

On the Use of the Words "Mass" and "Inertia"—a Suggestion.

As a teacher of dynamics to Engineer Students, I followed with interest the discussions in NATURE, as to the use of dynamical terms, that have taken place within the last two years, and have recently re-read the whole correspondence with care. Two points seem to me to have been not quite sufficiently brought out.

(1) Physicists and teachers of dynamics, however careful they may desire to be, use the word "mass" in two senses: (1) in the old, non-scientific, (Johnsonian) sense of a "lump of matter," and (2) in the precise scientific sense of the "inertia" of a lump of matter. Indeed, I suppose that no scientific man would hesitate to speak of "the inertia of a mass of matter."

The phrase "attracting mass" is universal among scientific men, when attracting "lump" would do just as well. Thus, in Prof. MacGregor's very carefully written "Kinematics and Dynamics," we find, in Art. 290, "mass" carefully defined (in the sense of inertia) as the value of a certain ratio, and in the next article the use of the word in the sense of quantity of matter is deprecated; yet, in Art. 355, we have "attracting mass" where attracting "inertia" would not do, followed, a few lines further on, by "a particle of unit mass" where "unit inertia" would serve as well.

It is this double use of the word that, I think, sometimes escapes Engineers.

Each of the words "mass" and "weight" is used in two senses, one of which is common to both, but the other not. The fact confirms very strikingly Prof. Greenhill's contention that the scientific man is unwise to attempt to limit for his own purposes the signification of a word already well established in the language. For it shows that he cannot even keep straight himself. I think myself that the scientific man ought to back out with as much grace and celerity as may be, and determine for the future to say "inertia" when he means "inertia," and to use for its numerical representation the symbol "i" (or perhaps "s"—sluggishness) rather than the symbol "m." The symbol "I" might still be used for *moment of inertia*. Such an expression as a "mass of 20 pounds" would still mean exactly what it does at present, and nothing already written would be affected by the change.

(2) The second point that I have to mention is purely a question of procedure in teaching.

The whole subject of dynamics might well be termed the study of the inertia (the "sluggishness") of matter. This is the one new property whose existence, signification, and measurement has to be brought home to the student. Now, I would urge that it does not seem reasonable to ask the student simultaneously to comprehend a new property of matter and to alter his unit of force by defining it with reference to the new property. Do what we will, our students before they begin to learn dynamics will be familiar with the notion of "force" as a "push" or a "pull," and measured in terms of "pounds" and "ounces."

I think it would be far the best plan to define the British unit of force as the weight *in London* of the standard pound lump, and the unit of inertia as that of the mass or lump on which this force generates the unit acceleration of 1 foot-per-second per second.

Thus the unit of inertia would be that of 32·1912 standard pounds, the number 32·1912 being, for brevity, throughout the teaching, written "g."

This would be to adopt with careful definition, by which it is rendered perfectly precise, the Engineers' unit of inertia for purposes of instruction in dynamics. It means empoying a *force-time-length* system of units instead of an *inertia-time-length* system.

Such a system would be in harmony with the order of our experiences and of our ideas as we grow in intellectual stature, and with the history of human thought as written in our language, and it is unwise to wage war with our own past even under the encouraging leadership of your correspondent "P. G. T."

Perhaps I may be allowed here to deprecate the somewhat misleading effort now being made by some chemists and physicists to substitute the word "mass" for "weight" where no question of inertia is involved or dreamt of, as, for instance, in the definition of specific heat, by reference to equal masses, instead of equal weights, as if the idea of quantity of matter had not been attained quite independently of the conception of inertia, and were not in the case in question always determined by weighing. A. M. WORTHINGTON.

Royal Naval Engineer College, Devonport,
December 30, 1888.

Eight True Ribs in Man.

IN the number of NATURE which appeared on November 1, 1888, there is a notice to the effect that "at one of the meetings of the Anatomical Society, during the session of the Medical Congress in Washington, Dr. Lamb, of the United States Army Medical Museum, spoke briefly of a singular phenomenon he had observed in his examination of human breast-bones. It was the occurrence, in a number of specimens, of an eighth rib,

the cartilage that is usually found below the seventh rib being fully developed into a rib." This description is somewhat ambiguous, but I presume it refers to the occasional elongation of the eighth costal cartilage in man, and its direct articulation with the sternum. At the time when I read this notice I was organizing a system of "collective investigation" in my class of practical anatomy, in Trinity College, Dublin, and I asked Mr. O. L. Robinson, one of my assistant demonstrators, to undertake the investigation of this point. During the last two months he has examined thirty subjects, and he has found the eighth costal cartilage united to the sternum in no less than five of these. In four subjects (two males and two females) the eighth cartilage of the right side alone showed this condition; in the remaining case (a male) the eighth cartilage on each side reached the breast-bone and articulated with its fellow in front of the upper part of the ziphi-sternum. The anomaly is therefore one of some frequency, seeing that it has been noted by Mr. Robinson in about 17 per cent. of the subjects which he has examined. I may mention that all the specimens are now in my possession.

But my object in making this communication is not so much to record the results obtained by Mr. Robinson, seeing that these will be more fully dealt with elsewhere, as to call attention to a remarkable statement which is made by NATURE on the authority of *Science*. It runs as follows:—"Dr. Lamb has made a thorough search of anatomical literature for references to this peculiarity. In the English books there is only a single incidental reference to it, and in that case the author does not say that he has ever seen a specimen. In German books there are two references. one of them being the one mentioned by the English authority." Certainly this is not my experience of the literature on this subject. There is hardly a German text-book of importance in which the anomaly is not referred to.[1] Thus Gegenbaur, Aeby, Luschka, and Henle, all mention it, and the two last enter into the question at some length. Henle likewise quotes the observations which have been made upon this point by Hyrtl and Prof. Oehl of Pavia. It is true that our own text-books are for the most part silent on the subject, but Prof. Humphry in his classical work on the human skeleton, p. 323, remarks: "In a specimen in the Cambridge Museum, which measures seven inches, there are eight cartilages of ribs separately united to the sternum." This is not the description of a man who has never seen such a specimen.

It is interesting to note that, of the ten cases recorded by Dr. Lamb, nine occurred in Negroes and one in an Indian. Luschka, referring to the assertion that the anomaly is more frequently observed in black races, says: "Im Verlaufe weiterer Nachforschungen hat es sich jedoch herausgestellt, dass bei den Negern nicht häufiger als bei anderen Menschenstämmen und immerhin nur in Ausnahmsfällen acht Rippenpaare an das Brustbein angeheftet sind" ("Die Anatomie der Brust," 1863, p. 119).

Another feature of interest in connection with this anomaly is centred in the fact that in the lower apes, and also in the chimpanzee, it is the typical condition. As a rule, they present eight true ribs on each side. The orang, however, resembles man in this respect, and normally possesses only seven true ribs. Curiously enough, the transition stage between man and the orang on the one hand, and the chimpanzee on the other, is to be found in the gibbon. In this ape, so far as my experience goes, the cartilage of the eighth rib, although it is long and rests by its tip against the ziphi-sternum, does not articulate with the sternum. A condition similar to this is occasionally seen in man. D. J. CUNNINGHAM.

Trinity College, Dublin, January 2.

"The Cremation of the Dead."

IN your excellent article on the above subject (p. 219), it is stated that a provision contained in the will of a testator directing the cremation of his remains has no legal effect. This is no doubt correct, for, although the law permits a man to dispose of his property by will, it does not permit him to dispose of his own corpse.

This legal difficulty may, however, be surmounted by an indirect method. Most testators bequeath legacies to their executors, and also to their nearest relatives; and the legacies bequeathed to the latter are not unfrequently of considerable value, even when the testator is a man of only moderate means. If, therefore, each of the legacies are made conditional upon the legatee taking, or concurring in taking, the necessary steps to procure the cremation of the testator's remains, the wishes of the latter would in the majority of cases be carried into effect; since any attempt on the part of a legatee to interpose any obstacle would involve the forfeiture of his legacy. A. B. BASSET.

Conservative Club, S.W., January 6.

"Degradation" of Energy.

IT may perhaps have occurred to others besides myself that the term "degradation"—as applied to the transmutation, for instance, of mechanical energy into heat energy—is a rather stronger one than our present knowledge warrants us in using: that it casts, in fact, an unmerited slur on the character of that eminently respectable concept, energy. We seem hardly justified in supposing that any *intrinsic* deterioration of the energy takes place in such transmutations as the above.

Might not "depreciation" be a rather preferable expression? This would imply nothing more than a lowering in the value of energy in relation to the particular needs and mere agencies of man, and not any absolute change in its character for the worse.

Similarly, money securities are said to be "depreciated" in a particular market, while they may not be at all lessened in absolute value. H. G. MADAN.

Eton College.

Hares Swimming.

IN *Chatterbox* of May 12, 1879, published by Wells Gardner, Paternoster Buildings, is an account by J. G. Fennell of several instances in which he has seen hares swim across both fresh and salt waters. OCTS. DEACON.

Loughton, Essex, January 5.

THE RECENT SOLAR ECLIPSE.

WITHIN the next few days we may expect detailed news of the various parties organized to observe the eclipse of January 1. In the meantime, the following telegram from Mr. Pickering, chief of the Harvard Eclipse Expedition, who was at Willow, California, will be read with interest:—

"The sky was clear during the whole of totality. The corona was larger and more irregularly shaped than usual, exhibiting great detail in its filaments. Three of the geometric contacts were observed. The duration of totality was 118 seconds, or three seconds longer than had been predicted.

"Capital drawings were obtained of the whole corona. Eight negatives were obtained with a 13-inch refracting telescope, and six with an 8-inch telescope, and seven photometric observations were made of the corona's light. The drawings show the corona extending outwards from the sun for two of its diameters—that is, 2,000,000 miles in both directions. The corona somewhat resembled that of the eclipse in July 1878."

Another account of the doings of Prof. Pickering's party states as follows:—

"During yesterday's eclipse of the sun twenty-five negatives were taken at Willow, California, to measure the brightness of the corona and its surroundings. Five of these were obtained to search for intra-Mercurial planets, and twenty to study the spectrum of the corona in order to determine its composition. These negatives will reach from the yellow rays to the extreme ultra-violet.

"The general illumination during the period of totality was found to be lighter than during the eclipses of 1878 and 1886. The corona was similar to those of 1868 and 1878, but showed much more detail than the latter, and was exceptionally fine, extending usually on one side to two solar diameters. A striking characteristic was two forked wings of light. The polar rays were well defined and con-

[1] It is not referred to by Krause, Hermann Meyer, or Pansch, but in these books, as in our English text-books, the omission is evidently due to want of space and not to want of knowledge.

siderably shorter. At Cloverdale, the eclipse was observed with great accuracy, but shortly before totality some cirrus clouds passed over, all tinged with the most brilliant colours of the rainbow.

"Venus appeared during the early stages of the eclipse, while Mercury and the other planets were plainly in view during every phase that was photographed.

"At Willow, the temperature dropped 7°, but the fluctuations of the barometer were quite imperceptible. The velocity of the wind diminished at first, but afterwards increased."

Prof. Todd secured a number of fine photographs of the corona, showing, according to Reuter's telegram, rays extending 10° or 12° from the sun.

At Winnemucca, the United States Signal Service observers made drawings of the streamers of the corona, and also took successful photographs. They saw the edge of the moon projected against the corona for some time after the total phase had passed.

At Lick Observatory, the eclipse was successfully observed, and a number of photographs were taken.

At Norman, California, the fourth contact was observed, but the first was lost in clouds. The moon's limb was not seen projected on the corona either after or before totality, although careful search was made. The telescope was used for making drawings of the corona adjacent to the sun's poles, and the sketches show very complex filaments. The negatives taken are excellent, and show the corona very similar to that seen in 1878. Long streamers were readily traced through 4°.

The party of observation at Bartlett Springs report that the corona was beautifully distinct, and that they saw remarkable changes in the length of the coronal lines. They obtained nine photographs of all contacts, studied the structure of the inner corona, and made measures of light intensity during totality.

Four long streamers were seen proceeding from the prominences, and the chromosphere was strong for a full quadrant distance of the west side of the sun. The northern and southern limbs of the sun showed a great number of fine radiating filaments.

At Chicago, a beautiful view of the corona was obtained. Two long streamers pointed nearly west, and two shorter ones were almost opposite. At the beginning of totality intense red flames burst out on the sun's western side, covering a space of 90°. The inner corona presented a beautiful spectacle in the telescope. Its radiating filamentary structure, with both curved and straight lines, was distinctly seen.

At Healdsburg, although only nineteen-twentieths of the sun's surface were obscured, Venus, Mars, Jupiter, Mercury, and the principal fixed stars were visible. The corona also appeared with long rays of light parallel to the sun's equator.

Mr. Swift, Director of the Warner Observatory, stationed at Nelson, California, reports that, as far as it afforded an opportune search for an intra-Mercurial planet, the eclipse was a failure, owing to clouds and haze.

At Anaheim, no photographs were secured, but it is claimed that an intra-Mercurial planet was seen during the period of greatest obscuration.

At Winnemucca, Nevada, one observer discovered a comet near the sun. No appreciable change of temperature was noticed at this station. Accurate observations were also made of the shadow bands. The corona was similar in general appearance to that of 1878. The streamers extended to a distance of from three to four diameters, and the red protuberances were strongly marked.

At Grass Valley, during the period of totality, the stars and large planets were seen with the naked eye, and the corona and protuberances offered a grand spectacle. The thermometer fell 7° between the moment of first contact and totality. At Virginia City, Nevada Territory, the thermometer fell 10° during the progress of the eclipse.

At Blackfoot, Idaho, all four contacts were observed. A short time before totality the moon's limb was seen projected against the corona. The mercury fell 13°.

From the above accounts it is quite clear that new information touching many important points has been secured. This is most fortunate, for the eclipse occurred at a well-marked minimum of solar spots; indeed, it was as marked as that of 1878, when again the eclipse swept over the American continent and was most fully observed.

There seems no doubt that the expansion of the sun's surroundings in the plane of its equator, dwelt upon strongly by Newcomb in his account of the eclipse of 1878, has been re-observed. We read that this ring was seen to extend some 2,000,000 miles on either side of the sun, and to put on the appearance of two forked wings of light. Not only in 1878 was this ring-like extension well marked, but, in consequence of the extreme quietude of the sun's atmosphere at the time, the exquisite structure of the atmosphere over each pole was one of the most striking features of the eclipse. The appearance was produced by the structure of the coronas bending gracefully over from the sun's axis prolonged, that nearest the pole bending least. This or something very like it has evidently been again seen, and the photographs which have been taken by Mr. Pickering's and other parties will evidently give a good account of them.

It must be noted, too, that the American astronomers have, as might have been expected, used large telescopes. We read of 13-inch and 8 inch refractors. Nothing so large as this has ever been employed before in eclipse expeditions, but then the parties this time have not been far from their base. In one of the telegrams it is stated that Mr. Pickering's party secured twenty spectrum photographs of the corona. This, perhaps, is the best news of all; and we read, too, that the less refrangible end of the spectrum has not been neglected.

Considering the short duration of totality, the results secured reflect the highest credit upon the organizers of the parties and upon the individual observers.

RECENT WORKS ON ALGÆ.[1]

PROF. ASKENASY tells us, in a brief preface, that the Algæ collected during the voyage of the Scientific Expedition in the *Gazelle* were intrusted to him for examination, and that in the work he was assisted by Herr Moebius, by whom the greater part of the excellent figures in the plates were drawn. The remainder of the figures, with the exception of those in Plate I., were drawn by the editor, thus affording an apt illustration of the great advantage to naturalists of acquiring facility in drawing.

In the determination of the Algæ, Dr. Askenasy had the assistance of MM. Bornet and Hariot; the Characeæ and Conjugatæ were described by Dr. Otto Nordstedt, by whom the well-drawn figures in Plate I. were executed. Herr Grunow described the Cystophyllum and Carpophyllum, and the difficult genus of Sargassum.

The only new plant among the Confervaceæ is *Anadyomene reticulata*, Askenasy, from the Island of Dirk Hartog, in West Australia. The Characeæ, now included among Algæ, are rather numerous; among them are two new species of Nitella.

[1] "Forschungsreise S.M.S. *Gazelle*," iv. Theil-Botanik, Algen; mit Unterstützung der Herren E. Bornet, A. Grunow, P. Hariot, M. Moebius, O. Nordstedt. Bearbeitet von Prof. Dr. E. Askenasy. Mit 12 Tafeln. (Berlin: Ernst Siegfried Mittler und Sohn, 1888.)
"Om structuren hos Champia och Lomentaria," med anledning af nyare tydningar. Af J. G. Agardh, Öfversigt af Kongl. Vetenskaps-Akademiens. Förhandlingar, 1888, No. 2. (Stockholm.)
"Fresh-water Algæ collected by Dr. Berggren in New Zealand and Australia." By Otto Nordstedt. With 7 Plates. Communicated to the Royal Swedish Academy of Science, June 1887. (Stockholm, 1888.)

Dr. Askenasy prefaces his description of Halimeda by remarking that the structure of the genus has not been hitherto described. From this it would appear that Dr. Agardh's observations on Halimeda, contained in Part V. of his work, "Til Algernes Systematik" ("On the Classification of Algæ"), is still unknown at Berlin. It may be mentioned that, while the title is in Swedish, the work is in Latin.

The remarks of Dr. Askenasy are, however, not the less welcome, illustrated as they are by the figures in Plate IV. One new species, *H. macrophysa*, is described. It is to be regretted that so little is as yet known of the fructification of these plants.

One new species of Caulerpa, *C. delicatula*, allied to *C. Brownii*, is added to the sixty-seven species of this genus already known to science.

The Ectocarpeæ are carefully worked up, and one new species from Kerguelen's Land, *E. Constantiæ*, has been added by Dr. Hariot.

Perhaps the most interesting part of the work is the result of Dr. Grunow's study of the genus Sargassum. Every algologist is aware how difficult it is to identify the specimens, often very fragmentary, of this plant, which lie before him for examination. Fortunate is the collector who obtains a whole plant of Sargassum, comprising root and lower leaves—which often differ materially from those in the upper part of the plant—branches bearing leaves only, and other branches bearing fruit and vesicles as well as leaves. It is owing to this fragmentary state of the plants that the published descriptions are frequently defective.

Dr. Grunow has done much to elucidate the life-history of the genus Sargassum by his discovery that some species are monœcious, and others diœcious. In describing *S. Carpophyllun* (see "Voyage of the *Novara*," p. 56), Dr. Grunow mentions the occurrence on the same plant of two kinds of fruit—namely, short receptacles which correspond with those described by Dr. Agardh, and also linear receptacles three-quarters of an inch long. He, however, makes, in this work, no further observations on the subject.

In the present work he merely mentions that the plant is monœcious. It will be seen from the descriptions of the other species in the text that Dr. Grunow has been able to prosecute successfully his researches on the fruit of the Sargassa. In most cases it is indicated in the text whether the species are monœcious or diœcious. It seems to be ascertained that the smaller kind of fruit contains spores; while the antheridia are contained in receptacles nearly twice the size of the former. There also exists much diversity in the form of the receptacles belonging to the same species. The spore-bearing receptacles are sometimes forked or spiny, while those bearing antheridia are simple, smooth, and cylindrical. The list of Sargassa in the present work contains twenty-eight species and varieties. In the case of the varieties Dr. Grunow is careful to mention in what respects they differ from the original species.

As to *S. bacciferum*, it is mentioned in the text that its history is still insufficiently known. The editor refers to the pelagic specimens, called "gulf-weed," which float for a time without root or fruit, and subsequently decay; but he does not seem to be aware the *S. bacciferum* was found by Mr. Moseley,[1] during the voyage of the *Challenger*, growing plentifully and full of fruit on rocks in Harrington Sound, Bermudas. The *S. bacciferum*, var. *foliifera*, also bears fruit.

It may be remarked that while there is a general impression that no parasitic Algæ are found growing on gulf-weed, Dr. Askenasy met with a specimen among the Algæ brought home by the *Gazelle* on which were growing a *Rivularia* and a *Calothrix*, and that other epiphytic Algæ were found on the same species by Martens.

[1] See extract from Mr. Moseley's letter, dated June 27, 1873, in Dr. Dickie's paper on the marine Algæ of St. Thomas and Bermuda, Journal of the Linnean Society, vol. xiv.

Among the Rhodophyceæ of this collection, there is one new genus, *Episporium centroceratis*, Moebius. It is from West Australia, and is classed with the Cryptonemiaceæ. The new species are four in number—namely, *Hildebrandtia Lecannellieri*, Hariot; *Chaukausia Naumanni*, Askenasy; *Rhabdonia decumbens*, Grunow; and *Sarcomenia intermedia*, Grunow. Among the rarer species are *Corynospora Wüllerstorfiana*, Grunow; *Ptilota Eatoni*, Dickie; and *Marchesettia spongioides*, Hauck. A plentiful harvest was obtained of the beautiful and very rare Nitophylla and Delessericæ of the Southern Ocean.

The attention of algologists will be drawn to the minute and careful analytical descriptions of many species of the Florideæ. Great pains have been bestowed by Dr. Askenasy on the description of some species of Galaxaura: the more delicate parts of these plants, he observes, have not been described. Fruit is rare, and but imperfectly known in this genus. It may be observed that Dr. Askenasy's classification of Galaxaura differs from that of Agardh ("Til Algernes Systematik," Part vii., Florideæ). By the former it is placed among the Chætangiaceæ; the latter retains it among the Helminthocladieæ.

Dr. Askenasy gives an elaborate description of that singular production of Nature, *Marchesettia spongioides*, Hauck. He mentions it as "this plant or organism," for it seems to be between a Sponge and an Alga. It was known imperfectly to Semper and Esper. Dr. Hauck, who had found it among the Sponges in the Museum at Trieste, announced that it was an Alga, which belonged to the Florideæ, and to the group of the Areschougieæ ("Su un nuovo caso di simbiosi," *Atti del Museo Civ. di Stor. Nat. di Trieste*, 1884). In external appearance this "organism" resembles a branched Sponge; the Alga being entirely inclosed within it. Dr. Askenasy has devoted one whole plate (Plate XII.) to illustrate the Marchesettia. The fructification is at the end of the branches. Tetraspores were seen by Dr. Hauck and by Dr. Askenasy, but it is not mentioned whether they were cruciate or zonate. Dr. Agardh observed cystocarps, which he thought approximated to those of Rissoella.

On the inside of the "organism," and among the branches of the Alga, Dr. Askenasy found, in the specimens brought home by the *Gazelle* Expedition, and preserved in spirit, a slimy substance, like that which constitutes the life of a sponge, and he is decidedly of opinion that *Marchesettia spongioides* is, to use his own words, "eine symbiose zwischen einer Floridee und einer Spongie darstellt" (p. 41). Marchesettia is a native of Madagascar, Singapore, the Philippines, and New Caledonia.

The external resemblance of the Marchesettia to a species of Thamnoclonium of the section Dictyophoræ is so great that Dr. Agardh gave to the latter the name of *Th. Marchesettioides*. The plants, when in fructification, may, however, be always easily distinguished, the fruit of the Thamnoclonium being contained in leaflets, which spring from the sides of the plant.

This is a very useful work to algologists; but its utility would have been increased by the addition of an index. It may also be observed that the size of the type might have been enlarged with benefit to the eyes of students.

The subject of Dr. Agardh's essay is the structure of Champia and Lomentaria. He observes that within the space of little more than a year, four special essays—two by N. Wille, one by F. Debray, and another by R. P. Bigelow—have been published with a view to demonstrate the structure of these well-known plants, and that these publications give an entirely erroneous representation of the entire development of these Algæ. He therefore thought it expedient for him, who had published ten years ago a very different description, to state his opinion on

these new works and his own views on the subject. This he has done in the present essay.

The descriptions referred to by Dr. Agardh appeared in his work entitled, "Florideernes-Morphologi" (published in the Transactions of the Royal Swedish Academy in 1879); but as this work was written in Swedish, it has probably not met with so many readers as it deserves. With a view to make this work more accessible, Dr. Agardh issued, in 1880, a Latin translation of it.

After stating the views of the essayists, and commenting on them, he quotes the concluding words of Mr. Bigelow, the most recent of them: "We have to leave our subject for the present in an unsettled and therefore rather unsatisfactory condition."

Dr. Agardh then quotes from his Swedish work the description of the structure of Lomentaria and Champia, showing that in the young state the interior is never hollow, but is interlaced with delicate coloured filaments, which disappear in the older parts of the plants. He also mentions that some Florideæ, which are apparently hollow, such as Chrysymenia, Dumontia, &c., are in part filled with a gaseous fluid, which probably assists such plants as have thin walls in preserving an erect position.

Dr. Otto Nordstedt is already well known to British algologists by the specimens of fresh-water Algæ which he has issued in conjunction with Prof. Wittrock, of Stockholm. The work he now sends us proves that he is a good draftsman and linguist, as well as algologist. It is on the fresh-water Algæ of New Zealand, and is written in very good English, and carefully got up in every respect. The author mentions that the Desmids have received his greatest attention, and that comparatively little attention has been bestowed on the Phycochromaceæ; a few only have been taken from brackish water.

Dr. Nordstedt mentions that he has not met with any new genus of fresh-water Algæ, or with any genus not represented in Europe, with the exception of *Phymatodocis*, which, he tells us, occurs also in North and South America, and in Australia.

With regard to the localities in New Zealand where fresh-water Algæ are found, no one is better acquainted with them than Dr. Berggren, who had made an interesting collection, subsequently examined by Dr. Nordstedt, and included in the present work on these plants. Dr. Berggren's remarks, as recorded by Dr. Nordstedt, will be read with interest. He says:—

"The fresh-water Algæ in New Zealand do not, from several causes, occur so frequently as in the regions of the corresponding latitudes of the northern hemisphere. The ground, which is generally sloping, gives a rapid course to rivers and brooks, and the surface occupied by stagnant water, swamps, and bogs is not very extensive. The comparatively small number of water- and bog-plants growing sociably together (such as *Potamogeton* and others), which in the stagnant waters and marshy spots of Europe are favourable to the existence of the fresh-water Algæ, is of great consequence. The usually dry summer generally causes the draining of those lowland spots, which in the wet season (the winter) are swamps. Therefore the Algæ are more frequent in the damp and moss-grown localities of the mountainous regions in the Northern as well as in the Southern Island. In the rivulets from hot springs in the hot lake district in the Northern Island, the Algæ are especially Phycochromaceæ, but likewise Confervaceæ and Zygnemaceæ, to be found growing in great abundance."

Dr. Nordstedt mentions that from his examination of Dr. Berggren's collection, it appears that the swampy ground on the Canterbury Alps and the highlands round the Taupo Lake are the best localities, especially for Desmids.

The description of the New Zealand Algæ is supplemented by lists of a few fresh-water Algæ from Australia and the Hawaiian Islands. Then follow a list of the principal works consulted, and an index. The work is illustrated by seven plates, the figures of which are all drawn—and well drawn—by the author.

MARY P. MERRIFIELD.

THE JOURNAL OF MORPHOLOGY.[1]

THE year 1887 marked an epoch in the advance of natural science in America, as that in which the above-named journal made its appearance. The first number was not published until some months after the advertised time, but, once in circulation, it became clear to all that the delay was warranted by the eminently satisfactory result obtained. The journal was defined in the preliminary advertisement as one "devoted principally to embryological, anatomical, and histological subjects," it being stated that "only original articles, which deal thoroughly with the subject in hand, will be admitted to its pages." The three parts before us present in the aggregate 593 pages crown octavo, with thirty plates, and woodcuts interspersed with the text. Seventeen papers have in all appeared, and of these, seven or eight are devoted to embryology, with a total of 361 pages, and four or five to anatomy and histology, with that of 182 pages. One is purely experimental, and deals with the mental powers of spiders (37 pages), and another is largely palæontological (12 pages), while the three which remain (61 pages in all) are largely controversial. The illustrations are throughout most excellent, but it is surprising to what an extent the work in this department has been done in Germany, especially as it has resulted in "a great loss of time and inconvenience in supervision." We are assured, however, that "there is no remedy except in the employment of an expert lithographer, to work under our immediate direction." We sincerely hope the editors may soon see their way to the employment of such an one, for surely he is to be found in the United States.

It will be seen from the foregoing that while, in the early issues, all branches of animal biology have been represented, there is a marked preponderance of embryological literature; and, taking into account the share which the discursive papers contribute towards this subject, there would appear to be a predisposition in favour of the same. The study of embryology is one which lends itself, by virtue of its constitution, to the production of hypotheses and broad generalizations; and, in knowledge of the extent to which previous workers have often availed themselves of this, we are led to inquire how far the predisposition in question may be due to this cause. Certain of our American brethren are notorious for their power of accumulating superfluous detail. Publications could be cited in which the "padding" is inversely proportionate to the actual work done, and we would fain desire that the authors should work more and write less; indeed, the senior editor has acknowledged this. He writes: "Concentration is our need"; and further, "The inaccessibility of our literature—scattered as it is among the publications of so many societies and institutions, and mixed up with a mass of heterogeneous matter that has no value for a zoologist—is notorious." All this being so, it is not surprising that the editors have decided to issue the numbers only "as often as the requisite material is furnished."

In estimating the usefulness of a private journal such as this, especially when so largely devoted to the interests of a subject lending itself to broad generalization, we cannot refrain from deploring the tendency, elsewhere manifest, towards the introduction of a bias in favour of

[1] *The Journal of Morphology*, edited by C. O. Whitman, with the co-operation of E. P. Allis, Jun. (Boston: Ginn and Co.)

certain restricted lines of thought. We meet with indications of cliquism and faction, and it would not be difficult to show that, in respect to this and some other matters, such journals at times compare unfavourably with that of the Society, which is kept in check by freedom of discussion and by criticism at the hands of referees. Instances are not wanting to show that the elementary student, starting research, is at times turned adrift in the labyrinths of a highly-involved problem, before he knows with what he is dealing; and if, as has occasionally happened, he be working under the influence of a preconceived bias, mischievous results must accrue. Although, in the pages before us, no such instance as this is actually forthcoming, we are of opinion that, in respect to certain matters referred to below, consultation with specialists prior to publication, would have resulted in the withdrawal of heresies which constitute the only jarring element in this beautiful work.

The authors of the papers thus far published are, for the most part, men of established repute. For many of them we entertain a personal regard, and there are among their leading productions monographs that are continuations of those with which, under the old *régime*, they have honoured our national journals. Others there are with the preliminaries to which we claim a proud familiarity, and many of us look back with pleasure upon the fact that one of the original communications on the subject of the opening paper was made in our midst (cf. P.Z.S., 1886, p. 343). The journal makes its *début* under auspicious circumstances, and the above and other similar considerations show it to be the outcome of a growing want, in the unfolding of which we have ourselves had a stake; it is manifestly our duty, therefore, to support it.

The papers, when considered individually, must be declared of excellent merit, and, detailed criticism being here impossible, we pass to a brief comment upon those most conspicuous or most likely to raise discussion. Chief among all are the contributions of the senior editor. We defer mention of the more philosophic of these till the end of our notice; his "Contribution to the History of the Germ-Layers in Clepsine" (78 pages) is a marvellously-wrought piece of work; and, if his leading deductions concerning the fate of his "macromeres" be capable of support, he may well lay claim to a masterly stroke in advance. With his "teloblasts" there is initiated an entirely new line of inquiry, in itself refreshing after the ceaseless quibbles as to the fate of the "blastopore," while it gives promise of a direct and important bearing upon some of the most revolutionary of recent embryological discoveries (*e.g.* that concerning the part played by the ectoblast in the development of the excretory system). The author marshals his facts in faultless sequence: his monograph is a model of its kind, the more contemplated the more to be admired; and it fully establishes his reputation as a leader among American embryologists.

Four of the papers offered us pertain to the eye, with a total of 171 pages. The authors of these are Messrs. Patten and Kingsley, and the first of the series, by the former, is a condensation of his larger contribution to the Naples *Mittheilungen*. This paper has obtained a notoriety on account of the heated discussion which it evoked, and, setting personalities aside, we admit that the author's ill-conceived "dynamophagous organs" received, together with his more flagrant heresies, a well-deserved refutation. Many of this investigator's suggestions and aspirations are neither better nor worse than those of his critics and predecessors; he has, like many more enthusiasts, aimed at high game with a resolve to be sensational at all hazards. It must be admitted, however, on careful perusal of his work, that he erred in an over-enthusiasm, and that there underlies his remarkable production a substratum of solid fact. Excessive theorizers, like excessive controversialists, stand in a fair way of being shelved, if only by virtue of their verboseness. On examination of the later contributions of this author, we observe that he has profited by his hard-earned experience, and that he has, under the influence of his able editor, chosen the wise, though very obvious, path. The other writer on this subject produces a paper of an altogether more modest cast. In his 16 pages there are embodied a series of very useful observations and suggestions which, if ultimately accepted, will simplify our conceptions of the complex visual organ in the Arthropods. He differs from his contemporary on points of primary importance. We welcome this as a healthy sign (cf. *ante*), and the reader will find that in these papers, and others proffered in this journal, differences of opinion are asserted in inoffensive language, in a spirit as free of both animus and bias as it is becoming the dignity of the subject in hand.

Prof. Osborne contributes two most interesting papers: one, on the internal structure of the Amphibian brain, is the completion of a series of beautiful studies, largely inspired by our greatest living master (cf. *Morph. Jahrb.*, vol. xii. p. 247); the other, on the fœtal membranes of the Marsupials, is also a continuation of earlier studies, and we watch the growth of them with intense interest, in view of those so long looked for at the hands of a countryman of our own, upon the same in the Australian forms.

One of the most ambitious communications is that bearing the title, "On the Phylogenetic Arrangement of the Sauropsida." The author has elsewhere expressed many of his views on the subject; his enthusiasm and daring admitted, we cannot pass unnoticed the superficiality of his essay; in respect to this, it does not even come within the conditions imposed by the founders of the journal. More than that, however. Reference is made by the author to his order *Proganosauria;* if his primary diagnosis of the same (*Zoolog. Anzeiger*, 1886, p. 189) be compared with the original drawing (Proc. Amer. Phil. Soc., vol. xxiii. No. 121) and with the replicas in our national collection, it will be found that the existence of his leading structural peculiarity (we refer to the presence of five distinct tarsalia) is at least doubtful. Even if it did exist, the exaltation of such a character to a position of ordinal value would be unwarrantable, and, as employed by the author, meaningless, inasmuch as a fifth tarsale is present in the Chelonia. This he admits (cf. *Zoolog. Anzeiger*, 1888, p. 597), and in doing so he lands himself in a contradiction. We cannot but regret the hasty introduction of generalizations so sweeping into papers of a provisional nature, and we take this opportunity of entering a protest against this abuse of the "*vorläufige Mittheilung*": as a means of establishing a claim of priority in the discovery of a sound fact it is of the utmost utility, but as converted into a medium for contradiction it becomes intolerable. The over-cultivation of this unfortunate habit bids fair to involve its devotees in difficulties irrecoverable; the best work always has been, and always will be, done, as wrote Goethe, "*ohne Hast, ohne Rast.*" The waste which accrues from the abuse here deplored will ultimately find its own level, but its accumulation is none the less to be regretted.

In matters of *technique* this journal is not deficient. Its general "get-up" is most admirable, and full attention has been paid to minute detail: the type is excellent. The plates are faultless, and admirably arranged in relation to their accompanying explanations, and the eye is never offended by the unpardonable intrusion of a woodcut upon the margin of the page. One or two minor modifications might be suggested, but they are so trivial that we prefer not to burden this notice with them. We observe, with extreme satisfaction, that in leading papers care has been taken to discriminate between the more important representations of fact and the less important diagrams, the latter being interspersed among the text, in woodcut. The

journal will stand the test of comparison with any of its contemporaries, and the immediate promise of a series of papers on the anatomy and embryology of Amia augurs well for its future.

Prof. Cope contributes a very characteristic paper on the tritubercular molar, the leading deduction of which is most interesting and suggestive; it reads (vol. ii. p. 21): "The tritubercular molars of man constitute a reversion to the dentition of the Lemuridæ of the Eocene period of the family of Anaptomorphidæ," and "this reversion is principally seen among the Esquimaux, and the Slavic, French, and American branches of the European race." The senior editor, discussing, in the most philosophic paper of the series, "The Seat of Formative and Regenerative Energy," writes as follows: "These higher (biological) units combine both atomic and molecular structure, but they have, superadded to and including this, a structure as a whole, which is entirely ignored in the expression, 'molecular aggregates.' As they result from the union, not of simple or complex molecules, but of complex molecular groups, their structure may be said to be at least as widely separated from the molecule as this is from the atom"; and, further, "in claiming that 'physiological units' have something higher than molecular structure and power, I am not treading on ultra-scientific ground, but following the course already sanctioned by chemistry and physics, and the only one which can ever reconcile physico-chemical and biological conceptions." We heartily recommend this valuable essay to our readers, for the author's contentions in defence of his belief that "the organism as a whole controls the formative processes going on in each part," are worthy of all the consideration that can be given them. He appears to us to underestimate the importance of recent advance in organic chemistry. The work of unravelling the constitution of the more complex organic bodies—a work in which certain of our own countrymen are playing a leading part—gives us hope beyond that which he entertains. The presence of the above-cited remarkable passages is, in itself, sufficient to invest the early numbers of this journal with a lasting interest.

We congratulate the editors upon their enterprise; they are supported by influential friends and surrounded by enthusiastic investigators; they have, in turn, fulfilled, thus far, the highest expectations of their most sincere well-wishers, and merited the confidence and support of the biological brotherhood throughout the world.

G. B. H.

THE BALD-HEADED CHIMPANZEE.

THERE is no longer any room for doubt amongst naturalists as to the complete distinctness of the larger anthropoid ape of tropical Africa, the gorilla, from its smaller brother, the chimpanzee. The differences are amply sufficient for specific, if not for generic, distinction. But, on the question whether there is only one chimpanzee, spread over a great extent of the African continent, or several species confounded under the same name, there is still much difference of opinion. As long ago as 1853, M. Duvernoy communicated to the Academy of Sciences of Paris a short description of a second species of chimpanzee (see *Comptes rendus*, vol. xxxvi. p. 927), based on specimens obtained by Dr. Franquet in Gaboon in 1851. M. Duvernoy subsequently published an elaborate memoir on the same subject in the *Archives du Muséum* (vol. viii. p 1). The distinctions insisted upon by Duvernoy between his *Troglodytes tschego* and the ordinary *T. niger* were chiefly osteological; at the same time he characterized the *tschego* (from M. Franquet's description) as having the "face black, and the ears small," while, according to the same authority, the ordinary chimpanzee has "very large ears, and its face flesh-coloured."

In 1858, in a memoir also published in the *Archives du Muséum* (vol. x. p. 94), on the specimens of anthropoid apes in the Paris collection, M. Isidore Geoffroy St. Hilaire published a letter from Dr. Franquet in which the latter again insisted on the differences of the three species of anthropoid apes observed by him in the district of Gaboon. These were characterized as follows:—

(1) The *Chimpanzee*, with the face flesh-coloured, the ears red and large, and the fur black.

(2) The *Gorilla*, with the face black, the ears small and black, and the fur of a brownish chestnut, but varying in tint in different parts of the body, and with always a row of reddish hair starting from the middle of the forehead and following the line of the sagittal suture.

(3) The *N'tchego*, with the face black and the ears small, as in the gorilla. The hairs of this ape, he says, are shorter and darker in colour, and it never attains the size of the gorilla, or carries the red crest across the forehead.

In 1860, the well-known traveller Mr. P. B. Du Chaillu gave his account of the anthropoid apes of the Gaboon to the Boston Society of Natural History (see Proceedings of that Society, vol. vii. p. 296). Mr. Du Chaillu described, as a new species of chimpanzee, *Troglodytes calvus*, "with the head entirely bald to the level of the middle of the ears behind," and "having large ears," while he identified the *N'tchego* of Dr. Franquet as being nothing but the adult chimpanzee (*T. niger*). In a second communication to the same Society (*op. cit.* p. 358), he described another new species of chimpanzee, with a black face, but the forehead not bald, which he called *Troglodytes kooloo-kamba*, from its peculiar cry.

In 1861, the late Dr. J. E. Gray examined Mr. Du Chaillu's specimens of apes, and came to the conclusion that both his supposed new species were only varieties of the common chimpanzee (see P.Z.S., 1861, p. 273). Such also, as was stated by Dr. Gray, was my own opinion at that time, and I have remained in a doubtful state of mind on the subject until a recent period. But the acquisition of the fine female specimen of chimpanzee, generally known by the name of "Sally," by the Zoological Society in 1883, caused me to change my views very materially. There can be no doubt that this animal, when compared with specimens of the ordinary chimpanzee, presents very essential points of distinction. The uniform black face and nearly naked forehead, which is only covered with very short black hairs, together with the large size of the ears, render "Sally" conspicuously different from the many specimens of the common chimpanzee (at least thirty in number) that the Society has previously received. I was at first inclined to believe that "Sally" might be referable to the *Troglodytes tschego* of Duvernoy. But nothing is said, in M. Duvernoy's description, of the bald forehead; and the small ears attributed to the *N'tchego*, are directly contrary to this hypothesis, as in "Sally" these organs are exceedingly large and prominent. On the whole, I was inclined to believe that "Sally" might belong to the *Troglodytes calvus* of Du Chaillu, and she was accordingly entered in the Register of the Society's Menagerie as the Bald-headed Chimpanzee (*Anthropopithecus calvus*[1]), which is certainly a very appropriate name, even if it be not technically correct.

In the beginning of the present month we purchased of Mr. Cross, of Liverpool (from whom we had also obtained "Sally"), a second specimen of the Bald-headed Chimpanzee, likewise a female, which, although much smaller in size, closely resembles "Sally" in every other respect.

Fortunately, there is now in the Gardens a young specimen of the Common Chimpanzee (*Anthropopithecus troglodytes*), presented to the Society in May last by

[1] The term *Troglodytes* being more properly used for a genus of birds, it becomes necessary to employ for the chimpanzees the generic term "*Anthropopithecus*," of Blainville, as suggested by Peters in 1876.

Mr. F. J. Aldridge, F.Z.S., by whom it was brought from Sierra Leone. This specimen is of about the same size and age as the young Bald-headed Chimpanzee, and enables an easy comparison to be made between the two species. Looking first at *A. calvus*, we find the skin of the head, face, ears, and limbs of a dark brownish clay colour, which will, no doubt, get blacker as the animal becomes adult. The ears are perfectly naked, and of large size, and stand out at nearly right angles from the head. The top of the head is very scantily covered with short blackish hair. The whole of the body and limbs are also very thinly covered with hair, especially the abdomen.

When we turn to the young specimen of *A. troglodytes*, we find the upper part of the face and the brows of a dirty flesh colour. Between the eyes, above the nostrils, and passing down the cheeks, it is black. The nose and muzzle are of a dirty flesh colour. The chin and upper lip are covered with longish white hair. The inside of the ears is nearly black. The forehead, cheeks, and the whole of the body are covered with long, harsh, black hair. The colour of the hands and feet are of a brownish clay colour, much the same as those of *A. calvus.*

The rump above and below the anus is covered with longish white hair.

With regard to the size of these two animals, the length of limbs, and other measurements, they are nearly equal. It is probable that *A. troglodytes* is a trifle older than the new specimen of *A. calvus.*

It may be of interest to mention that, as Mr. Bartlett informs me, the young *A. calvus* will kill and eat sparrows in the same manner as "Sally" kills and eats pigeons, whereas the common chimpanzee will not touch any food of this kind.

It must be admitted, however, that the specific term *calvus*, applied to "Sally" and her "younger sister," can only be considered as provisional. When these specimens die, which, we trust, will not be till some distant period, they must be compared with the example of the *Troglodytes calvus* of Du Chaillu, which is now in the British Museum. On the same occasion the skulls of these specimens can be compared with the descriptions and figures given by Duvernoy of his *Troglodytes tschego.* Until this can be done, it is impossible to say decisively whether these two specimens belong to one of the supposed species already described, or should receive a new name.

Finally, I may add that the Ape House in the Society's Gardens, besides these two chimpanzees, contains at the present time a young female Orang (*Simia satyrus*), received on deposit, and a specimen of the Silvery Gibbon (*Hylobates leuciscus*), lately presented by Captain D. L. Delacherois; so that all the three known genera of anthropoid apes may be now seen represented by living specimens. P. L. S.

NOTES.

A MOVEMENT has been started in Norway for the despatch in the summer of 1890 of an Expedition which would try to reach the North Pole, and it is proposed that the leadership shall be offered to Dr. Nansen. Those who are arranging the plans maintain that no other country could furnish such a crew of experienced and hardy ice-men and Arctic travellers as Norway, and that a winter or two in the Arctic regions would affect these men very little. The intention is that an attempt shall be made to reach the Pole by way of Franz Josef's Land, a route advocated by the most experienced Norwegian Arctic travellers as well as by several well-known men of science who have studied the problem. *Ski*, which have played such a prominent part in the Nordenskiöld and Nansen Greenland expeditions, would no doubt again be of great service.

THE Royal Academy of Sciences of Turin, in accordance with the last will and testament of Dr. Cesare Alessandro Bressa, and in conformity with the programme published on December 7, 1876, announces that the term for competition for scientific works and discoveries made in the years 1885–88, to which only Italian authors and inventors were entitled, was closed on December 31, 1888. The Academy now gives notice that from January 1, 1887, the new term for competition for the seventh Bressa Prize has begun, to which, according to the testator's will, scientific men and inventors of all nations will be admitted. A prize will therefore be given to the scientific author or inventor, whatever be his nationality, who during the years 1887–90, according to the judgment of the Royal Academy of Sciences of Turin, shall have made the most important and useful discovery, or published the most valuable work on any of the following subjects—physical and experimental science, natural history, mathematics, chemistry, physiology, pathology, geology, history, geography, and statistics. The term will be closed at the end of December 1890. The value of the prize amounts to 12,000 Italian lire. The prize will in no case be given to any of the national members of the Academy of Turin, resident or non-resident.

THE Board of Electors to the Linacre Professorship of Human and Comparative Anatomy at Oxford have, on account of Prof. Moseley's continued illness, nominated Mr. W. Hatchett Jackson, M.A., F.L.S., to serve as Deputy Professor.

SIR HENRY ROSCOE has been elected to represent the Royal Society on the governing body of Eton College.

WE regret to announce the death of Mrs. Merrifield, whose name as a writer on Algæ and kindred subjects is well known to our readers. She died on January 4, in her eighty-fifth year. To-day we print an article by Mrs. Merrifield on some recent works on Algæ. We learn that she was very ill when this article was written, but it was not thought that the end was so near.

THE annual general meeting of the Royal Meteorological Society will be held at 25 Great George Street, Westminster, on Wednesday, the 16th instant, at 7.15 p.m., when the report of the Council will be read, the election of officers and Council for the ensuing year will take place, and the President (Dr. W. Marcet, F.R.S., will deliver an address on "Fogs," which will be illustrated by a number of lantern slides.

In the January number of the *Kew Bulletin* there is a most interesting paper on the coca-plant, to which considerable attention has lately been devoted, mainly because of the valuable properties ascribed to one of its alkaloids, called cocaine, as a local anæsthetic. It appears that since the discovery of the anæsthetic properties of cocaine the demand for coca-leaves in South America has considerably increased for export purposes. A distinct loss in the alkaloids generally, as well as in cocaine, has been noticed during the transit of leaves to this country; and latterly, in consequence, it has become the practice to extract the alkaloids from the leaves in South America, and export to the United States and Europe a crude preparation, which is largely taken up by manufacturers of cocaine. The demand for coca-leaves has, therefore, fallen off, and the writer of the paper thinks that the cultivation of the coca-plant in our tropical colonies will probably never assume large proportions. South America, he says, is able without further extension of cultivation to produce such enormous quantities of coca-leaves that the one-eightieth part would be sufficient to swamp the cocaine markets of the whole world. The other subjects dealt with in this number are beetles destructive to rice-crops in Burmah, fibre from Lagos, yam bean, Schweinfurth's method for preserving plants, a starch-yielding bromeliad, and the fruits of Mysore.

The Swedish Superintendent of Fisheries, Dr. F. Trybom, has, at the instance of the Swedish Government, been engaged during the past autumn in making a series of scientific experiments on the coast of Sweden for the purpose of studying the condition of the herring when undisturbed. He brought back with him newly-hatched herring-fry and herring-spawn ready to be hatched. The bottom on which these were found consisted of stones, gravel, and shells; the depth of water was about 20 metres, and the temperature of water at the bottom about 11° C. The results of these experiments are not yet known, but a Swedish paper says that they are such as to encourage the Government to permit Dr. Trybom to continue next autumn his inquiries into the development, habits, and habitats of the most important fish on the Swedish coasts.

Dr. Kolthoff, the well-known Swedish naturalist, is arranging an interesting zoological museum at the Upsala University, being a complete representation of the fauna of Scandinavia, with nests, representations of lairs, &c. This is the only museum of the kind in Sweden.

Gaseous fluoride of methyl has been obtained in the pure state, and its density determined, by MM. Moissan and Meslans. The existence of this gas was announced some years ago by MM. Dumas and Peligot, who describe a mode of preparation by the action of methyl sulphuric acid upon fluoride of potassium. The gas obtained, however, by this method is now shown to be a mixture of oxide and fluoride of methyl, and a new method has been developed by means of which it is obtained sufficiently pure for accurate analysis. The reaction is analogous to the one recently described for the preparation of ethyl fluoride, methyl iodide being allowed to act upon fluoride of silver. A regular evolution of gas commences at once in the cold, and the gaseous mixture, consisting of methyl fluoride and vapour of methyl iodide, is led first through a spiral leaden condenser cooled to − 50° C., where most of the latter substance is condensed, and afterwards through a couple of glass tubes heated to 90° and filled with fresh fluoride of silver, which removes the last traces of methyl iodide. This reaction is found to be the only one yet known which gives the gas in anything like a state of purity. MM. Moissan and Meslans have, however, also obtained it in a lesser degree of purity by the action of pentafluoride of phosphorus upon methyl alcohol. The fluoride of methyl obtained as above possesses a normal density corresponding to the formula CH_3F. It liquefies at ordinary temperatures when submitted to a pressure of thirty-two atmospheres in Cailletet's apparatus. It is slightly soluble in water, 100 c.c. at 18° dissolving about 193 c.c. of the gas; it dissolves very much more readily in methyl iodide or methyl alcohol. Perhaps the most interesting fact about it is its great stability, for, even on heating in sealed tubes at 120° in presence of water or a dilute solution of potash, it saponifies only with great difficulty.

Besides the above-described fluoride of methyl, another entirely new one, isobutyl fluoride, C_4H_9F, has been prepared by acting in a similar manner upon silver fluoride with isobutyl iodide. One scarcely knows whether to describe this new fluoride as a gas or a liquid. As a matter of fact, at this time of the year it is a colourless and very mobile liquid, but in summer weather would be a gas, inasmuch as its boiling-point is just about the temperature of an ordinary room, 16° C. The reaction between isobutyl iodide and silver fluoride commences vigorously in the cold, but the mixture requires warming to 50° in order to obtain a theoretical yield. In the form of a gas it burns on ignition, with deposition of carbon and formation of clouds of hydrofluoric acid. The liquid, when pure, is singularly incapable of attacking glass. It is interesting that M. Moissan has now succeeded in preparing, by means of the silver fluoride reaction with the iodides of the corresponding organic radicals, the fluorides of methyl, ethyl, propyl, and butyl, finding them in each case remarkably more stable than the analogous chlorine compounds.

In the latest volume of the American Consular Reports it is stated that Switzerland has followed other civilized countries in adopting a law for the protection of inventions. This law cannot have effect until it has been decided whether 30,000 voters will petition against it, in which case it must be submitted to the people. The American Consul states that it contains several new and interesting provisions, and he gives, in his Report, the text of the law. It is remarkable that only material objects, and not processes, are protected. This peculiarity is said to be due to the efforts of manufacturers of aniline colours and chemicals, who would be injuriously affected by a law which would protect arts as well as tools and machines. The duration of a patent is to be fifteen years; a fixed fee of 20 francs must be paid for the first year, and a progressive annual fee, which amounts in the fifteenth year to 160 francs.

A severe shock of earthquake, accompanied by loud subterranean noises, occurred on December 26 at a part of the Vogtland; it was also felt at Röttis, Lengenfeld, Plauen, and Auerbach. Shocks also occurred at Messina, Jagonegro, and Castroreale on the same day, but no serious damage was done.

The anomalies of weather felt in October and November last in Western Europe were also felt in Russia and Central Asia. From October 27 to November 13, at several places in Northernt, Eastern, and Southern Russia, the daily averages were below the normal temperatures of the same days by no less than from 16° to 19° C. On October 28, it was freezing at Tashkent early in the morning. Most Russian rivers were covered with ice from seven to twenty-five days earlier than they have been frozen on the average for a long series of years. Two great waves of cold could be distinguished, both spreading from the north-west to the south-east. In the second part of November there was a sudden return of warm weather, and the Russian rivers were opened again. Throughout this disturbed period the barometer gave valuable indications as to the movements of the cyclones and anticyclones. The centre of the latter moved as follows: on November 5, it was at Pinsk (782 mm. of barometric pressure, reduced to the sea-level); next day it was at Kherson (782 mm.); on November 5, at Pyatigorsk (781 mm.); and next day, at Erivan in Armenia (779 mm.) The height

reached by the barometer at Pyatigorsk was greater than it had ever before been known to be. As to the low pressures of the air, which soon followed the high pressures, they were not less remarkable. Thus, a pressure as low as 723 mm. was measured at Christiansund on November 19; 716 mm. at Nikolaistad (Wasa), and 720 mm. at Kuopio, on November 20; and 721 mm. at Povyenets, in Olonets, on November 21. So low a pressure as 716 mm. (reduced to the sea-level) is of exceedingly rare occurrence. The next minimum of pressure came on November 25, and it was accompanied by frightful storms which blew over Central Russia. The barometer did not fall so low as during the preceding days, but the displacement of the minimum was characterized by a further decrease of pressure in proportion as the centre of the depression advanced towards Central Russia. Like cases were observed also in 1881 and 1886, but they are rare on the whole. On November 25, the barometer at Moscow was as low as 723·8 mm.

At the meeting of the French Meteorological Society, on December 4 last, the President stated that the Minister of Commerce and Industry had appointed a Committee of Organization for the proposed Meteorological Congress to be held in 1889. The Committee met on November 24, and elected M. Renou as President, and M. L. Teisserenc de Bort as General Secretary. M. Moureau, Secretary of the Society, presented the results of magnetic observations made by him in the western basin of the Mediterranean by direction of the Minister of Public Instruction. M. Renou presented a Report by M. Cœurdevache on the relations of temperature and wind direction at Clermont and the summit of the Puys-de-Dôme during winter. He pointed out that the isotherms at the summit of the mountain are not parallel to those in the plain, the latter being influenced by various agencies, such as sea or mountain. M. L. de Bort stated that it was proposed to hold a meeting of meteorologists at Hamburg, with the view of preparing the basis of an understanding as to the classification of clouds. He thought that "nimbus" and "cumulus," especially, were a source of confusion in cloud observations, and that the height of clouds was much exaggerated in rainy weather. It had been observed that the Eiffel Tower, which now reaches over 670 feet, was frequently enveloped in cloud at a height of about 520 feet. M. Lemoine made some remarks upon the bad effect of the low summer temperatures upon the grape harvest, the first frosts occurring in many cases before the grapes had reached maturity.

The volume of *Abhandlungen und Berichte des K. Zoologischen und Anthropologisch-Ethnographischen Museums zu Dresden* (Friedländer and Son, Berlin)—in which there is a German translation of Mrs. Nuttall's article on a relic of ancient Mexico (referred to in another column)—contains, in addition to this translation, some interesting and valuable papers. It opens with a full account, by Dr. A. B. Meyer, the editor, of certain new arrangements for the better protection and display of objects in the Zoological and Anthropological Museum of Dresden. The editor also gives a list of the Reptilia and Batrachia collected by him in the East Indian Archipelago in the years 1870–73. There are papers on mammals from the East Indian Archipelago, by Dr. B. Hoffmann; on the Indian-Australian Myriopoda, by Dr. Erich Haase; and on other subjects. The volume is carefully illustrated.

Prof. R. Meldola, F.R.S., has in the press a work on "The Chemistry of Photography," which will shortly be issued by Messrs. Macmillan and Co., as one of the volumes of "Nature Series." The work consists of a course of lectures delivered last year at the Finsbury Technical College. The chief object kept in view by the author is the necessity for the full recognition of photographic chemistry as a branch of applied science in technical Colleges. Each lecture is followed by an appendix containing hints for the experimental illustration of the subject by means of lecture demonstrations, many of which are new, and all of which have been revised with a view of enabling lecturers to demonstrate the chemical principles of photography before an audience in a simple manner. The mode of treatment adopted will, it is anticipated, be found of use also to practical photographers, by enabling them to obtain a concise and comprehensive view of the scientific principles of their art.

Messrs. Crosby Lockwood and Son have issued a waistcoat-pocket book abounding in tables and concise information on a variety of topics connected with rural affairs, revised by Prof. Fream, of Downton Agricultural College. The title is "Tables, Memoranda, and Calculated Results for Farmers, Graziers, Agricultural Students, Surveyors, Land Agents, Auctioneers, &c.," by Sidney Francis. The contents are trustworthy and useful, and are readily found by means of a detailed index. It would be difficult to enumerate even the principal subjects dealt with in this cubic inch of printed matter; but we may say that, whether the inquirer opens it in order to find the composition of foods, fertilizers, or crops; the rules of measurement for animals, hay-stacks, timber, or water-courses; the strengths of materials; the advantages of water, steam, or horse power; the value of tillages, of tithes, or of woodlands; particulars as to piece-work, or costs of embanking, excavating, &c.,—he will find statistics on all these, and countless other subjects.

Messrs. Crosby Lockwood and Son have published a second edition of "The Blowpipe in Chemistry, Mineralogy, and Geology," by Lieut.-Colonel W. A. Ross, R.A. In this edition the work has been revised and enlarged. It contains 120 illustrations by the author.

We have received "The Mining Manual for 1888," compiled by Mr. Walter R. Skinner. The object of this work is to give the fullest possible information with regard to mining companies. The compiler mentions that, without reckoning South African mines, he has referred to 900 companies. Owing to the growing importance of mining at the Cape, Natal, and the Transvaal, a separate section on South African mines has been added.

In "The Floral King; a Life of Linnæus" (W. H. Allen and Co.), Mr. Albert Albery has presented a very good sketch of the career of the great Swedish botanist. It includes a number of interesting extracts from the late Dr. Ährling's selection from the correspondence of Linnæus.

Mr. John Murray has issued an interesting little book on "The Invisible Powers of Nature," by E. M. Caillard. Its object is to create in its readers a sufficient interest in physical science to lead them to the study of more advanced works on the subject.

A second edition of "Nature's Fairy-Land," by H. W. S. Worsley-Benison (Elliot Stock), has just been published.

Messrs. E. A. Petherick and Co. have published "A Classified List" of Mr. S. W. Silver's collection of New Zealand birds at the Manor House, Letcomb Regis. Sir Walter Buller has added short descriptive notes for the information of visitors. The value of the "List" is greatly increased by a number of woodcuts, most of which are borrowed from Sir Walter Buller's "Birds of New Zealand." Mr. Silver's collection consists of birds contained in twelve cases. Of these cases eight were on view in the New Zealand Court at the Colonial and Indian Exhibition in 1886. The four cases since added contain many of the rarer species of New Zealand birds.

In the current number of the *Mineralogical Magazine and Journal of the Mineralogical Society* there is a valuable article,

by Mr. H. A. Miers, entitled "Contributions to the Study of Pyrargyrite and Proustite." The paper, as the author explains, is the result of a study of the rich collection of red silvers in the British Museum (Natural History). The analyses have been made by Mr. G. T. Prior, and the specific gravity determinations in most instances by both Mr. Prior and Mr. Miers himself. The number also includes a paper on a peculiar variety of hornblende from Mynydd Mawr, Carnarvonshire, and a note on picrite from the Liskeard district, both by Prof. Bonney; a paper on dufrenite from Cornwall, by Prof. Kinch; and notes on some minerals from the Lizard, by Mr. J. J. H. Teall.

THE following figures show the devastations caused in the Hungarian vineyards by the Phylloxera. In 1881, 50 vineyards were infected; this number rose in 1882 to 79, in 1883 to 107, in 1884 to 237, in 1885 to 388, in 1886 to 582, and in 1887 to 811. In 1887, 132,352 acres of land were infected, the area of all the Hungarian vineyards together being 740,000 acres.

A LIST of the minerals of New York County, by B. B. Chamberlain, appears in the Transactions of the New York Academy of Sciences, vol. vii. No. 7. This list has now been reprinted. The lists of Robinson and Cozzens numbered some thirty-five minerals. That of Mr. Bailey, in 1865, embraced about forty-five titles. Mr. Chamberlain, omitting some of the less important varieties, has placed on record about a hundred names. The majority of the specimens described are from his own collection.

IN an interesting paper on the decay of the building-stones of New York City, recently read before the New York Academy of Sciences, Mr. Alexis A. Julien says it is "pitiable" to see new buildings erected in soft and often untried varieties of stone covered with delicate carvings of foliage and flower garlands, which are almost certain to be nipped off by the frost before the second generation of the owner shall enter the house. Mr. Julien points out that many of the best building-stones of America have never been brought into New York. Among the examples he mentions are siliceous limestones of the highest promise of durability, allied to that employed in Salisbury Cathedral; refractory sandstones, like some of those of Ohio and other Western States, particularly fitted for introduction into business buildings in the "dry-goods district," storage houses, &c., where a fire-proof stone is needed; and highly siliceous varieties of Lower Silurian sandstones, such as occur near Lake Champlain, quartzitic and hard to work, like the Craigleith stone of Edinburgh, and possessing the valuable qualities of that fine stone, in resisting discoloration, notwithstanding its light colour, and in remarkable resistance to disintegration.

THE French *Revue des Colonies* reports that from a plant called *Kanaff*, which grows in the summer on the shores of the Caspian, M. O. Blakenbourg, a chemist, has obtained an admirable textile matter, which is soft, elastic, tough, and silky, and which can be bleached chemically without losing these properties. The resistance of this new material is said to be far greater than that of hemp, while its specific weight is much less.

PROF. HEYDECK, of Königsberg, has been lecturing on a pile dwelling, in the Szontag Lake, in East Prussia. Ten years ago, the lake was lowered a little more than a metre. The land thus gained was cultivated, and a pile dwelling was discovered. Many flint implements were found. There was only one bronze ornament, but articles of bone were numerous. There were also vessels of clay, of which nineteen were quite uninjured.

IN our review of "The Orchids of the Cape Peninsula," by Harry Bolus, F.L.S., last week, it was noted that the omission of the publisher's name might cause inconvenience to persons wishing to purchase copies. Messrs. Wesley and Son write to us that some copies of the work have been sent to them for sale.

THE additions to the Zoological Society's Gardens during the past week include an African Zebu (*Bos indicus* ♀) from East Africa, presented by Mr. W. Mackinnon, F.Z.S.; a Coot (*Fulica atra*), British, presented by Mr. J. Cutting; a Greek Partridge (*Caccabis saxatilis*) from Bussorah, presented by Mr. Harold Hanaeur, F.Z.S.; two Red and Yellow Macaws (*Ara chloroptera*) from South America, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*), a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Lady Meux; a —— Guinea Fowl (*Numida* ——) from East Africa, presented by Mr. Percy C. Reid; three Ruffs (*Machetes pugnax*), two Snow Buntings (*Plectrophanes nivalis*), British, purchased; a Great Wailaroo (*Macropus robustus*), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *JANUARY* 13-19.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 13

Sun rises, 8h. 3m.; souths, 12h. 9m. 6·8s.; sets, 16h. 15m.: right asc. on meridian, 19h. 41·8m.; decl. 21° 24′ S. Sidereal Time at Sunset, 23h. 48m.

Moon (Full on January 17, 6h.) rises, 13h. 35m.; souths, 21h. 31m.; sets, 5h. 36m.*: right asc. on meridian, 5h. 5·2m.; decl. 19° 52′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 45 ...	12 52 ...	16 59 ...	20 25·2 ...	21 22 S.
Venus ...	9 52 ...	15 5 ...	20 18 ...	22 37·8 ...	9 58 S.
Mars ...	9 43 ...	14 49 ...	19 55 ...	22 21·7 ...	11 16 S.
Jupiter ...	6 11 ...	10 7 ...	14 3 ...	17 39·3 ...	23 0 S.
Saturn ...	18 25*...	1 55 ...	9 25 ...	9 26·1 ...	16 16 N.
Uranus...	0 27 ...	5 50 ...	11 13 ...	13 22·0 ...	7 57 S.
Neptune..	12 34 ..	20 17 ...	4 0*...	3 51·5 ...	18 26 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Jan.		
17 ...	— ...	Partial eclipse of the Moon: visible throughout the United Kingdom: first contact with shadow 3h. 59m.: middle of eclipse 5h. 30m.: last contact with shadow 7h. 0m. About two-thirds of the moon's diameter will be obscured.
18 ...	21 ...	Saturn in conjunction with and 1° 20′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5 ...	81 17 N. ...	Jan. 13,	21 33	*m*
			,, 18,	21 12	*m*
Algol	3 1·0 ...	40 32 N. ...	,, 19,	2 28	*m*
λ Tauri...	3 54·6 ...	12 11 N. ...	,, 17,	3 34	*m*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,, 17,	23 0	*m*
R Canis Majoris ...	7 14·5 ...	16 11 N. ...	,, 14,	0 41	*m*
			,, 15,	4 6	*m*
S Cancri	8 37·6 ...	19 26 N. ...	,, 13,	20 51	*m*
T Virginis	12 8·9 ...	5 25 S. ...	,, 14,		*M*
δ Libræ	14 55·1 ...	8 5 S. ...	,, 15,	6 44	*m*
U Coronæ	15 13·7 ...	32 3 N. ...	,, 18,	5 45	*m*
R Scorpii	16 11·0 ...	22 40 S. ...	,, 16,		*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,, 16,	5 45	*m*
R Lyræ	18 52·0 ...	43 48 N. ...	,, 18,		*M*
S Vulpeculæ ...	19 43·8 ...	27 1 N. ...	,, 13,		*M*
R Sagittæ	20 9·0 ...	16 23 N. ...	,, 17,		*m*
X Cygni	20 39·0 ...	35 11 N. ...	,, 17,	21 0	*M*
T Vulpeculæ ...	20 46·8 ...	27 50 N. ...	,, 13,	6 0	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 13,	17 40	*m*
		and at intervals of		36 0	
δ Cephei	22 25·0 ...	57 51 N. ...	Jan. 16,	5 0	*m*
			,, 17,	20 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near π Orionis	72 ...	5 N.	
,, κ Cygni	295 ...	53 N. ...	Slow, trained.

GEOGRAPHICAL NOTES.

AT the Royal Geographical Society on Monday night, a paper was read by Mr. F. S. Arnot on his journey from Natal to Bihé and Benguella, and thence across the central plateau of Africa to the sources of the Zambesi and the Congo. Mr. Arnot reached Natal in September 1881, and has only just returned from his seven years' wanderings, during which he crossed the continent to some extent in the route of Livingstone. His paper forms an important supplement to the work of Livingstone, Cameron, Ivens and Capello, and the German traveller Reichart. Crossing from Natal obliquely, he struck the Zambesi near Sesheke, and ascended the river to Lealui, the town of Liwanika, to endeavour to persuade the chief to let him proceed northwards among the Batonge and Mashashe. Unsuccessful in this, Mr. Arnot left Lealui in May 1884, and proceeded to Bihé and the coast. Returning to Bihé, Mr. Arnot proceeded eastwards, crossing the interesting country from which so many rivers take their rise, flowing north, south, and west, to the Congo, the Zambesi, and the Atlantic. He touched Lake Dilolo, which he has reduced to very small dimensions, and has done something to rectify our knowledge of the sources of the Zambesi. The main stream, according to Mr. Arnot, comes from the east, and of this the Leeba is only a tributary. He stayed for two years at the capital of the kingdom of the chief Msidi, of whom and his government he gives an interesting account. Here he was in the region of the sources of the Lualaba. Msidi, who is really a native of Unyanyembe, seems a man of some ability, and is rapidly extending his power. He and Kangombe between them have almost swallowed up the once powerful kingdom of Muata Yanvo. Mr. Arnot returns to the Bangweolo region in March next.

DR. MEYER, and his companion Dr. O. Baumann, who were recently compelled by the hostility of the natives in East Africa to take flight to the coast, actually succeeded in crossing the country of Usambara by a new route. After marching through Bondei to the mission station of Magila, they travelled for several days through a fertile, and in places thickly-wooded depression, which forms part of the Sigi basin, reaching Hanon on September 8. Crossing the Mielo Ridge they descended into the valley of the Kumba River, and on September 18 reached the valley of Mlalo, where the Umba River runs. This region is well cultivated, and covered with numerous and large villages. Proceeding to Masende, Dr. Baumann with some natives explored the mountains, arriving eventually at a fertile region inhabited by the Wambunga. These people differ completely from the Washamba of Usambara, and are a remnant of the aborigines of the mountains, speaking a dialect similar to the Kiparé.

FROM the new volume of the *Geographische Jahrbuch* we learn that there are now 101 Geographical Societies in the world. Of these, France and her colonies have more than any other country,—29, with 19,800 members; next comes Germany, with 22 Societies, and 9200 members; followed by Great Britain and her colonies with 9 Societies, and 5600 members. There are altogether 130 geographical serials published in the various countries of the world.

M. JEAN CHAFFANJON, the explorer of the Orinoco, we learn from the *Scottish Geographical Magazine*, is about to undertake a new task. He is going to explore the peninsula and lake of Maracaibo. A tribe of Indians live in the peninsula, concerning whom no scientific data have been obtained, for they allow no one to go among them. M. Chaffanjon will try to penetrate this mystery. He will also examine the lacustrine dwellings of an extinct race in the Maracaibo Lake, and then, following the chain of the Andes, will ascend to the source of the Magdalena, cross the group of mountains which separates this river from the Rio Canca, and explore the latter down to Antioquia.

SOME ANNELIDAN AFFINITIES IN THE ONTOGENY OF THE VERTEBRATE NERVOUS SYSTEM.

IN the controversy respecting the ancestry of the Vertebrata the nervous system has always played an important part: that system is—I think Prof. Wiedersheim was the first to say it—the most aristocratic and conservative of all the organ systems of the animal body, and it clings to ancestral traditions more than any other. Anyone who has read Kleinenberg's marvellous account of the complicated manner in which the permanent nervous apparatus of the Annelid worm is built up from that of the larva (in which process of building up it passes through stages which can only be looked upon as ancestral), will readily agree that if we are ever to trace the ancestry of Vertebrates at all, the nervous system will probably form a significant factor in the solution.

The attempts made hitherto to homologize the nervous system of Vertebrates, either in the embryo or in the adult, with that of some Invertebrate or other, do not appear to have met with much success. To take one of the most recent of these. Prof. Hubrecht has, at the close of his *Challenger* Report on the Nemertines, indicated what he would regard as points of homology between the nervous system of this group and that of Vertebrates. The comparison is, in my opinion, exceedingly strained, and indeed it would not be difficult to show that it is absolutely erroneous.

The theory of the descent of Vertebrates from animals allied to the Tunicata was, as is well known, partially based on certain characters of the nervous system in the Tunicate larva; but that theory can now hardly be defended, since Dohrn has adduced powerful arguments for putting the descent the other way about—*i.e.* from Vertebrates to Tunicata—by insisting that the structure and development of Tunicata prove them to be degenerate Vertebrates.

As a third alternative we have a descent offered us by Bateson from Balanoglossus-like animals, with gill-clefts and a nervous system and notochord resembling that of Vertebrates. Many zoologists see the main and only resemblance between Balanoglossus and Vertebrates in the possession of gill-clefts. It is many years now since these structures in the two groups were first compared, and the supposed relationships between them more recently insisted upon do not seem to me to be of a very stable order. The nervous system and notochord of Balanoglossus are to be excluded from the comparison simply because they are on the hæmal side of the body, and therefore cannot be compared to structures which, like the nervous system and notochord of Vertebrates, are *not* on the hæmal side. As I am here only considering the claims of the nervous system to an homology, I cannot fully discuss the gill-clefts of Balanoglossus, and need only remark that a respiratory function of some part of the alimentary canal—generally the anterior part—is very commonly met with in many classes of the animal kingdom. Now, gill-clefts alone, without sense-organs, skeleton, nerves, or muscles (and these have not been described yet for Balanoglossus), are merely the results of a gut respiration, the alimentary tract having acquired openings on the lateral surface of the body, and it is by no means improbable that such openings could be acquired independently in two groups of animals otherwise widely separated. Two such groups are Balanoglossus and the Vertebrata.

The only remaining theory[1] of Vertebrate ancestry demanding consideration is that of Semper and Dohrn, which would derive those animals from Annelid worms. The first comparison concerned the nephridia; and it is to be remarked that the nervous system, the question of the homology of which has not been left in the background, has always been the great obstacle in the way of its acceptation, for no one has, as yet, succeeded in finding, in the Vertebrate, any homologue of the Annelidan supra-œsophageal ganglion. There have been plenty of wild and improbable speculations as to its whereabouts. A new era, however, opens with Kleinenberg's hint that possibly the supra-œsophageal ganglion of Annelids is suppressed even in the ontogeny of Vertebrates; and, if we concede this, we must look to the ventral chain of the Annelids as typifying the initial structure from which the central nervous system of Vertebrates arose. And now what points of agreement have been discovered between these two structures and their related nerves and sense-organs?

Eisig has compared the lateral sense-organs of Capitellidæ, which are segmentally arranged along the whole body of the animal, with the lateral sense-organs of Vertebrates. The latter arise in the head, and are at first confined to the head metameres: later they grow on to the trunk; they there become also segmental, but they are innervated by a true cranial nerve. Now, although Dr. Eisig's comparison is a very enticing one, it can be neither accepted nor rejected without further inquiry. There are many facts for it and some very important ones which, though not directly opposed to it, are not in its favour. We must attach a good deal of importance to it, for the

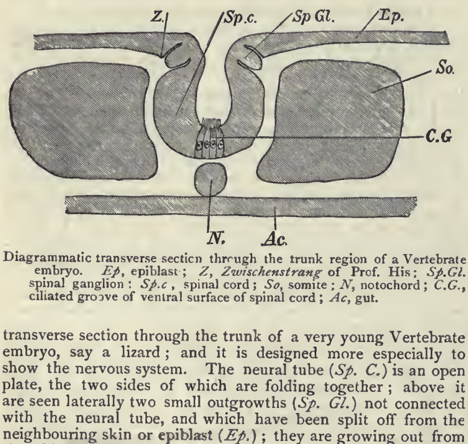
[1] Balfour ("Elasmobranch Fishes," p. 171) enunciated a different theory which can hardly now be maintained.

sense-organs of these Annelids are the only ones of which we know they in any way fulfil the conditions required of the ancestry of the lateral sense-organs of Vertebrates. It appears also that, if we admit the proposed homology as based upon the Capitellidæ, we can carry the origin of the lateral sense-organs of both groups back to still simpler structures, for it seems clear that the lateral sense-organs of Annelids have been derived from cirri and portions of the parapodial ganglia (Kleinenberg, Eisig). The rest of the Vertebrate sense-organs are easily accounted for. It is becoming more and more probable that the nose and ear are modified portions of the system of lateral sense-organs, and I am not without hopes of showing that the taste-buds of the higher Vertebrates may be derived from lateral sense-organs which wandered through certain of the gill-clefts during the development.

The eyes are not difficult to account for, as plenty of Annelids have eyes at the extreme anterior end of the ventral cord, in connection with the first ganglion.

To refer, in passing, to another structure. The Vertebrate notochord has been shown by Ehlers and Eisig to correspond with the "*Nebendarm*" of Annelida. At the risk of being pronounced a heretic, I venture the opinion that the swimming-bladder of fishes is also a derivative of the "*Nebendarm*," and that the notochord and swimming-bladder are parts of the same structure which have acquired different functions, and so developed divergently. It is certainly not difficult to raise arguments against the so universally accepted homology of swimming-bladder and lungs.

And now let me refer to some recent results of my own on the Vertebrate nervous system. I have depicted them, very diagrammatically, in the accompanying figure. It represents a



Diagrammatic transverse section through the trunk region of a Vertebrate embryo. *Ep*, epiblast; *Z*, *Zwischenstrang* of Prof. His; *Sp.Gl.* spinal ganglion: *Sp.c*, spinal cord; *So*, somite; *N*, notochord; *C.G.*, ciliated groove of ventral surface of spinal cord; *Ac*, gut.

transverse section through the trunk of a very young Vertebrate embryo, say a lizard; and it is designed more especially to show the nervous system. The neural tube (*Sp. C.*) is an open plate, the two sides of which are folding together; above it are seen laterally two small outgrowths (*Sp. Gl.*) not connected with the neural tube, and which have been split off from the neighbouring skin or epiblast (*Ep.*); they are growing out from the skin, and will soon be segmented off from it to form the spinal ganglia. It has usually been supposed that the cranial and spinal ganglia of Vertebrates arise as outgrowths of the central nervous system. Such is not the case; the diagrammatic figure given above partially disproves that, and it would be easy to give a series of figures which would demonstrate beyond doubt that the spinal ganglia and certain portions of the more complicated cranial ganglia arise from the epiblast outside and beyond the limit of the central nervous system, and that not a single cell of either cranial or spinal ganglia is derived from the latter. Now, the mode of development of the ganglia in Vertebrates tallies exactly with that described by Kleinenberg in Annelida for the parapodial ganglia. These latter also arise as epiblastic differentiations just above the lateral limit of the ventral cord, and, like the ganglia of Vertebrates, they appear segmentally.

Here, then, is one point of close resemblance, and not an unimportant one, between the Annelid and the Vertebrate.

Having got thus far, one is tempted to study the development of the central nervous system of Vertebrates more closely, in order to see whether other Annelid peculiarities recently discovered are represented. Of such, two have presented themselves, and while probably but the forerunners of a series yet to be unravelled, they are in themselves of the highest significance. I have represented these in the diagram in such a form that they may be easily understood; but be it remarked that they do not appear so obviously till at a later stage than that depicted.

Evidence has long been wanting of a bilateral origin of the central nervous system of Vertebrates (no doubt it is a bilateral structure—everyone, except perhaps Prof. Hubrecht, believes that), and if it is comparable to the ventral chain of Annelids, it ought to show traces of such origin in its early development. It is precisely this which I believe to have discovered.

Even before the actual closure of the two limbs of the neural plate occurs (a phenomenon which takes place much later than is generally supposed) the neuro-epithelium of the one limb does not pass directly into that of the other limb, for the two are separated below by a tract of non-nervous epiblast having the characters of a ciliated epithelium (*C.G.*). Thus, with greater truth than one can speak of the absence of primary connection between ganglia and central organ, it must be admitted that the two lateral halves of the central nervous system itself are at first destitute of nervous connection with each other. This ciliated groove (*C. G.*) is a very obvious structure in transverse sections of great numbers of Vertebrate embryos. Curiously enough, I cannot find it figured by any embryologist except His, and he does not say anything about it.

The peculiarities just described (a developing nervous apparatus composed of two bands of neuro-epithelium separated from each other by a ciliated groove) are eminently characteristic of Annelids. Nay, more; Kleinenberg states that the ciliated groove takes its origin from a double row of cells in the mid-ventral or neural line, and I am of opinion that such is the origin of the ciliated neural groove of Vertebrates. If this discovery of the double nature of the neural plate has the significance which I claim for it, the generally accepted opinion as to its primary structure must fall to the ground. The neural plate is usually supposed to be composed of two layers—an inner nervous one, and an outer ordinary non-nervous one; the inner layer is supposed to give rise to the nerve-cells, &c., while the outer epiblastic one, having unfortunately got shut in with the tube formation, has nothing left to it but to form the ciliated canal.

Both these conclusions are wrong. Years ago, Altmann showed—and it has been confirmed scores of times—that it is just those cells next the primary central canal which increase most, and so form the antecedents of the ganglion-cells. The real truth is, that the greater part of the epithelium lining the primary (as opposed to the permanent) central canal is a neuro-epithelium, for only such a one has the faculty of producing ganglion-cells on its inner side.[1]

The epithelium of this ciliated groove having developed cilia, undergoes no further differentiation for some time; *it is the only part of the primary central cylinder which is ciliated, and which does not form ganglion elements, and hence it is the only part which is not neuro-epithelium.* It forms later, by the growth and increase of its elements, *most if not the whole of the ciliated epithelium of the permanent central canal.*

In one respect the ciliated groove of the Vertebrate differs from that of Annelids—it gets invaginated along with the central nervous system; and I am not aware that any Annelid is known in which the ciliated groove is removed from the outer surface of the body, along with the ventral cords.

Now these facts are very remarkable, and, taken in connection with other points previously mentioned—such as the formation of the notochord and swimming-bladder, the lateral sense-organs, the origin of the ganglia—they furnish us with a combination of Vertebrate characters for which a parallel is to be found in the Annelida, and in no other group.

Further, we have in the nephridia of Vertebrates a series of structures which, as Semper first showed, find their parallel in Annelids. When one considers recent advances in our knowledge of the nephridia of Annelida (more especially those we owe to Drs. Eisig and Ed. Meyer) in connection with the, as yet, partially unpublished researches of Dr. Van Wijhe on Vertebrates, the justice of Semper's and Balfour's renowned comparison becomes more and more obvious.

I have shown, in a former number of NATURE (vol. xxxvii.

[1] As I write this, there occurs to me a beautiful idea of Kleinenberg's, that the ganglion-cells in the central organ which are perceptive of light have that power in virtue of the fact that they were themselves once retinal elements or parts of such elements.

p. 224), in what a marvellous manner the development of the hypophysis cerebri of Vertebrates, with its oral and neural portions, accords with the development of the permanent œsophagus and its special nervous system in Annelids. I now submit some no less striking resemblances between the two groups; and I am of opinion that we may hope, with work and increasing knowledge, to encounter many more such, as yet undreamt of.

J. BEARD.

Anatomisches Institut, Freiburg i/B., September 21.

THE JOURNAL OF THE ROYAL AGRICULTURAL SOCIETY.[1]

IT is seldom that the Journal of an important Society so abounds with obituary notices of prominent contributors as the one now before us. The sad refrain of "*In Memoriam*" runs through but too many of the closing pages of the number, in affectionate remembrance of names which have been associated with the advancement of agricultural knowledge throughout a considerable part of this century. The late Charles Randell, of Chadbury, was essentially a farmer of the widest views and experience, and full of sympathy for scientific work. The late John Chalmers Morton, the late John Algernon Clarke, and the late John Coleman ranked among the most distinguished ornaments of the literary aspect of agriculture. The editor, remarking upon these losses, says: "It is a noteworthy but melancholy circumstance that, in the short space of six months, the three leading professional writers on agricultural subjects should have been gathered in by the Great Harvester." We should be wanting in respect to pass over unnoticed these bereavements, and when we call to mind the very recent deaths of Dr. Voelcker and Mr. H. M. Jenkins, the late secretary and editor, we must admit that this Society has sustained exceptionally heavy losses.

The present number, however, bears witness to the fact that able successors are to be found to carry on the good work of the Society, and that, as the veterans pass away, young and enthusiastic labourers step into their places.

As usual, the material of the half-yearly issue may be divided into official Reports and articles by unattached contributors. The first section includes the Reports on the farm prize competition in Northumberland; on the implements, live stock, and poultry at the Nottingham meeting of last summer; on horse-shoeing, followed by an able paper on the structure of the horse's foot by Prof. G. T. Brown, C.B.; and on the Newcastle (1887) engine trials, by the Consulting Engineer to the Society. These Reports we cannot do more than notice as well worth the attention both of mature agriculturists and students of the art. The remaining portion of the volume contains articles upon the principles of forestry, farming in the Channel Islands, the propagation and prevention of smut in oats and barley, and various papers on stock-feeding and crop-growing.

None of these papers will create more interest than that upon the herbage of old grass-land, by Dr. W. Fream, and this paper stands prominently forward as the only one which may be described as an original investigation. The question is not only important, but controversial. The best way of producing that inimitable natural product, a rich pasture, has long been a subject of vital interest to landowners. In the long period of agricultural depression, grass-lands have scarcely shared in the general depreciation of values. Good grass-land will always let, and it is likely to maintain its value. The difficulty of converting tillage land into grass has always, however, been a problem hard of solution, and anyone who throws light upon this question is deserving of gratitude.

One of the chief difficulties has consisted in ascertaining the proper descriptions of seeds for producing a permanent pasture, and a great deal of discussion has taken place upon the relative merits and demerits of the members of the large family of the *Gramineæ*, as well as of the *Leguminosæ*, composing the complex herbage of a good meadow or pasture. Certain grasses have been named as especially suitable, while others, although occurring in all pastures, have been condemned as worse than useless. On the other hand, it has been freely asserted that many of our best grazing-lands are largely composed of grasses which have been stigmatized as worthless by certain authorities, and the inspection of high-class pastures has often staggered the botanist by the perverseness with which which they carried the "wrong" descriptions of grasses, and nevertheless held their own as producers of valued hay, or, if grazed, of beef, mutton, and milk.

[1] Second Series, vol. xxiv. Part 1. (London: John Murray, 1888.)

One of the most maligned of the grasses of late years has been common rye-grass. This grass, although popular with farmers, was stigmatized by Mr. Faunce de Laune, in an able paper published a few years ago, as a short-lived and inferior grass, foisted upon the farmers by seedsmen because of its cheapness and the ease with which it germinated and covered the ground. Mr. Faunce de Laune *ruined* rye-grass, his views being somewhat too precipitately indorsed by the officers of the Royal Agricultural Society, and the seed trade was ruled into unwilling obedience. Rye-grass was banished from all mixtures sown by truly enterprising and advanced agriculturists, but its use still lingered among the less scientific but more practical members of the confraternity of farmers.

In spite of this crusade against rye-grass, many observing and scientific agriculturists were in doubt, especially as rye-grass was seen to occupy a leading position in all natural pastures, and hence its evanescent or short-lived character was doubted.

Prof. Fream, partly from a desire to test the true value of rye-grass, but also with a view to investigating the botanical composition of good grass-land, put himself in communication with a number of experienced agriculturists in England and Ireland, and with their co-operation transplanted twenty-five representative sods, 2 feet long, 1 foot broad, and 9 inches deep, from as many pastures, and planted them side by side in a bed 72 feet long and 6 feet wide in the Botanical Garden of the College of Agriculture, Downton. This transplantation was accomplished in the winter and spring of 1887-88.

In the month of July the herbage of each turf was cut, and submitted to a quantitative botanical examination, with very interesting and surprising results. In the first place, these samples of pastures, brought from twelve English and eight Irish counties, gave evidence that the preponderance of their herbage was composed of two plants, one being the maligned and tabooed perennial rye-grass (*Lolium perenne*), and the other chief constituent being common white clover (*Trifolium repens*). As each of the twenty-five sods was selected from the best grass-land of its district by resident agriculturists of well-known judgment, the case appears to be conclusive in favour of the recently, but only recently, discarded grass. The actual fact is that rye-grass constituted in the various plots high percentages of the total gramineous herbage, as the following figures show:—No. 1 turf (Wainfleet), 75 per cent.; No. 2 turf (Tenterden), 90 per cent.; No. 3 turf (Sherborne), 76 per cent.; No. 4 turf (Sherborne), 77 per cent.; No. 5 turf (Somerset), 82 per cent.; No. 6 turf (Derbyshire), 18 per cent.; No. 7 turf (Somerset), 90 per cent.; No. 8 turf (Tipperary), 66 per cent.; No. 10 turf, 78 per cent.; No. 11 turf, 83 per cent.; No. 12 turf, 90 per cent. It is needless to continue this list, and it is sufficient to say that, with very trifling exceptions, these important turfs unanimously showed themselves in favour of rye-grass; in fact, this species heads the list in 21 out of the 25 cases.

Similarly, the leguminous herbage was found to contain one constituent in paramount abundance—namely, white clover; so that it may be approximately stated that, while the grassy herbage was chiefly composed of rye-grass, the leguminous herbage was chiefly composed of white Dutch clover. In one case—that of a turf sent by Sir Louis T. Delcomyn, of the Old Court, Bradwardine, Herefordshire—rye-grass and white clover composed the entire herbage, without the intervention of another plant of any kind whatsoever.

A more crushing piece of evidence against the enemies of perennial rye-grass could not well have been produced, and the farmer will once more be justified for his slowness in accepting the *dicta* of some of his would-be teachers. The case is of such practical importance that we have dealt with it at some length; and it should be added that the percentage botanical composition of the gramineous herbage of each turf is given in detail, so that the labour involved must have been very great.

Prof. Curtis, in a useful paper upon Forestry, deprecates the founding of a School of Forestry, but recommends the formation of a representative Board of Examiners in Forestry on the lines proposed by Mr. Rogers, of the Surveyors' Institution, and Colonel Pearson, to the Select Committee on Forestry. The Report on the Farm Prize Competition is of value for comparative purposes, chiefly as showing the amounts expended by good farmers upon fertilizers and feeding-stuffs, and the different practices obtaining in the locality where the competition took place. The remaining papers we cannot at present notice particularly, but have indicated their presence.

A RELIC OF ANCIENT MEXICO.

THE appearance of No. 1 of the *Peabody Museum Papers* marks a new departure in the publications of the Museum of American Archæology and Ethnology. Henceforth, Prof. F. W. Putnam states, the special papers, hitherto published in connection with the Annual Reports, will be issued in a separate but similar octavo form at irregular intervals, as the means for printing them is obtained. Part 1 of vol. i. of this new series, just received, consists of an interesting and thoughtful historical essay on a relic of ancient Mexico entitled "Standard or Head-dress," by Mrs. Zelia Nuttall, accompanied by three coloured plates. A quarto German translation by Dr. A. B. Meyer appeared in the last volume of the *Abhandlungen und Berichte des K. Zoologischen und Anthropologisch-Ethnographischen Museums zu Dresden.* It treats mainly of a remarkable piece of ancient Mexican feather-work inlaid with gold of the time of Montezuma, which was one of the first presents received and forwarded by Cortes to the Emperor Charles V. It subsequently formed part of the famous Ambras collection of historical armour, figuring in various catalogues of that collection as a "Moorish hat," an "Indian apron," and a "Mexican head dress," and is now preserved in the Imperial Natural History Museum of Vienna. It was carefully restored by the late Prof. F. von Hochstetter, who published in 1884 a description of it as a "banner" or "fan-shaped standard," basing this identification chiefly upon the resemblance it presented to a "fan-shaped object" depicted behind the portrait of a Mexican warrior in the "Bilimek" collection acquired by the Museum in 1878. In the present essay Mrs. Nuttall adduces abundant testimony that the feather piece in question was a head-dress which formerly presented all the attributes of colour, form, and insignia of the war-god Huitzilopochtli, the hero-god and totemic divinity of the Mexicans. Such head-gear could have been worn only at the time of the conquest by Montezuma, "the living representative of the god," as "supreme pontiff and chief warrior." An exactly similar emblematic head-dress, she points out, is depicted on the so-called "sacrificial stone" as worn by Jiz-oc, one of Montezuma's predecessors. It is further maintained that the painting of the "Bilimek" warrior must be regarded as a rebus and not as a portrait. The "fan-shaped object" is the insignia of Quetzal feathers, characterizing the high rank of the warrior, who was also a priest, and is represented as clad in a human skin. The house = *calli*, piece of cord = *mecatl*, and arrows = *tlacochtl*, similarly depicted, yield, together with the Quetzal feather insignia when deciphered with the aid of the associated complementary sign, the phonetic values: (1) the surname *Calmecahua*; (2) the title *Tlacochcalcatl* = lord of the house of arrows or supreme war-chief; and (3) the tribal designation *Quetzalapanecatl*, a native of Quetzalapan, a locality near Mexico conquered by the Mexicans in 1512. This renders probable the identification of the individual as that Calmecahua, or lord of the *calmecac*, who, as Diaz relates, "fought like a lion on the side of the Spaniards" at the battle of Otumba against his natural foes the Mexicans, and was afterwards baptized as Don Antonio, and is cited by Txtlilxochitl as the author of a history of Tlaxcala, written in 1548. However this may be, it is evident that he was a *pilhua*, or head of a large family, as Mrs. Nuttall shows that the heads, surmised by Dr. Hochstetter to be those of "decapitated enemies," painted at his feet, are the usual signs for enumerating individuals, by reference to other Mexican manuscripts extant, in which similar heads under a figure are accompanied by the Nahualtl word for genealogy, and in the case of a manuscript dating about 1520, in the possession of Mr. Bernard Quaritch, by the Nahuatl text in Spanish letters = "Tenancacaltzin these his sons' heads." In an appendix to this suggestive paper Mrs. Nuttall discusses the complementary signs of the Mexican graphic system.

SCIENTIFIC SERIALS.

Bulletin de l'Académie Royale de Belgique, October 1888.—On the influence of diurnal nutation in the discussion of the observations of α Lyræ, made at the Washington Observatory, by L. Niesten. In these researches, which are somewhat analogous to his previous observations on γ Draconis made at Greenwich, the author adduces a fresh proof of the existence of diurnal nutation. The coefficient resulting from his determination is 0·095″, giving 69° east of Paris as the longitude of the first meridian.—On a new registering process by means of photography, by Eric Gérard. In this ingenious apparatus, instead of using the voltaic arc as the source of the light falling on the concave mirror whose movements have to be recorded, the inventor employs the secondary spark supplied by the Ruhmkorff bobbin. This spark being periodical, owing to the elasticity of the check-spring of the bobbin, naturally gives the division of time in equal intervals inscribed on the registering curve. In this way the use of all special chronographs may be dispensed with. The author has applied the method to the study of the variable currents supplied by dynamos with alternate currents, and has obtained excellent results.—Jean Masius contributes a memoir on the genesis of the placenta in the rabbit, with a view to the elucidation of the difficult questions connected with the origin and purpose of various elements present in the fully developed placenta.

Rivista Scientifico-Industriale, November 15, 1888.—Granular snow and the theory of the formation of hail, by Prof. Ferdinando Palagi. The author had a good opportunity of studying the phenomenon of granular snow during a heavy snow-storm at Teramo on October 20. The grains, about the size of ordinary peas, were perfectly dry, falling with a clatter like that of hail, which they resembled somewhat in appearance, although evidently formed, not by superimposed layers of ice, but by particles of snow agglomerated under certain atmospheric and perhaps electrical conditions. They were relatively light, perfectly white and opaque, yielding under pressure between the fingers, and from their general appearance and the circumstances of their formation Prof. Palagi concluded that granular snow is the first phase in the formation of hail.—On the development of electricity from the evaporation of marine water under the exclusive action of the solar rays, by Prof. Luigi Palmieri. Some recent experiments with the Bohnenberger electroscope are here described, which fully confirm the conclusions already arrived at forty years ago by the author, and in fact anticipated by Volta, regarding the origin of atmospheric electricity from aqueous evaporation.—Signor Giuseppe Terrenzi describes some remains of the beaver (*Castor fiber*, Lin.) lately discovered in the Pliocene formations of the Colle dell' Oro near Terni.

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, December 20, 1888.—Mr. W. Carruthers, F.R.S., President, in the chair.—Prof. R. J. Anderson exhibited a photograph of an apparatus for the microscope which he had designed, consisting of a revolving disk with clips, by means of which a number of slides may be successively brought opposite the microscope, which is fixed in a horizontal position in front of it.—Mr. Clement Reid exhibited fruit of the Hornbeam from the pre-glacial forest bed at Pakefield, near Norwich, and not previously recorded as occurring in any British deposit.—Mr. T Christy exhibited a collection received from Java of hairs from the base of various ferns, notably *Cibotium Cummingii*, and a species, as supposed, of *Dicksonia*, used as a styptic, for staunching blood. Prof. Stewart, in pointing out that the use of similar material for a like purpose in China was well known to surgeons, took occasion to explain the nature of the so-called "lamb of Tartary," on which an instructive little volume had been published by the late Mr. Henry Lee, F.L.S. Mr. D. Morris remarked that the use of "fern hairs" was also known as a styptic in South America, whence specimens had been forwarded to the Herbarium at Kew.—A paper was then read by Mr. D. Morris on the characteristics of plants included under *Erythroxylon Coca*, Lamarck, with a description of a new variety, which he proposed to name, from its origin, *E. novo-granatense.* He pointed out that the well-known coca-plant had been noticed by botanists and travellers for the last 300 years; and that, although Clusius was generally regarded as the earliest writer on it, he had been anticipated by Nicholas Monardes in his "Historia Medicinal," published at Seville in 1580, and translated by Clusius, who printed it in a condensed form in his "Exoticorum libri decem" in 1605. The plant was first described as a species by Lamarck, in the "Encyclopédie Méthodique" in 1786, from specimens brought by de Jussieu from Peru. Until lately the leaves had been used merely as a nervous stimulant, like opium in China, and betel in the East Indies; but had latterly come into prominence as the source of cocaine, a valuable alkaloid possessing anæsthetic properties in contact with the mucous membrane. There were several climatic forms more or less distinct; and after describing the typical plant, Mr. Morris pointed out the characters by which *E. novo-granatense* might be distinguished. The paper was ably criticized by Mr. J. G. Baker, Mr. Rolfe, and Mr. Thomas Christy.—Mr. Spencer Moore

contributed a paper on *Apiocystis*, which he regarded as a *Volvocinea*. The ciliated form was described, and it was shown that its zoospores may sometimes escape as cœnobia, like a degenerate *Volvocinea* which has exchanged the motile for the fixed condition. The sexual cells being zoogametes, its affinity is rather with *Pandorineæ* than with oogamous *Volvoceæ*. The paper was criticized by Mr. A. W. Bennett and Prof. Marshall Ward, who, while testifying to the importance of the investigation, expressed the hope that no change would be made in classification until further examination had been made of some of the stages at a critical period of development. Mr. George Murray gave his warm support to the views expressed by Mr. Moore.—A paper was then read by Mr. G. B. Sowerby embodying descriptions of some new species of shells, of which coloured drawings were exhibited. Amongst these, the most noticeable were an *Orthalicus* from the Peruvian Andes, *Pleurotoma* (Hong Kong), *Amathina* (Mauritius), *Crassatella* (Japan), *Clavigella* (Mauritius), and *Pectunculus* (Australia). An interesting discussion followed, in which Prof. Stewart and Prof. Mivart took part, upon the coloration of Mollusca being possibly dependent upon the colour of their natural surroundings, or upon that of the host to which in many instances they were found to be attached.

Geological Society, December 19, 1888.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read :—*Trigonocrinus*, a new genus of Crinoidea from the "Weisser Jura" of Bavaria, with description of new species, *T. liratus* ; Appendix I. Sudden deviations from normal symmetry in Neocrinoidea ; and Appendix II. *Marsupites testudinarius*, Schl., sp., by F. A. Bather. This genus is proposed on the evidence of two calyces in the British Museum (Natural History) which were found among specimens of *Eugeniacrinus* from Streitberg. The species of *Eugeniacrinus*, *Phyllocrinus*, and *Trigonocrinus* may be arranged in a series which is apparently one of evolution. The present genus is, therefore, to be placed with the Eugeniacrinidæ, although its characters are not those of the family as heretofore defined. This is seen from the following diagnosis : *Trigonocrinus*, gen. nov. Calyx roughly triangular or trilobate in section. Basals 4, but one so atrophied as to be almost invisible ; all fused into a basal ring. First radials 4 ; the two on either side of the smallest basal half the size of the others, thus maintaining the triangular symmetry ; all closely united, with each suture-line in a groove. Processes of radials well developed, forming spines homologous with the petals of *Phyllocrinus* ; except the adjacent processes of the smaller radials, which only form a minute ridge. Articular surface of radials curved gently inwards and upwards ; muscular impressions indistinct or absent ; no articular ridge ; no canal-aperture. Arms unknown (? represented by fleshy appendages). Calycal cavity contained in first radials ; with small round ventral aperture, surrounded by a rim, which is the only relic of a muscular attachment. Stem unknown. The two calyces belong to the same species, viz. *T. liratus*, sp. nov. Calyx rather more elongate than in the known species of *Phyllocrinus* ; basals ornamented with minute granules ; radials ornamented with similar granules run into curved ridges, which, owing to their differing intensity, give an imbricated appearance ; spines, triangular in section, with the base of the triangle directed inwards, the apex outwards, the angles often rounded. The differentiation of *Trigonocrinus* from the central Eugeniacrinid type has been effected on the one hand in accordance with the principles of "Degeneration," "Reversion," and "Use and Disuse" ; while, on the other hand, it exemplifies certain methods of change in organic forms, which may be referred to the categories of (1) sport, (2) hypertrophy and atrophy, (3) fusion and fission. Thus considered it is of unique interest among Crinoidea. An examination of the variations in symmetry presented by the Echinodermata suggests the conclusion that the Pentamerous type was originally evolved from another system, or at least that it was selected from among other variations, that it has survived, and that it has been kept true, as being the fittest. Appendix I. Sudden deviations from normal symmetry in Neocrinoidea. A collection of instances from previous authors, with a few additions, the whole illustrating the latter portion of the paper. Appendix II. On *Marsupites testudinarius*, von Schlotheim, sp. A synonymy of the genus *Marsupites* ; it contains but one known species, and all other names must yield to this one. After the reading of this paper the President welcomed a new palæontologist to the Society, and some comments on the author's views were offered by Dr. P. H. Carpenter and Prof. Seeley.—On *Archæocyathus*, Billings, and on other genera allied thereto, or associated therewith, from the Cambrian strata of North America, Spain, Sardinia, and Scotland, by Dr. G. J. Hinde.—On the Jersey brick clay, by Dr. Andrew Dunlop.

PARIS.

Academy of Sciences, December 31, 1888.—M. Janssen in the chair.—Mean elevation of the continents and mean oceanic depths in relation to geographical latitude, by General Alexis de Tillo. Tables are given of the mean elevations and depths, in metres, for every zone of 10° of latitude from pole to equator in the northern and southern hemispheres, based on J. G. Bartholomew's hypsometric chart of the globe. The greatest mean heights and depths are found in the northern hemisphere, between 30° and 40° ; in the southern, between 10° and 30°, which also correspond to the zones of greatest atmospheric action and mean annual pressure. The mean height of the dry land and the mean oceanic depth for the globe are found to be, respectively, 693 and 3803 metres.—Observation of shooting-stars for the period August 9-11, 1888, in Italy, by Père F. Denza. The results are tabulated of the records taken at twenty-nine Italian stations, showing the number of meteors observed in the space of one hour during the periodic showers on the nights of August 9, 10, and 11, 1888. These results differ considerably for the different stations, owing to the varying state of the atmosphere, the experience of the observers, and other causes ; but, on the whole, the meteoric shower was tolerably copious compared with those of previous years.—On the volumes of saturated vapours, by M. Ch. Antoine. From the general relation established by Zeuner between the pressure and volume of aqueous vapour, formulas are here determined for the volumes of the vapour of water, ether, acetone, chloroform, chloride of carbon, and sulphide of carbon.—Propagation of the electric current on a telegraph line, by M. Vaschy. From a consideration of Sir W. Thomson's theory of propagation applied to long submarine lines, it is generally inferred that the currents are propagated along the line *without change of form*, their amplitude alone decreasing in geometric progression. An important practical consequence of this result is indicated for the working of telegraph lines, and this is stated to be also applicable to telephonic messages.—Action of sulphuretted hydrogen on the sulphate of zinc in a neutral or acid solution, by M. H. Baubigny. A current of hydrosulphuric gas passed through a *saturated* solution of zinc causes a portion of the metal to be precipitated ; but the action is arrested when the solution becomes acid to a certain point of intensity. This statement of Berzelius is correct enough, as thus expressed. But the law deduced from its generalization for all cases is here shown to be false, and completely at variance with experience, especially when the solutions are diluted.—Artificial reproduction of chromiferous iron, by M. Stanislas Meunier. After repeated failures, the author has at last succeeded in obtaining this substance (*chromite*) by combining the protoxide of iron obtained from the carbonate with the sesquioxide of chromium obtained by the reduction of the bichromate of potassa.—A chemical study of the Algerian soils, by M. A. Ladureau. A careful analysis of samples from various parts of Algeria shows a general dearth of phosphates, which explains the inferior quality of the cereals grown in that colony.—Combination of the glycol-alcoholate of soda with glycol, by M. de Forcrand. The author has already shown that many alcoholates unite with one or more molecules of a monatomic alcohol to form more or less stable crystalline compounds analogous to the acid salts, and to the numerous hydrates of the salts, bases, or acids. Here, he shows further that glycol may combine in the same way with the glycol-alcoholate of soda at equal equivalents.—On the active crystalline substance extracted from the seeds of the smooth or hairless Strophanthus of the Gaboon, by M. Arnaud. An analysis of this substance, used by the Pahouins (Fans) for poisoning their arrow-heads, shows its close affinity to the wabaine of *Acokanthera Wabaio* from Somali-land, and the strophanthine of *Strophanthus Kombé*, from Senegambia, previously described by the author.—Biological and therapeutic experiments on cholera, by M. W. Loewenthal. A series of experiments undertaken last year with the cholera bacillus seems to show that salol might be advantageously administered both as a prophylactic and a therapeutic during the prevalence of the cholera epidemic.—On a deposit of fossil bones in the Island of Samos, contemporaneous with the Pikermi age, by Mr. Forsyth Major. This find is the result of a scientific exploration of some islands in the Ægean in the year 1887. It comprises some forty species of mammals, some of

which have been identified beyond doubt with members of the Pikermi fauna. Amongst them are the Ictitherium (three species), a mastodon (*M. pentelici*), a rhinoceros, a hipparion (*H. mediterraneum*), *Sus erymanthius*, and seven antelopes. Representatives were also found of the two families of Edentates still living in the Old World; a gigantic Ruminant belonging to the giraffe family, but forming a new genus (*Samotherium boissieri*, Major); and an ostrich (*Struthio karatheodoris*, Major), equal in size to the largest members of the *Struthio camelus* group.

BERLIN.

Physical Society, November 30, 1888.—Prof. Kundt, President, in the chair.—Prof. Neesen spoke on a photographic method of registering the oscillations of projectiles. The conical end of the projectile is hollow, and at the point of it there is a small round opening; a sensitive photographic plate is placed in the cavity of the projectile. If the latter is now fired towards the shining sun, a ray of light must fall on the centre of the sensitive plate as long as the projectile moves horizontally; any deviation in a vertical or horizontal direction must produce an elongated image on the plate, and from this the deviation of the projectile from its true flight may be determined. If the projectile rotates in its flight a spiral will be obtained on the plate. The speaker had made some preliminary observations on rotating and vibrating hollow conical balls, and exhibited the negatives which he had obtained. The rotation of projectiles presents great difficulties, inasmuch as in a series of experiments the sensitive plate must not participate in the rotatory motion. The arrangements necessary for securing this result were described. Experiments as above described must be of the greatest interest in connection with the theory of projectiles, since up to the present time but little is known of the extent of the vertical and horizontal deviation during flight.—Prof. Neesen also gave an account of a stroke of lightning whose effects he witnessed while on a journey last summer. The lightning struck the centre of the roof of a two-storied house, passed along externally for a short distance, then made a round hole through the wall, and came upon the hook from which a mirror was suspended; it then passed over to the glass, fusing it at the upper corner, in the middle where the two halves of the glass joined, and at the lower opposite corner, and finally passed out again through a round hole in the wall below the glass. The way in which the latter was injured by the lightning was especially remarkable, as also was the way in which the lightning, instead of passing straight along the outside of the wall, made its way by one hole to the looking-glass in the room, and then passed out again by another similar opening.

Physiological Society, December 7, 1888.—Prof. du Bois-Reymond, President, in the chair.—Prof. Munk continued the communication which was interrupted at the last meeting of the Society, on the physiology of the thyroid.

December 21, 1888.—Prof. du Bois-Reymond, President, in the chair.—Dr. Barth gave a detailed description of his method of preparing the membranous labyrinth, and exhibited a series of preparations which had been made by this method. He intends to study fully the minute anatomy of the internal ear with the help of these preparations.—Dr. Weyl gave an account of his researches made with a view to determining the toxic or harmless action of the colouring-matters derived from tar. Inasmuch as the German Statute-book only forbids the use of two of these colouring-matters derived from tar as being poisonous, the speaker had made a systematic examination of an extended series of these colours, including such as might possibly be employed for the coloration of food-materials and might hence be a matter of dispute. He first tested the nitroso-and nitro-derivatives of benzol and phenol, and found the first to be non-poisonous, taking phenyl green as a typical representative. The nitro-derivatives which he examined—namely, picric acid, dinitro-kresol, and Martius's yellow—he found to be poisonous; the sulpho-compounds of the last-named colouring-matter, of which two are now articles of commerce—namely, naphthol-yellow S, and brilliant-yellow S—he found to be harmless. This fact points to a relationship between the chemical constitution and physiological action of these bodies. He busied himself further with an examination of the azo-colours, of which many hundred are used commercially. These fall naturally into two groups—namely, one in which the colouring substances contain only one azo-group, and a second in which they contain the azo-group twice, or as it may be called the Congo-group. These groups are distinguished physiologically by the fact that the first does not impart any colour to the urine, while the second does; they are further distinguished technically by the fact that the first group can only be used for dyeing by the help of a mordant, whereas the second does not require the use of any mordant. Dr. Weyl first investigated the action of substances containing one azo-group—namely, aurantia or imperial-yellow of commerce (hektanitro-diphenylamine); this colouring-matter was non-poisonous, and remained so after it had become soluble by the introduction of the sulpho-group (HSO_3) into its molecule. In the above researches the speaker used fibres of wool or silk, either mordanted or not according to the nature of the colouring-matter, for the purpose of determining their presence in the fluids and urine from the animals on which he was experimenting, dipping the threads into the fluids: he found that the commencing coloration of the fibres was the most certain sign of the presence of the colouring-matter.

Note.—In NATURE for December 13, p. 167, column 2, the sixth line from the bottom of the page, instead of "fall" read "rise."

AMSTERDAM.

Royal Academy of Sciences, December 29, 1888.—Mr. J. A. C. Oudemans criticized the value of the retrogradation of the plane of Saturn's ring, determined by Bessel in 1835, and generally adopted also for the plane of the orbits of the inner seven satellites of that planet. He remarked that Bessel's value 3''·848, being exceeded by its mean error, is not trustworthy. He prefers the theoretical value, for which he finds 0''·25.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Text-book of Elementary Biology: R. J. H. Gibson (Longmans).—Chance and Luck, new edition: R. A. Proctor (Longmans).—The Photographer's Diary and Desk Book, 1889 (Wyman).—A Text-book of Physiography: E. Hull (Deacon).—The Telephone: W. H. Preece and J. Maier (Whittaker).—Triennial Calendar of the Tungwen College (Peking).—Descriptive Catalogue of the Sponges in the Australian Museum, Sydney: R. von Lendenfeld (Taylor and Francis).—Corona; the Bright Side of the Universe: F. T. Mott (Williams and Norgate).—Manual of Orchidaceous Plants, Part 4, Cypripedium (Veitch).—Explosion of an Air Receiver at Ryhope Colliery (Newcastle-upon-Tyne).—The Anatomy of Megascolides australis (the Giant Earth-worm of Gippsland): W. Baldwin Spencer (Melbourne).—Journal of Anatomy and Physiology, January (Williams and Norgate).—Mind, January (Williams and Norgate).—Quarterly Journal of Microscopical Science, December (Churchill).—Quarterly Journal of Royal Meteorological Society, October (Stanford).—Geological Magazine, January (Trübner).—Journal of Society of Telegraph Engineers and Electricians, No. 75, vol. xvii. (Spon).—Journal of the College of Science, Imperial University, Japan, vol. ii. Part 4 (Tōkyō).—Proceedings of the Society for Psychical Research, Part 13 (Trübner).

CONTENTS.

THURSDAY, JANUARY 17, 1889.

THE HISTORY OF MATHEMATICS.

A Short Account of the History of Mathematics. By W. W. Rouse Ball. (London and New York: Macmillan and Co., 1888.)

THE quaint words addressed "to the great variety of readers" by the editors of the folio Shakespeare of 1623 are equally applicable to the useful compendium of mathematical history which is the subject of our review. "It is now public; and you will stand for your privileges, we know—to read and censure. Do so, but buy it first: that doth best commend a book, the stationer says. Then how odd soever your brains be or your wisdoms, make your licence the same, spare not." But, as goods are usually "bought by judgment of the eye, not uttered by base sale of chapmen's tongues," we produce our samples in the open market by making a few extracts from Mr. Ball's book.

In the opening chapter, on Egyptian and Phœnician mathematics, we become acquainted with an old Egyptian, "a priest named Ahmes," who, "somewhere between the years 1700 B.C. and 1100 B.C.," wrote, on imperishable papyrus, a book entitled "Directions for Knowing all Dark Things," which "is believed to be itself a copy with emendations, of an older treatise of about the time 3400 B.C." Remembering that this work was written certainly five hundred, and probably more than a thousand, years before the time of Thales, the first of the Greek mathematicians, and founder of the Ionian school, it must be regarded as a most remarkable production; for Profs. Cantor and Eisenlohr have shown that Ahmes had some notion of trigonometry. In his problems on pyramids, "Ahmes desires to find the ratio of certain lines, which is equivalent to determining the trigonometrical ratios of certain angles. The data and the results given agree closely with the measurements of some of the existing pyramids." But perhaps the most interesting feature of this ancient treatise is the algebraic notation employed in it, which our author describes in these words:—"The unknown quantity is always represented by the symbol which means a heap; addition is represented by a pair of legs walking forwards; subtraction by a pair of legs walking backwards, or by a flight of arrows; and equality by the sign /∠." Our own + and − first appeared in Widman's "Mercantile Arithmetic" (published at Leipzig in 1489): with him (see p. 186, Ch. XII.) they "are only abbreviations, and not symbols of operation; he attached little or no importance to them, and would no doubt have been amazed if he had been told that their introduction was preparing the way for a complete revolution of the processes used in algebra." The philosophic conception of the nature of algebra (symbolized by the legs walking forwards and backwards; a notion closely related to, if not identical with, Sir W. R. Hamilton's definition of algebra as the science of pure time) perished with its author: the mere abbreviations (+ and −) lived and flourished—but then Widman was able to print *his* book.

The first date that can be assigned with absolute precision is that of Thales. "It is well known that he predicted a solar eclipse which took place at or about the time he foretold: the actual date was May 28, 585 B.C." It marks the real commencement of the history of mathematics; for the science, now revived in Greece, was at this time neglected and completely forgotten by the Egyptians. When we read that Thales, to the utter amazement of the King and all who were present, showed them how to find the height of a pyramid, by a simple application of the theorem that *the sides of equiangular triangles are proportionals*, we may well wonder why Ahmes did not burst his mummy-case and appear in their midst with his book opened at the problems on pyramids.

From the time of Thales to that of Euclid, the knowledge of mathematical facts acquired in one generation was transmitted to the next, almost exclusively by means of oral tradition. That such was the case is mainly due to the Pythagorean secret Society. "Pythagoras himself did not allow the use of text-books, and the assumption of his school was, not only that all their knowledge was held in common, and secret from the outside world, but that the glory of any fresh discovery must be referred back to their founder: thus Hippasus (*circ.* 470 B.C.) is said to have been drowned for violating his oath by publicly boasting that he had added the dodecahedron to the number of regular solids enumerated by Pythagoras. Gradually, as the Society became more scattered, it was found convenient to alter this rule, and treatises containing the substance of their teaching and doctrines were written. The first book of the kind was composed by Philolaus (*circ.* 410 B.C.), and we are told that Plato contrived to buy a copy of it."

Now Anaximander, the immediate successor of Thales as head of the Ionian school, had the honour of teaching Pythagoras; while Eudoxus, Philolaus, and Plato, all of them received their mathematical training from Archytas of Tarentum, who was one of the most celebrated of the Pythagoreans; and "Menæchmus, who was a pupil of Plato and Eudoxus," was alive as late as 325 B.C., which brings us down to about the time of Euclid. Thus the chain of tradition connecting Thales with Euclid is complete. Its successive links can be traced in the second and third chapters of the work before us.

Among the contemporaries of Plato, Eudoxus of Cnidus deserves special notice. His biography is to be found in Diogenes Laertius, who speaks of him as an astronomer, geometer, physician, and statesman; mentions his great works on astronomy and geometry, and his minor treatises on other subjects; and refers to the fact that he *discovered curved lines.* Modern research has found out what the curves of Eudoxus were, though all his writings are lost: in our author's words, "he discussed some plane sections of the anchor ring," among them the curve which ought in future to be named after him, but is "generally called Bernouilli's lemniscate." Thus, Eudoxus (who died in 355 B.C.) anticipated James Bernouilli (d. 1705 A.D.) by more than 2000 years!

The foundation of Alexandria by Ptolemy marks an epoch in the history of mathematics. Alexander himself did little more than choose the site, and it was entirely due to Ptolemy that the city did not share the fate of at least two others of the same name whose foundation

by Alexander is duly recorded by his biographer, Quintus Curtius. What Alexandria actually became, is thus briefly and graphically described :—

"The earliest attempt to found a University, as we understand the word, was made at Alexandria. Richly endowed, supplied with lecture-rooms, libraries, museums, laboratories, gardens, and all the plant and machinery that ingenuity could suggest, it became at once the intellectual metropolis of the Greek race, and remained so for a thousand years. It was particularly fortunate in producing, within the first century of its existence, three of the greatest mathematicians of antiquity—Euclid, Archimedes, and Apollonius. They laid down the lines on which mathematics were subsequently studied, and, largely owing to their influence, the history of mathematics centres more or less round that of Alexandria, until the destruction of the city by the Arabs in 641 A.D."

It would occupy too much space to discuss, or even to enumerate, the writings of the Alexandrian mathematicians. The most precious relics they have left behind them are: the greater part of the numerous works of Euclid, many of the writings of Archimedes, the "Conics" of Apollonius, the "Almagest" of Ptolemy, the "Mathematical Collections" of Pappus, and the "Arithmetic," or, rather, the "Algebra," of Diophantus. These and other valuable pieces of work, which, like them, have reached us in a more or less mutilated condition, are reviewed in the fourth and fifth chapters of Mr. Ball's "History," in which the best editions of these classical authors are mentioned, and other sources of information concerning them are referred to. We owe the preservation of most of them to the Greek refugees at Constantinople, as will be seen from the following quotation :—

"After the capture of Alexandria by the Mohammedans, the majority of the philosophers, who had previously been teaching there, migrated to Constantinople, which then became the centre of Greek learning in the East, and remained so for 900 years. But, though its history covers such an immense interval of time, it is utterly barren of any scientific interest; and its chief merit is that it preserved for us the works of the different Greek schools. The revelation of these works to the West in the fifteenth century was one of the most important sources of the stream of modern European thought, and the history of the school may be summed up by saying that it played the part of a conduit-pipe in conveying to us the results of an earlier and brighter age."

Before the fall of Constantinople in 1453, which is alluded to in the above extract, such mathematics as were known in Western Europe were derived from Arabian sources.

The history of Arab mathematics and their introduction into Europe forms the subject-matter of the ninth and tenth chapters of Mr. Ball's book. The first of these excellent chapters tells us, in the beginning, how the Arabs, by their intercourse with Constantinople and India, in the reign of Al Mamun, the successor of the renowned Caliph Haroun Al Raschid, acquired a knowledge of the principal Greek and Hindu authors; it then gives an account of the works of the three chief Hindu mathematicians, Arya-Bhatta, Brahmagupta, and Bhaskara; and finishes with an analysis of the great treatise of Alkarismi, the first Arab mathematician, and an enumeration of the works of the most prominent among his successors from Tabit-ibn-Korra down to Alhazen and Abd-el-gehl. The account of Bhaskara is very much fuller than that given by M. Maximilien Marie in his "Histoire des Sciences Mathématiques et Physiques" (twelve vols. 8vo, 1883–88), and in other parts of the chapter some very interesting facts are mentioned, which we do not find noticed by M. Marie. Among these we may instance the solution of the cubic by Tabit-ibn-Korra, about 650 years before the time of Tartaglia, and, what is even more remarkable, the enunciation by Alkhodjandi of the proposition that the sum of two cubes can never be a cube.

The next chapter begins with the introduction of mathematics into Europe by the Moorish conquerors of Spain in the eighth century; shows how the Christians gained from them some knowledge of Arab science in the twelfth century, and, before the end of the thirteenth, were in possession of "copies of Euclid, Archimedes, Apollonius, Ptolemy, and some of the Arab works on algebra"; and brings the history of European mathematics down to the middle of the fifteenth century. During this long interval there lived only two great mathematicians in all Christendom, both of whom belonged to the thirteenth century. One was the famous Roger Bacon; the other, Leonardo Fibonacci, of Pisa, was the earliest European writer on algebra that we are acquainted with. Their biographies, though concisely written, necessarily occupy a large portion of the chapter.

The three following chapters contain the history of mathematics from the invention of printing to the year 1637, when the "Géométrie" of Descartes made its appearance. In this brief space of time, barely three-quarters of a century, owing to the labours of Pacioli, Recorde, Stifel, Tartaglia, Cardan, Ferrari, Bombelli, Vieta, Harriot, Oughtred, Stevinus, and others, vast improvements in algebra had been effected; trigonometrical and logarithmic tables had been brought to a high state of perfection by Regiomontanus, Rheticus, Napier, and Briggs; Desargues had invented the modern projective geometry; while, in astronomy, Copernicus, Kepler, and Galileo had replaced the old Ptolemaic system by a still older one (propounded by the Pythagoreans), which was now, for the first time, established on a firm basis.

Our author, as he tells us in the preface, has "usually omitted all reference to practical astronomers, unless there is some mathematical interest in the theories they proposed," and, accordingly, the name of Tycho Brahe does not figure in the above list. It would be better, in our opinion, to treat Copernicus in the same manner, rather than to do him the injustice of speaking of "his conjecture that the earth and planets revolved round the sun." Granting that "he advocated it only on the ground that it gave a simple explanation of natural phenomena," we would ask what other, or what better, proof could he have of it? It should be borne in mind that Copernicus spent the best years of his life in testing his "conjecture" by observations, and that nothing short of a firm conviction of its truth could possibly have induced him to publish it in the face of the fierce opposition which he well knew it would provoke.

With this exception, the short sketches of the lives and writings of all the mathematicians we have named are well drawn, and convey a clear idea of the importance of

their work, and of the amount contributed by each of them to the advancement of the science.

The remaining portion—about half—of the book is divided into six chapters (numbered XIV. to XIX. inclusive), in which the history of modern mathematics is briefly considered. These are so full of great discoveries and illustrious names that they must be read to be appreciated. We can only, in the limited space at our disposal, quote their titles and add some remarks.

Chapter XIV. "Features of Modern Mathematics." In this chapter, which is a sort of summary of the other five, we read that "five distinct stages in the history of this period can be discerned." Turning to the table of contents, we find the five stages thus described: (1) "invention of analytical geometry and the method of indivisibles," (2) "invention of the calculus," (3) "development of mechanics," (4) "application of mathematics to physics," (5) "recent development of pure mathematics." The mere remark that each of these might be made the title of a bulky volume, will show at once the enormous extent and importance of modern mathematics.

Chapter XV. "History of Mathematics from Descartes to Huygens." The principal names in this chapter are Descartes, Cavalieri, Pascal, Wallis, Fermat, Barrow, and Huygens. In many of their writings may be found the germs of those ideas which have since been developed in the infinitesimal calculus. Especially would we mention Cavalieri's *method of indivisibles*, of which our integral calculus is the modified descendant, and Barrow's method of drawing tangents to curves, substantially the same as that given at the beginning of any modern differential calculus. Full explanations of both methods may be found in the present chapter.

The history of modern mathematics dates from the publication of the "Géométrie" of Descartes, and we wish to call attention to a bibliographical point connected with it. M. Marie ("Histoire," &c., t. iv. p. 20) speaks of "quatre traités separés: 'Le Discours de la Méthode,' 'La Dioptrique,' 'Les Météores,' et 'La Géométrie,'" all of them published in 1637; Mr. Ball (p. 241) says that "Descartes's researches in geometry are given in the third section of the 'Discours.'" We cannot positively say which is correct, but our impression is that we have seen a copy of the separately-published "Géométrie." The point is of small importance, but it should be cleared up in subsequent editions.

Chapter XVI. "The Life and Works of Newton." There are two sections—one devoted to the life, the other to an analysis of the works, of our English Archimedes; his three capital discoveries—fluxions, the decomposition of light, and universal gravitation—will occur to most of our readers. Most of the well-known facts relating to Newton's private and public life are mentioned in this chapter, together with some others that have only recently come to light.

Chapter XVII. "Leibnitz and the Mathematicians of the First Half of the Eighteenth Century." The following sentence occurs in the opening paragraph:—

> "Modern analysis is, however, derived directly from the works of Leibnitz and the elder Bernouillis; and it is immaterial to us whether the fundamental ideas of it were obtained by them from Newton, or discovered independently."

It forms a fitting sequel to the tale told in the preceding chapter of the celebrated controversy between Newton and Leibnitz.

The present chapter is in three sections: (1) "Leibnitz and the Bernouillis," (2) "The Development of Analysis on the Continent," (3) "The English Mathematicians of the Eighteenth Century." The two greatest French names in the chapter are those of Clairaut and D'Alembert; the two greatest English ones, those of Taylor and Maclaurin. Matthew Stewart succeeded Maclaurin as Professor at Edinburgh, and was "almost the only other British writer of any marked eminence in pure mathematics during the eighteenth century." After recounting his chief works, our author proceeds to say:—

> "These prove him to have been a mathematician of great natural power, but, unfortunately, he followed the fashion set by Newton and Maclaurin, and confined himself to geometrical methods."

This sentence gives the history, in epitome, of the decline and fall of British mathematics in the last century.

Chapter XVIII. "Lagrange, Laplace, and their Contemporaries." There are four sections: (1) "The Development of Analysis and Mechanics," (2) "The Creation of Modern Geometry," (3) "The Development of Mathematical Physics," (4) "The Introduction of Analysis into England." The greatest foreign name in this chapter (we single it out from a number of others) is that of Euler; the greatest English one is possibly that of Thomas Simpson, who seems to be rather harshly treated by being allotted only three lines in a footnote, when others of less ability are noticed in the text.

In Section 4 we read: "The introduction of the notation of the differential calculus into England was due to three undergraduates at Cambridge—Babbage, Peacock, and Herschel—to whom a word or two may be devoted."

Doubtless the success of the movement was largely due to their efforts, but the initiative was taken by Woodhouse in 1803 (see J. W. L. Glaisher on the "Tripos," Proc. Lond. Math. Soc., vol. xviii. p. 18). The name of Woodhouse is surely as deserving of mention as the other three.

Chapter XIX. "Recent Times." The author begins with a long list of names well known in the mathematical world. This list, however, "is not and does not pretend to be exhaustive." He then classifies the writers he has enumerated "according to the subjects in connection with which they are best known, arranging the latter in the following order: elliptic and Abelian functions, theory of numbers, higher algebra, modern geometry, analytical geometry, analysis, astronomy, and physics."

The section on the theory of numbers is, in our opinion, the best. It contains biographies of Gauss and the late Prof. Smith (about four pages being allotted to each), and mentions the researches of Cauchy, Liouville, Eisenstein, Kummer, Kronecker, Hermite, Dedekind, and Tchebycheff.

We may now say with old Martial—

> "Ohe jam satis est, ohe libelle:
> Jam pervenimus usque ad umbilices."

But we have yet to record the impression left by the perusal of the entire work. The most desirable thing in a book of reference is that the reader should be enabled

to find his way readily to any part of it. In the one before us this want is met by an admirable index, and an equally complete table of contents, and by the liberal use of clarendon type in the body of the book. The printing is clear and generally correct, but we notice the following errata:—

P. x. line 8 from top, for "1885–1888" read "1883–1888."

P. 110, in the heading of Chapter VI., for "641–1543" read "641–1453."

P. 168, line 4 from bottom, for "Act iv. sc. 3" read "Act iv. sc. 2."

P. 358, line 8 from bottom, for "1728" read "1738."

All the salient points of mathematical history are given, and many of the results of recent antiquarian research; but it must not be imagined that the book is at all dry. On the contrary, the biographical sketches frequently contain amusing anecdotes, many of the theorems mentioned are very clearly explained, so as to bring them within the grasp of those who are only acquainted with elementary mathematics, and there is a very interesting account (in a footnote) of the early history of the Universities of Paris, Oxford, and Cambridge. For those who wish to study mathematical history in detail there is a long list of authorities at the beginning, and many references to other works are made in different parts of the book. We would suggest that in future editions reference should be made to "Les Fondateurs de l'Astronomie Moderne Copernic—Tycho Brahé, Képler, Galilée, Newton," by Joseph Bertrand (8vo, Paris, n.d.), and to the article "Viga Ganita" in the "Penny Cyclopædia" (which contains the opinions of Colebrooke, the translator of the "Lilavati," &c., on many points connected with Hindu mathematics).

Finally, we would suggest that the following motto should be printed on the title-page of the second edition:—

"Habetis originis ac progressionis mathematicæ historiam brevem. Ex qua matheseos antiquitas, præstantia, ac dignitas apparet."

The quotation is taken from the concluding paragraph of the "Historica Narratio" prefixed to Andrew Tacquet's "Euclid" (2nd ed., by Whiston, 1710). It describes perfectly the contents of the present treatise.

Mr. Ball promises us a supplementary volume containing a list of mathematicians and their works, which is to be as complete as possible. It will be a most important contribution to mathematical bibliography, and we sincerely hope that the reception that this volume meets with will encourage him to write the supplement.

THE BUILDING OF THE BRITISH ISLES.

The Building of the British Isles: a Study in Geographical Evolution. By A. J. Jukes-Browne, B.A., F.G.S. (London: George Bell and Sons, 1888.)

IT is now thirty-three years since Godwin-Austen, in a paper which glows with the instinctive perception that is one of the marks of genius, suggested to geologists an application of their science which lifts it out of the region of technicalities, gives it a human interest, and attracts all those who care to follow the long chain of events of which the present state of things is the outcome. It was an attempt to go back to Mesozoic and Palæozoic days, and mark out the main outlines of the physical geography of Great Britain and the adjoining parts of Europe during those epochs. To enable its conclusions to be more easily grasped, the paper was accompanied by a map, almost bewildering in its complexity and somewhat hazy in its outlines, but full of the masterly generalization that marshals into one compact body a crowd of isolated facts, and of the intuition that foresees the complete meaning of imperfectly ascertained data.

Many a geologist has since been tempted to try his hand at similar tasks, but few have mustered courage, when it came to the point, to embody their conclusions in a map. And no wonder: everyone who has speculated in this direction knows how easy it is to clothe his conceptions in words, and soon finds out how hard verbal descriptions of physical geography are to follow. So he becomes keenly alive to the fact that, if he wishes to be listened to, he must make the road easy by presenting his restorations to the eye in the pictoral form of a map. But if he be haunted by any sense of accuracy, and any horror of vagueness and hasty reasoning, he finds himself beset on all sides, when he begins to plot out his map, with uncertainties and hesitations that give him pause. It may be easy to say that land lay on this side and sea on that, but when a coast-line is actually to be laid down, though it may be possible to fix the limits between which it must lie, these limits are often so wide apart that the feeling of uncertainty as to the actual position of the boundary becomes unbearable, and the prospect of making a map that shall be even approximately accurate grows hopeless. Worse still is it—and this not unfrequently happens—when there are not even bounding limits, and the coast-lines can be no better than such guess-work as rashness delights in and the logical temperament abhors.

But even those who realize most clearly the difficulties of the task of making maps which show the distribution of land and sea during past geological epochs, welcome with keen delight attempts, such as those in the book before us, which are made in the right spirit; and it would ill become me to carp at the author's restorations, even were they less satisfactory than is the case, for I believe that, in noticing a former work of his, I ventured to take him to task for not having appended maps to his verbal descriptions of the old physical geography of our islands.

Mr. Jukes-Browne has explained, in the introduction, the principles which have been his guide; and the words with which he concludes his opening remarks show how fully he is aware of the difficulties that attend the task he has undertaken, and how much uncertainty hangs over many of his results. Even where we cannot agree with him, we feel sure that he has never been hasty and has spared no pains to arrive at the most probable conclusions.

With commendable caution no attempts are made to depict on a map the physical geography of Archæan and Cambrian times; but preference is given to Prof. Hull's conjecture that the great mass of Cambrian land "lay to the north-west of Europe, and occupied a large part of what is now the North Atlantic Ocean." The words "large part" are vague, but a partiality for filling up the

Atlantic on slight provocation is, I fear, rife among us, and ought not to be encouraged.

The Ordovician and Silurian maps are probably as near approximations as the present state of our knowledge allows—maybe as we shall ever attain to. In the Lower Old Red Sandstone map, the Old Red of South Wales is represented as having been formed in a bay of the Devonian sea. This view does get over some difficulties. If we suppose it formed in a fresh-water lake, we must admit that the barrier separating the lake from the Devonian sea was narrow, and, as our author remarks, there is no independent evidence for the existence of such a barrier. The absence, as far as we know, of any marine fossils tells the other way, but the district is yet geologically almost a *terra incognita*, and we must wait before the question can be settled. In the same way it must be confessed that it is hard to see on what grounds the Glengariff Grits are classed as marine; but here again the "retort courteous" might be, "What reason have you for thinking them fresh-water deposits?" It is too large a question to go into here.

The Carboniferous map I turned to with the greatest curiosity, for it so happens that years ago I was rash enough to try my hand at a similar production. The thing has thrust itself upon me many times since, and each time I have seen something in it that dissatisfied me, and it has been touched up and tinkered till now I hardly recognize my own child; and till I know my own mind, it would be hardly fair, even supposing it possible, to attack another man for differing from me. Really, the two maps have more in common than appears at first sight; and on some points of fundamental importance—the land-locked character of the Carboniferous sea for instance—Mr. Jukes-Browne and I are in complete accord. I should not have ventured on this bit of personal reminiscence, if it had not been that it seemed to me that it may possibly be typical. Put a number of equally qualified men to construct one of these geographical restorations, and the result will probably be this: there will be some few points on which all must agree; but the data for settling details will be so vague, that no two maps will be alike in their minor points. Even if this be so, it by no means proves the work to be unprofitable; but it is as well to bear this in mind when comparing two independent restorations.

Space will not allow of detailed criticism of the series of maps with which the book is lavishly illustrated; but all readers will gratefully thank the author for the pains he has taken to render such effectual help to them in following his reasoning. Every chapter bears the mark of patient and conscientious work; and though in a book of this size no more than an abridged and concise statement of many of its facts can find a place, its suggestiveness will insensibly lead the real student to the original papers of which it is an epitome, and open for him a wide field of reading.

The chapter on the Pleistocene epoch is one on which the author has evidently spent much pains, but it seems to me the least satisfactory in the book. With much that he says I heartily agree. I cannot help feeling that some of our most eminent glacialists have ridden their theories rather hard. That the Scotch Till is a *moraine profonde* seems to me the only hypothesis yet put forward which gives anything like a satisfactory explanation of the origin of that deposit; but I am not prepared to admit a like origin for all the so-called Boulder-clays: most of those who have studied on the ground in detail the Boulder-clays of the plains of Lancashire and Cheshire have come to the conclusion that they are submarine, and that their boulders have been supplied by floating ice. Some of the objections which Mr. Jukes-Browne urges against the ground-moraine theory, however, do not seem to me serious. He cannot understand how it is that an ice-sheet could groove and polish the rocks and form a ground-moraine at the same time. There are many ways out of the difficulty. Ice-scratching is most conspicuous on high ground and steep slopes, where there is little or no Till. It is true that it is far from uncommon on lower and flatter ground, where it is covered and indeed preserved by a coating of Till. Here it may be the first work of the ice-sheet before much *débris* had been dragged down from the hill country; but we must also bear in mind the probable character of a ground-moraine: packed closely by the weight of the ice above and frozen hard, it would be very different from the imperfectly consolidated mass we see now: rather it would act as a solid whole, and, as it was dragged along, would be quite capable of effecting a large amount of abrasion. The alternative which is suggested involves the floating of the ice-sheets bodily over wide extents of sea; but, as far as we know, ice-sheets do not float as a whole when they push their way out to sea, they break up into icebergs. We may picture to ourselves the probable action of an ice-sheet somewhat after this fashion. While descending slopes even moderately steep, it would push before it and drag beneath it any loose *débris* that it found ready made to its hand or that it had itself torn off the surface. But here its motive power would be sufficient to carry with it all the loose matter; consequently here no Till would be formed, unless the sheet happened to encounter a gorge in its path. In such a case the stones and dirt would be driven into the hollow till they filled it up, and the ice would then ride over it. When the ice-sheet reached flatter ground, its dragging power would be seriously diminished: it would probably at first heap up the *débris* into a mound or ridge in front of it: this mound after a time it would override, and flatten and spread out its materials; by a continuance of the process a sheet of Till would be spread over the lowlands. Of course here too any valley that lay athwart the path of the ice would be filled up. Thus would be produced exactly the distribution of the Till which occurs: in the hill-country little or none except as filling in valleys; over the plains a broad sheet, and great thicknesses in the valleys of the low country. So that when our author states that Prof. James Geikie's views might be accepted "if the Boulder-clay was found to fill in lake-like hollows," he is describing very nearly an essential feature in the actual manner of its occurrence.

Having now discharged the functions of the critic, and pointed out what appear to me some weak points, I will only add that if I seem to have been scant of praise, it is because there was no need. The book recommends itself.

A. H. GREEN.

OUR BOOK SHELF.

The Civilization of Sweden in Heathen Times. By Oscar Montelius, Ph.D. Translated from the Second Swedish Edition, by the Rev. F. H. Woods, B.D. (London: Macmillan and Co., 1888.)

EVERYONE who knows anything of archæology is aware that a book on the subject by Dr. Montelius is sure to be worth reading. The work translated by Mr. Woods ranks among the best existing summaries of the antiquities of particular countries. The author begins with the Stone age, and passes on, through the Bronze period, to the various stages of the Iron era. For some reasons it might perhaps have been better if he had reversed the order, taking first a group of antiquities the date of which can be approximately fixed, and working his way back to more remote times. This plan has been adopted, with excellent results, by Mr. Anderson, in his study of Scottish antiquities, and by Dr. Lindenschmidt in the work he is writing on the antiquities of Germany. The method chosen by Dr. Montelius is, however, favourable to clear, popular exposition, and he has made excellent use of the opportunities it has provided for him in this direction. He has a dread of far-fetched, fanciful explanations, and, at every stage of the story he has to tell, is careful to show that his statements are in strict accordance with facts. His account of the Bronze age is particularly interesting, but all that is essential to the comprehension of the remains of the Stone and Iron ages in Sweden he also presents with remarkable conciseness and lucidity. The second Swedish edition, of which the present volume is a translation, was published in 1878. Many additions were made by the author to a German translation, which appeared in 1885; and these additions, with others specially provided for the English rendering, have been incorporated by Mr. Woods in his interesting volume. Mr. Woods has done full justice to the original by his vigorous and lucid style, and the notes he has added—especially those relating to the "Corpus Poeticum Boreale," edited by Dr. Vigfusson and Mr. F. York Powell—will be welcome to all serious students of archæology. The work, we may add, is well printed, and the value of the text is greatly increased by a large number of admirable illustrations.

The "Indispensable" Hand-book to the Optical Lantern. Compiled and Edited by W. D. Welford and Henry Sturmey. (London: Iliffe and Son, 1888.)

THIS is mainly a catalogue of lanterns, accessories, and slides, one section of the book being devoted to each. Each section commences with brief general remarks, and is followed by a price list of the various pieces of apparatus concerned, as manufactured by different firms. The details of each piece of apparatus are described, and in some cases special remarks are made. All the important makers are represented, and their full addresses are given.

The classified descriptive catalogue of the various sets of slides in the market will perhaps be the most useful part of the book, seeing that the possessor of a lantern is likely to be most interested in determining what he shall exhibit. This catalogue is such that one can immediately ascertain full particulars relating to any set of slides, without waiting to see them before purchasing.

The illustrations which crowd the book are of a very high class, notwithstanding the fact that most of them are used for advertising purposes by the firms whose productions they represent.

To anyone about to purchase a lantern, or anything concerned with one, the book is fully entitled to its claim to be indispensable. We can further confidently say that it will interest and prove useful to each one of the ever-increasing number of persons who use the lantern either for purposes of instruction or entertainment.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Alpine Haze.

QUESTIONS of nomenclature are of some importance, and of some difficulty, in subjects not thoroughly investigated. M. Antoine d'Abbadie's last letter (NATURE, January 10, p. 247) is so interesting, and, from the linguistic and bibliographical points of view, so exhaustive, that it is with reluctance that I point out why my opinion slightly differs from his on nomenclature, having already indicated a difference of opinion as to the physical nature of the phenomenon itself. I gave, as a matter of course as well as of courtesy, the title of "Alpine Haze" to my last communication, out of deference to Prof. Tyndall, and shall continue to do so until I know Prof. Tyndall's final opinion, but deference to the highest authority cannot alter my belief that this title is not a fortunate one—a belief confirmed by Antoine d'Abbadie's own evidence. Ludolf's definition is good, but what I required was a simple English term for the use of non-scientific observers, and of some careful scientific observers like Dr. Burder. "Dry haze" (together with the specific term, of which it is the translation) begs a more serious question than is begged by "dust haze."

I am also inclined to think that the un-scientific English "dry haze" may be unfortunately applied to the ordinary haze of comparatively dry weather which Dr. Burder describes. There is, I suppose, little doubt that this latter common haze is composed principally of water-particles (usually with some admixture of smoke and dust), *pace* all the hygrometers in the world. It does not differ from mist, and does not differ from fog, except in amount. W. CLEMENT LEY.

A Remarkable Rime.

DURING cold fogs the accumulation of ice on the branches of trees due to the contact of water particles with solid substances, frequently causes damage to timber in the Continental forests: not often, I think, in this country. No snow has fallen here until to-day since October 2, 1888, but anticyclonic frost has been on several occasions accompanied by fogs of unusual density. During the frost of last week, ice-crystals of about 2 inches in length, at first very hard and adhesive, were formed on the windward (south-south-west) side of all exposed objects, but particularly on metal, even at no greater height than 3 or 4 feet above the earth's surface. This is a common sight on the higher hills even in the British Isles, but at this altitude (460 feet above mean sea-level) appears to be rare. The result has been great injury to timber, and a great "wind-fall," without much wind, to the tenant-farmers. Of deciduous trees, the ash seems to have suffered the most, while little damage, so far as I have observed, has been received by the ornamental conifers which usually suffer so much from snow. It is impossible to estimate, with much approach to accuracy, the amount of moisture drawn from the atmosphere in this rime, but during the thaw we measured $4\frac{1}{2}$ inches of ice-crystals on the ground on the leeward side of a rather spare elm-tree 39 feet in height, while the boughs above this surface, on the leeward side, still retained their exquisite robe of rime. ANNIE LEY.

Ashby Parva, Lutterworth, January 12.

Mass and Inertia.

MR. WORTHINGTON is rather unkind in blaming the chemists for perhaps somewhat pedantically doing that which is right, while he encourages his new friends the engineers in continuing to do that which is wrong.

If he could point to a handy and permanent force, independent both of age and position, which could be boxed up in small compass and handed down to posterity with perfect security against alteration, and with complete certainty of precise accuracy in Auckland, or wherever the future capital of the race may be, there might be something to say for his proposal to adopt force as one of the fundamental units instead of mass. Otherwise, there is practically nothing to be said for it.

Mr. Worthington speaks as if we were anxious to do away with a student's familiarity with force as a push or a pull. This shows that he does not appreciate our position.

I even venture to assert that in what he says concerning mass and inertia he is not so absolutely clear in his own mind as it is desirable for a reformer to be. May I suggest to him that the "inertia" or "inertia-reaction" of a lump or mass of matter—that which is measured in an experiment, and the only thing that can be measured in an inertia experiment—is $m\frac{dv}{dt}$; and that the coefficient of the otherwise measurable kinematic factor in this quantity is properly called "the coefficient of inertia," but is, for brevity, styled "mass," and is taken as a measure of the quantity of matter in the body, because, experimentally, it is found to be absolutely unalterable by every physical and chemical process except those which change the amount of matter in the lump. Fancy making our standard of quantity of matter depend upon the approximately determined gravitative attraction of some arbitrarily selected planet at some arbitrarily selected spot near its present surface!

Sometimes, indeed, m is briefly called merely "inertia," just as the coefficient $\frac{V-V^1}{C}$ in Ohm's law is for brevity styled "resistance"; but the full names of these quantities are "coefficient of inertia" and "coefficient of resistance," respectively. In the case of friction the full name is usually given. With junior students it is clearest to give the full names in every case; just as it is much clearer with them to avoid the misleading abbreviation specific heat, and to use the full phrase specific capacity for heat. OLIVER J. LODGE.

Liverpool, January 14.

A Hare at Sea.

AMONG the notes published in NATURE for December 27, 1888, is an account of a hare swimming across a river; perhaps the following account of a hare taking to the sea may be of interest. In October 1887, I was a member of a shooting party staying near Auchencairn on the Kirkcudbrightshire coast, where for miles the waves of the Solway beat on red sandstone cliffs, broken here and there by small bays, where the burns run down to the sea through little glens. One day I had left the others and was standing among the seaweed-covered boulders of such a bay, when the sounds of a course reached me from a hill-side a quarter of a mile or more away, and presently I saw hare and greyhounds coming down to the shore; they ran close past where I was standing, and then to my astonishment the hare deliberately entered the water and swam out to sea.

I could not persuade the greyhounds to follow, though one was so close that, if she had done so at once, she could have caught the hare without swimming, as the latter was out of her depth directly and swam very slowly. The sun was shining very bright on the water, and it soon became very difficult to keep the hare in sight, as her head only showed now and then on the top of a wave, and about a hundred yards from shore I saw her for the last time, though I stayed about the place a long while.

This hare was perhaps hard pressed, still I could see no reason why she should not have run along the shore to the march dyke, which was close to, and where she would probably have made good her escape. W. J. BEAUMONT.

Sandiway, Northwich, January 13.

THE ARTIFICIAL REPRODUCTION OF VOLCANIC ROCKS.[1]

ORIGINALLY, the study of the crust of the earth was purely utilitarian: it seems to have been at first forced upon man by the necessity of exploring the strata in order to extract metallic ores, constructive materials, and combustible minerals.

To anyone who glances at the history of the sciences, it becomes evident that they all owe their origin to some useful and practical aim, and that from this initial phase they have passed through a regular development: this progress, so far as geology is concerned, I shall proceed to sketch.

Man, then, commences to explore the depths of the earth in order to extract the materials which may minister to his wants. At first he works without rule; but as the miner's art is developed, method is introduced into the search for mineral wealth, and he observes the conditions under which useful minerals and rocks occur in the bosom of the earth. These observations, at first merely empirical and local, gradually become generalized, and thus lead to a recognition of some of the leading features in the architecture of our planet. On digging into the earth, it soon becomes evident that the world was not made at a single stroke, but owes its formation to a succession of epochs.

It follows, therefore, that, in order to interpret the history of the earth, and the operation of the agencies which have taken part in its formation, it is necessary to study the living world, and to investigate the present condition of our planet. In comparing the various strata of the earth with the deposits which are in course of formation under our own eyes, we realize the conditions which have presided at the formation of the stratified rocks of ancient geological periods. It is thus that, by the analysis of facts, and by induction which generalizes the observations, our knowledge of the crust of the earth enters on a new and truly scientific phase. We start by attempting to discover practical rules for the guidance of the miner, and we are gradually led to decipher the history of the earth.

In this reconstruction of the past history of our planet we are guided by a fundamental principle—namely, that the essence of the forces which have acted upon the earth has never changed. We ought, then, to seek in geological epochs for traces of only such phenomena as are of the same nature as those which we can witness to-day, and submit to direct observation.

Since geologists commenced, towards the close of the last century, to apply the inductive method to the study of the mineral masses which form the crust of the earth, to their architecture, and to the organic remains embedded in the rocks, a vast collection of documents has accumulated, bearing upon the history of our planet. During this period, Geology has made such immense progress that she need not envy the older branches of natural science.

Let us see how, in applying this analytical method and relying on induction, geology interprets the formation of the rocks. Rocks, we know, are the solid mineral masses which constitute the earth's crust. Observation teaches us to recognize two groups. The first are characterized by an arrangement in beds or strata: these are the sedimentary rocks. The second group, which does not present this stratified arrangement, comprises rocks of volcanic character, with a massive structure. These differences in the structure and composition of the two great lithological groups lead us to regard them as having been formed under special conditions, which have left their imprint upon each group.

We see the sedimentary rocks in the course of formation when we observe how detrital matter is rolled about by stream and wave, and how such waters deposit pebbles, sand, and mud upon their beds. After the death of the organisms which inhabit these waters, their skeletons or their shells become mingled with the mineral deposits, and with them build up sedimentary masses. The minerals so deposited assume, by successive accumulation, a stratified arrangement. All their constituent particles were originally isolated grains, and still retain traces of their origin: they are either the *débris* of pre-existing rocks or organic exuviæ, which, by physical and chemical processes, may become subsequently consolidated.

Let us now compare these modern sedimentary deposits, characterized by a stratified arrangement, and

[1] A Lecture delivered in French at the Royal Institution, on Friday, May 18, 1888, by M. Alphonse Renard, LL.D., Hon.M.R.S.E., Corr.G.S., Curator of the Royal Museum, Brussels. Translated by F. W. Rudler.

by the detrital nature of their constituents, with certain geological strata. We observe on continental surfaces masses of rock of geological antiquity, which offer close analogy in aspect and structure to the materials which are deposited under our very eyes by fluviatile and marine action. This comparison leads us to regard the old stratified rocks as having been formed by the operation of the same causes, and we hence consider them to be deposits of submarine or fluviatile origin. Water is therefore the agent which is everywhere at work in the formation of sedimentary or detrital masses.

The second group, of which we have specially to treat, includes the massive rocks—those which may be observed in course of formation during volcanic manifestations. The molten matter, vomited from the crater or injected into the sedimentary beds, consolidates on cooling. The constituents of the lavas are crystalline individuals developed at the expense of the surrounding magma. These crystals are not detrital, in the sense in which we have just used that term. Speaking in general terms, we may say that the eruptive masses do not present the stratified arrangement of the sedimentary rocks; but in place of the original horizontality and the regular superposition of the stratified beds, the lavas offer an appearance which indicates the thrust from below upwards, to which they were subjected during eruption. Finally, the massive rocks are destitute of organic remains.

Let us now compare the contemporary volcanic rocks with certain ancient crystalline rocks—granites, porphyries, trachytes, and basalts. We observe that these present close analogy in structure and composition to the products of active volcanoes. From the possession of these common characteristic features, we may conclude that the massive rocks, which traverse the strata, have been, like the modern lavas, injected from below, and share with them an eruptive origin.

But while we see the sedimentary rocks in course of formation under our eyes, and can closely follow the conditions which preside at their origin—the work being accomplished, so to say, in broad daylight—the eruptive masses are elaborated in the depths of the earth; their genesis is to some extent enshrouded in mystery, and our vision fails to penetrate the vast subterranean reservoirs where the molten masses are formed, and whence they are projected in volcanic eruptions.

Here the paths of direct observation are partially closed against us. Neither the finest analysis nor the strictest reasoning can supply the missing data; they are powerless to show us all the causes which are at work in the formation of the eruptive rocks.

In order to resolve our doubts, and to control and complete our observations, we therefore attempt to reproduce the volcanic rocks artificially; to form them synthetically. Armed with the results of observation which must serve as our guide, we endeavour by scientific manipulation to imitate the products of Nature. The science of the earth, previously analytical, enters thenceforth upon its final phase—that of synthetic experiment.

These attempts to imitate Nature, guided by the intelligence of man and executed by his hand, enable him, though limited in resource, to obtain results which offer analogy to that which he desires to investigate; he can direct and regulate the progress of the phenomena, can note with exactitude their relations, and can vary at will the conditions under which they arise. The knowledge acquired by observation, analysis, and reasoning, is thus, according to Bacon's expression, "tested by steel, and by the fire of experiment."

We have now indicated in broad outline the three great steps in the progress of our knowledge of the earth's crust. We have watched it at its birth, when it was limited to utilitarian ends; we have followed it later in its course, when, guided by observation and reasoning, it rose to the dignity of a science. Geology, entered now on its last phase, is transformed into an experimental science.

We shall now show, in studying the artificial reproduction of recent volcanic rocks, how powerfully the resources of the laboratory can assist the direct observation of Nature. But before explaining the methods employed in the synthesis of modern volcanic rocks, we must briefly summarize our knowledge of the constitution and formation of these volcanic masses, as derived from analysis and observation. It is to these natural lavas that our synthesis must be directed; they form the models which we must copy, and it is therefore necessary to become thoroughly acquainted with them in order that we may imitate them in their closest details.

Let us, then, recall what we know about lavas and the conditions of their formation. Without dwelling on these grand manifestations of the internal forces of the earth, or the succession of phenomena in an eruption—those formidable disturbances which shake the volcano to its very base, and eject pulverized vitreous matter and red-hot stones—we may remark that in the midst of such a cataclysm, the crater and the flanks of the mountain, rent by pressure of the matter seeking to escape, allow floods of lava to flow forth, and this matter, rolling down the mountain, slowly solidifies upon its slopes.

The chief feature of an eruption is the emission of lava or streams of molten matter escaping from the crater. We may best compare the lava, in general terms, to a glass liquefied under the influence of the high temperature which prevails beneath the solid crust of the earth. Direct observation of the temperature of the liquefied lava at the moment of its emission from the crater is surrounded by dangers which few observers dare to encounter. Hence we possess on this point only approximate observations. But certain volcanoes, where the outflow of lava is never violent, and which are in a state of moderate and permanent activity, as in the Island of Hawaii, have allowed the intrepid observer to approach sufficiently near to estimate the temperature of the molten mass. It has thus been found that the temperature varies between 1000° C. and 2000° C. But on the outflow of the lava the temperature of the surface is rapidly lowered, the liquid sheet becomes incrusted with scoria, more or less thick, beneath which the fused matter flows like a stream, having a temperature of about the melting-point of steel. It is this mantle of scoria which hinders radiation, and enables the subjacent mass to retain for a long time a certain amount of viscosity.

Further on we shall discuss the observations on the phenomena of crystallization presented by this erupted matter, still liquid or viscous, but ready to congeal. Let us, however, first study some of the essential characteristics of the structure and composition of lavas. These erupted products are in many cases vesicular and scoriaceous; while in others they appear as homogeneous vitreous masses, more or less dark-coloured, in which the naked eye fails to detect any isolated mineral. Sometimes, again, this mass is charged with minerals, more or less numerous, which seem to squeeze aside the vitreous paste which cements them together. These embedded minerals, when perfectly developed, present regular polyhedral forms, constant for each species; they are, in fact, *crystals*—that is to say, perfect individuals of the mineral world. They have drawn from the original vitreous magma the chemical elements of which they consist, and which have grouped themselves according to their affinities; just as we observe that in a liquid saturated with a salt, crystals are developed, consisting of the substance which was dissolved in the mother-liquor.

Mineralogy teaches us to determine the mineral species which crystallize in lavas; chemical analysis, in turn, furnishes us with valuable information respecting the composition of volcanic products. If we subject the eruptive rocks to chemical processes, we find that they all contain

more or less combined silica, which may reach to upwards of 65 per cent. of the mass: these are the acid or light lavas. Thence we pass, by various gradations, to the basic or dense lavas, in which the proportion of silica, gradually diminishing, does not reach more than 55 or even 45 per cent. This silica does not exist in a free state in modern lavas, but is combined, in the form of silicates, with alumina, iron, lime, magnesia, potash, and soda.

In the slags of metallurgical works we find products which present close analogy to those of volcanoes, both in composition and in mode of formation. These artificial scoriæ are, like lavas, formed of silicates; and another point of resemblance between them lies in the fact that we may regard both as the scum of a metallic nucleus, of which they form the upper zones. The differences in composition result from the fact that they are derived from zones of greater or less depth.

Our knowledge of eruptive rocks came to be enriched in an unexpected manner by the application of the microscope to lithology. We need not here recall the almost marvellous results obtained by this method of investigation, inaugurated by H. C. Sorby; but we may say, in a word, that the microscopic analysis of rocks has changed the face of petrography. Let us confine our attention to some of the conceptions relating to modern volcanic rocks, as revealed by these new methods—methods which, in delicacy, in certainty, and in elegance, are unsurpassed in any other branch of natural science. Not only have they enabled us to verify and control hypotheses, but they have led to the remarkable discoveries to which I am about to refer.

The eye, assisted even by the most powerful lenses, could recognize in lavas only those minerals which appeared in rather large crystals; chemical analysis generally gave merely the composition of the total rock, and its mineralogical composition was only suspected. The intimate texture of the rock remained impenetrable; it was impossible to determine with certainty the order in which the constituents of the molten mass had solidified; neither could we trace the various states through which the crystals had passed—their germs, primordial forms, and skeletons—or the aspect of the rock at different stages of its development.

Let us now apply the microscope to the examination of a thin slice of lava, rendered transparent by polishing. The lavas, as we have said, may be compared to vitreous masses; but whilst in our artificial glasses we seek to obtain a pellucid and homogeneous product, the liquefied matter of volcanoes, when it flows forth, already contains certain differentiated products. The glass which contains these bodies may be regarded as the residue of the crystallization, whence the numerous crystalline individuals have extracted their constituent elements. In the black, brilliant, volcanic glasses, apparently opaque and destitute of crystallization, the microscope discovers a world of mineral forms. It shows us their various states of growth, and the arrest of their development consequent on the more or less rapid consolidation of the mass. It is especially in those rocks which, like obsidian, have preserved almost wholly their vitreous character, and are homogeneous to the naked eye, that we find the rudimentary crystals of curious form, representing the first step in the passage of the amorphous matter to the crystalline condition. Owing to the rapidity with which the vitreous paste consolidated, the crystals were unable to grow, and their development was sharply arrested. Hence the origin of these embryonic crystals which abound in natural glasses, and which we designate as *crystallites*. Analogous crystallites are produced in blast-furnace slags, which have close relations to the matter of lavas. Their common origin is betrayed by certain family likenesses which the microscope reveals. The slags, examined in thin sections, exhibit rudimentary crystalline forms, similar to the crystallites of volcanic glasses.

But usually the crystals have not remained in this embryonic state. If the lava has not been too rapidly cooled, the molecular movements are retained, even in a semi-liquid mass, and the paste develops crystals of minute dimensions, called *microlites*. These microscopic crystals are formed in the heart of the vitreous magma during its slow consolidation. Notwithstanding their infinite minuteness, these small polyhedra exhibit with marvellous exactitude all their specific characteristics, such as we are familiar with in much larger crystals, and which we should not expect to find in lavas. They often form, by their interlacement, a beautiful network in the paste, and give to the rock in which they are developed a *microlitic structure*.

The dimensions of these microlites, invariably microscopic, and their arrangement, prove that they may be referred to a period of disturbance; that they were formed, indeed, at a time when the lava, though still in motion, was solidifying. They separated from the magma during the very act of outflow or eruption.

Besides these microscopic crystals and these groups of crystallites, which belong to the last stage of consolidation, the lava contains also a supply of larger crystals, more fully developed, and in many cases recognizable by the naked eye. These have been formed under calmer conditions, analogous to those presented by a tranquil fluid in which crystallization is proceeding slowly. They were formed in the molten magma when it was still inclosed in the subterranean reservoirs. This slow growth is clearly proved by the formation of the crystals in concentric zones and by their size. These large crystals, existing ready formed in the lava at the time of its eruption, are surrounded by microlites or by a vitreous mass. It was after their slow development in the magma, during an intra-telluric period, that the mass in which they floated was upraised. The period of calm was succeeded by one of agitation, and the lava in its violent ejection carried forth the crystals, breaking them, corroding them, and partially fusing them. The microscope offers distinct evidence of these phenomena. We see the large crystals dislocated and their fragments dispersed, their edges rounded and eroded, and their substance invaded and penetrated by the paste.

While the physical and chemical agencies brought into play by the movement of the lava thus attack the ancient crystals to the verge of demolition, the microlites are in course of formation. This vitreous matter, in which the large crystals float, solidifies as a mass of microscopic individuals. The latter are therefore related to a second phase of crystallization: they are developed in a moving viscous magma, and their further growth is arrested by the rapid cooling which induces solidification *en masse*.

The fluidal arrangement of the microlites distinctly shows, too, that the crystalline action was contemporaneous with the movement of the lava-flow. Indeed, we see in microscopic preparations that the microlites are accumulated around the large sections of crystals, forming wavy trains and presenting the arrangement which micrographers designate as *fluidal structure*. It is marked by the orientation of these infinitely small acicular crystals. When these streams of microlites meet the large embedded crystals, they sweep round them, crowding into the spaces between the large sections, accommodating their flow to these outlines, and preserving for us the last movement of the mass at the very moment of solidification.

The microscope therefore proves that crystallization in lavas belongs to two periods: the first, anterior to the eruption, during which the large crystals already found are suspended in a mass that we may regard as entirely vitreous; and the second period, when the microlites and embryonic crystalline forms are separated, dating from the ejection or outflow, and contemporaneous with the solidification of the rock.

From these microscopic observations on the crystals of the second period, we may conclude that they are formed purely and simply by igneous action, without requiring the hypothetical temperatures and pressures formerly considered necessary, and without that absolute repose regarded as needful for the regular crystallization of minerals. We see, indeed, that the microlites are formed after the outflow, at the normal barometric pressure and at a temperature far from being as high as generally supposed, and we witness the birth of the crystals during the very flow of the lava stream. When the cooling is extremely rapid, the microlites have no time to form, and the lava can produce only crystallites.

But the microscope enables us to determine the chronology of the crystals in lava in a still more detailed manner. We have already distinguished two great periods in their history; let us now indicate in a general way how we may establish to some extent the date at which each species of the two groups is separated from the magma. Data leading to the determination of their relative age are afforded by their inclusions.

A crystal developed in a vitreous mass frequently incloses particles of the medium in which it grows. In this way certain sections under the microscope appear penetrated with vitreous grains, imprisoned in the interior of the crystals and frequently arranged along the zones of successive growth. These inclusions prove that the crystals in question were formed in a vitreous mass, liquefied by heat. In other cases the inclusions are mineral species in the form of microlites; and it is clear that they must have been anterior in date to the mineral in which they are inclosed. Finally, in other cases a species will mould itself around sharply defined crystals, conforming to their outlines, and filling up all the spaces between the minerals; thus showing that the crystals are of earlier origin than the surrounding mineral.

From these facts, which speak for themselves, we have been able to draw up chronological lists indicating the relative date of crystallization of each species of the two great periods. I will not stop to cite these lists, but we shall soon see how the law which governs the successive formation of the crystals, and their relative age, is evolved from synthetic experiments.

I have traced in broad outline the history of a lava, but have sketched only a few of the details which modern researches on lithological phenomena have developed with such startling reality: nevertheless, what we have seen is sufficient to show in a striking manner the power of analysis when supported by reasoning. I think I am not wrong in saying that from this point of view the study of a lava presents one of the finest examples of the application of the inductive method to the natural sciences. We hardly know whether to admire most the analytical processes, or the subtlety of observation, or the logical method by which the observed phenomena have been brought into connection.

Microscopic analysis, powerful as a method of investigation, has enabled us to trace with close exactitude the progress of crystallization in a rock where the unaided eye could discover only an indistinct and uniform mass; to penetrate into this marvellous tissue of volcanic products, where millions of polyhedra occur within the volume of a cubic centimetre; to determine with mathematical precision the nature of each of these infinitely small bodies; to track them to their birth, and follow them throughout their development, tracing all the modifications to which they have been subjected under the influence of physical and chemical agents.

Nevertheless, to the conscientious and modest investigator, how much still remains unknown in connection with the history of volcanic products, though the field seems so narrow, and has already been so well worked! What problems remain unsolved, even by the most refined observation! But when observation can no longer aid us, when we have exhausted all the resources of this method of investigation, there yet remains the method of synthetic experiment. This forms one step more on the road which leads to a perfect knowledge of the phenomena, and may conduct us to their definite solution. But, in order that synthetic operations may attain this end, they must be directed with due intelligence and design.

One of the essential conditions of a geological synthesis, as Sénarmont remarked, is, that each of the artificial operations should be compatible with all the circumstances traceable in the products of the natural operation. The slags and scoriæ of our furnaces, which, as we have shown, are related to certain natural products, are, it is true, the results of synthesis, but synthesis made at haphazard; and thus, notwithstanding their high scientific interest, cannot be placed on the same level with the synthesis of which I am about to speak, where the experimentalist, bearing steadily in view the problem which he desires to solve, attempts to realize in the laboratory the identical conditions which have surrounded the formation of the natural products which he wishes to imitate.

In logical order, the synthetic methods follow the progress of observation and of analysis. But even in the very infancy of geology there were certain powerful minds which foresaw, with the glance of genius, the part which experiment was destined to play in that science. Buffon proved by experiment that granite and the principal crystalline rocks are fusible, and that they were transformed by fusion into a vitreous mass. Some years later, Spallanzani performed an extensive series of experiments on the fusion of lavas, in order to overcome the prejudices which prevailed respecting the cause of the heat of eruptive matter.

But it is especially to Sir James Hall that belongs the honour of having, by his celebrated researches, introduced experiment into geology. He demonstrated its application in a masterly manner, and was led to sound generalizations. We have here to notice in Hall's researches only those which relate to the synthesis of rocks. About the time when Spallanzani studied, by laboratory methods, the conditions of the formation of lavas, the illustrious Scottish geologist was busy fusing the eruptive rocks in a vessel of graphite: he observed that the product of this fusion, if cooled rapidly, became an amorphous vitreous mass, while, if cooled more slowly, crystals were formed. James Hall had already observed by experiment the capital fact for future synthesis that, in order to regenerate the crystals of a rock which has been fused, it is necessary to maintain the glass obtained by the fusion at an elevated temperature, but yet a temperature always inferior to that required for the fusion of the rock. During this process, certain minerals crystallize. These facts may be paralleled with the phenomena which lavas display when their temperature is lowered after their emission.

Towards the commencement of this century, Gregory Watt directed his researches in the same direction. He experimented on masses of basalt, 700 pounds in weight: these he fused, and allowed to cool during eight days beneath a layer of charcoal, which was slowly consumed. During this prolonged *recuit*, spherulitic concretions of fibro-radiated texture, 6 centimetres in diameter, separated in the opaque black glass resulting from the fusion: finally, the glass passed into a stony condition, assumed a granular structure, and became charged with very thin crystalline lamellæ. At the same time, its magnetism was increased, while its density rose from 2·743 to 2·949.

One conclusion from the researches of Watt, which are closely related to those of Hall, is, that crystallization may occur during the period when the fused matter commences to solidify.

At the time when the road to the synthesis of rocks

was being thus opened up, analysis and the methods of investigation had not attained to the perfection which they enjoy at the present day; on the other hand, the prejudices which held sway in the infancy of geology increased the obstacles, and these were not surmounted until half a century later. We need not be detained here by the brilliant period of mineral synthesis which followed close on the development of chemistry and mineralogy. It is sufficient to cite the names of Ebelmen, Rose, Mitscherlich, and Sénarmont, to recall those remarkable results in the artifical reproduction of minerals. But the researches of these *savants* related chiefly to the synthesis of isolated species, and not to rocks which are aggregates of mineral species. Speaking generally, it may be said that their researches were essentially mineralogical, and bore but subordinately on lithology. Nevertheless, the researches of these skilful experimentalists shed much light upon geological problems. They also show us that, as the mineral sciences progress, we are led to seek, by experimental methods, the most complete interpretation of the phenomena of Nature. Finally, in 1866, Daubrée led the way to the reproduction of crystalline rocks by simple fusion. This is the method which has been since taken up and developed by MM. Fouqué and Michel Lévy. The researches of Daubrée, to which we refer, are those in which he sought to reproduce by fusion certain meteoric stones characterized by the absence of a felspathic element. By fusing lherzolite, a terrestrial rock which approximates in composition to certain meteorites, he succeeded in obtaining products which, in the details of their structure and composition, resembled the cosmical types which he desired to imitate.

While this eminent geologist thus foreshadowed the researches which some years afterwards shed so brilliant a lustre upon the geological laboratory of the Collège de France, the synthetic methods were still encumbered by hypotheses. It is true we had no longer to struggle against the assumed influence of mysterious forces; but it was held that the reproduction of geological phenomena in the laboratory would be possible only if we had an infinite duration of time at our disposal, and dealt with temperatures and masses far beyond those which we could hope to command in the laboratory. It was still supposed that the mineral associations in Nature were governed by other laws than those which determined the combinations produced by the chemist. Such prejudices would certainly not have hindered Daubrée from proceeding in the path in which he so bravely took the first step by his synthesis of meteorites; for he, indeed, is one of those whose works have largely contributed to banish such prejudices from the realms of geology; but the methods of analysis which then existed did not allow us to probe the nature of the rocks to their very base, and to compare their intimate structure with that of the products of synthesis. Our laboratories were not then in possession of the apparatus by means of which we can command those very high temperatures, maintained during a prolonged period, which such experiments require.

The great improvements in the construction of apparatus, and the application of the microscope to lithology, have at length enabled us to successfully attempt the reproduction of all the modern volcanic rocks. Two French *savants*, MM. Fouqué and Michel Lévy, who introduced into their country the study of micrographic lithology, began in 1877 a series of synthetic experiments destined to be memorable in the annals of science. One of them had already acquired a just reputation by his remarkable researches on volcanic phenomena, carried on in various classical regions; he was familar with all the secrets of the chemical analysis of minerals—a department which he had enriched by the most ingenious and useful methods. The other, prepared by the severe studies of the high French schools, had undertaken, with brilliant success, the examination of minerals by their optical properties: he had carried exact methods into micrography, far beyond what others had done, and he was known by his researches on the eruptive rocks of the older series.

By their joint labours, MM. Fouqué and Lévy have to some extent systematized and co-ordinated the facts relative to the chronological succession of the crystals in eruptive rocks, and have discovered many of the details which we have already noticed in explaining the results of the analyses of lavas. It is to this happy association of talent, to this fruitful collaboration, that we owe those beautiful discoveries which have given such celebrity to the laboratory of the Collège de France, and to which it is an honour for me to render homage before an audience ever ready to welcome scientific progress, and in a place where the immortal Faraday once brought forward, with generous enthusiasm, the admirable researches of Ebelmen in connection with mineral synthesis.

We have already indicated the data upon which these *savants* relied in their researches: they are furnished by chemical and mineralogical analysis. One point, however, not yet touched upon, lies at the base of their general procedure. Theory would predict that the most ancient crystals in an igneous rock should be those which are the least fusible. And this, speaking in general terms, is really what we observe: the minerals of the first period of crystallization are those which occupy the lowest degrees in the scale of fusibility. The constituent mineral species of lavas have appeared at successive periods, as the temperature diminished, according to their relative degrees of fusibility. These facts, proved in detail by microscopic analysis, served as the point of departure in the experiments of MM. Fouqué and Lévy. Their process rests, morever, upon a fact which James Hall foresaw: namely, that the fusion of a rock produces a glass which is more easily fusible than any of the constituent crystalline species of the rock. Now, if we fuse a natural aggregate of minerals and subject the glass produced by this fusion to a series of diminishing temperatures, but always higher than that of the fusing-point of the vitreous mass, the minerals, which can crystallize from this magma should make their appearance one after another, and the less fusible should be the first to separate. These crystals will be united and moulded round by those of which the fusibility is higher, and which will appear in turn as the temperature decreases. Without dwelling on the technical details of the apparatus, it suffices to say that, by aid of the furnaces and bellows, which MM. Fouqué and Lévy employ in their syntheses, we can obtain all degrees of temperature, from a dull red to a dazzling white heat, and can maintain a given temperature constant for an unlimited period.

Into the furnace we introduce a platinum crucible of a capacity of about 20 cubic centimetres, containing the mixture of mineral substances which by fusion and *recuit* are to be transformed into the rock. First, by aid of special arrangements, we subject it for a long time to a glowing white heat, and the mixture is converted into a glass. By regulating the admission of gas and air, and by uncovering the furnace, the temperature is lowered to an orange heat—the fusing-point of steel. By raising the crucible in the furnace, the temperature falls to a cherry-red heat—the melting-point of copper. Finally, if the crucible be completely removed from the furnace, it can still be maintained at a temperature at which copper would fuse with difficulty.

We have thus indicated the broad lines of the operation. These are the successive *recuits* at diminishing temperatures which cause the crystals to be formed in sequence, commencing with the least fusible, and which enable us to impart to the fused matter the texture and the mineral composition of volcanic products.

We proceed to illustrate by examples the method of

lithological synthesis. Let us first explain the manipulations for the reproduction of one of the rocks which plays the principal part in the eruptions of Vesuvius—*leucotephrite*. This rock is composed of leucite, labrador-felspar, and augite.

A mixture is formed of silica, alumina, lime, ferric oxide, potash, and soda, corresponding to one part of augite, four of labrador-felspar, and eight of leucite. This mixture is introduced into the crucible, and fused at a glowing white heat to a homogeneous glass. After fusion, the temperature is lowered, and the vitreous mass is exposed for forty-eight hours to the temperature of fused steel. During this first phase, crystals of leucite separate. They evidently correspond to the first period of consolidation in eruptive rocks.

The matter is then subjected during another forty-eight hours to the temperature of fused copper. All the mass, the residue from which the crystals of leucite first separated, is now transformed into microliths of augite and labrador-felspar, with octahedra of magnetite and picotite.

Let us now compare microscopic preparations of the synthetical product of this double *recuit* with those of the natural lava. Not only have the same minerals been reproduced by this dry fusion, but the order of their appearance and the proportion of the constituent species are identical; and their analogy may be pursued even to the details of the crystallographic forms. The leucite, in large crystals, offers all the features of this mineral in the Vesuvian lavas; and around these crystals are grouped the microlites of the second period—the augite and labrador. Finally, as in the natural rock, the leucite contains inclusions of magnetic iron-ore and picotite, which are the most ancient minerals in the rock.

As a second example, let us take the synthesis of *basalt*—one of the most widely-spread types of rock in the volcanic series, and one which, so far as its origin is concerned, has been the subject of numerous hypotheses. It is known that basalt is composed essentially of three minerals—olivine, augite, and labrador-felspar. The olivine in the natural rock appears in crystals of the first consolidation.

As in the case of the leucotephrite, so here, we form a mixture of the chemical constituents, or of the powdered minerals, corresponding to the mean composition of a basalt rich in olivine. Such a mixture is composed of three parts of olivine, two of augite, and three of labrador. This is first transformed into a homogeneous black glass. During forty-eight hours it is maintained at a white heat. If, after this *recuit* at a high temperature, we examine a thin section of the glass, we observe large crystals of olivine. These are as yet embedded in a vitreous mass, in which small octahedra of massicotite and picotite are isolated, as also a few crystals of augite.

It remains now to produce the microlites of the second consolidation, by which the crystals of olivine developed during the first phase ought to be surrounded. To produce these, the mass is maintained at a cherry-red heat for forty-eight hours. After this *recuit* we obtain a paste composed of microlites of labrador and augite, with magnetite and a vitreous substance which is the residue of the crystallization. We have therefore, in this second phase, reproduced the microlithic structure. These manipulations produce basalts which we can scarcely distinguish from the natural rocks, and thus a few grammes of a substance skilfully manipulated furnish us with the most convincing proof of the purely igneous formation of this rock.

We could go on explainng these remarkable series of experiments by MM. Fouqué and Lévy, in the same way as we have dealt with the two preceding syntheses. All the contemporary eruptive rocks have thus been reproduced: andesites, labradorites, basalts, limburgites, nephelinites, tephrites, leucite rocks, peridotites, and labradorites with ophitic structure. We will, however, confine ourselves, as a last example, to those processes by which they have succeeded in directly explaining, by means of synthesis, the eruptive phenomena of the older periods of the globe.

There are certain ancient crystalline rocks, common in the Pyrenees, which are known as *ophites*. The period at which they were formed, and their mode of origin, had not been definitively established, when in 1877 M. Lévy showed that they were eruptive and that they exhibited under the microscope a remarkable structure which he termed the "ophitic structure," the felspar being surrounded by very large plates of augite. It seemed, then, that the ophitic rocks were igneous rocks, in which the cooling had been more slow than in the ordinary rocks of modern eruptions. It was therefore necessary, in attempting to reproduce the ophitic type by synthesis, to cause the augite to crystallize during a phase sharply separated from that in which the felspar was reproduced; and, moreover, to give to the augite sufficient time to crystallize in large plates. For this purpose, a mixture of one part of anorthite and two of augite was submitted, after fusion, to a first *recuit*, in which it was maintained for forty-eight hours at the melting-point of steel: under these conditions the anorthite separated. A second *recuit* of the same duration as the first, but at the fusing-point of copper, led to the crystallization of the augite in large plates, which were moulded round the felspathic element, and to which were added small octahedra of magnetite and picotite. By this remarkable synthesis the eruptive origin of the ophites, and the cause of their structure, were established beyond all doubt.

It is thus seen how synthesis succeeds in explaining the genesis of rocks, and in settling those discussions which until recently were rife with respect to the principal crystalline types of modern date; those relating, for example, to basalt—a rock in whose formation it was argued that water played an important part. Now, the broad conclusion to be drawn from these experiments is that basalt, and, indeed, modern volcanic rocks in general, have been formed by a fusion purely igneous.

But by the side of these magnificent results the *savants* have had to record many fruitless experiments. It is useful to recall these by way of example, as they serve to indicate the paths to be avoided if we would attain success. These failures circumscribe the field of future experiment, and mark the limits within which hypotheses should have play. They demonstrate, moreover, that the rocks which we have not succeeded in forming synthetically by our methods must have been formed under different conditions from those which prevail in the formation of modern volcanic products. This conclusion, to which observation and analysis had already pointed, without, however, precisely defining the cause, is thus confirmed by the failure of our synthetic researches. If synthesis has succeeded in reproducing all kinds of lava from modern eruptions, it has failed to imitate those rocks which are no longer formed in contemporary eruptions. It may be said, generally, that up to the present time all the acid rocks have withstood our synthetic efforts, and those which contain among their constituent minerals, quartz, mica, orthoclase, and hornblende.

The processes of Nature involve no occult forces, and it may be that by combining those means which are already at our disposal, and in modifying their application, we may be permitted to witness the production of those rocks which have hitherto eluded our efforts. Such a hope is based on the results already attained, which we may regard as only the presage of others perhaps still more surprising. The failures of the past prepare for the conquests of the morrow.

In this rapid review of the progress of lithological synthesis, I have endeavoured to show the high scientific value of the researches instituted in the geological labora-

tory of the Collège de France. I could also have explained the successful syntheses, not less remarkable, of minerals and meteorites made by these experimentalists or by their pupils, among whom M. Bourgeois occupies a special position. But I must limit myself; and, indeed, what I have said is sufficient to show how their methods have advanced our knowledge in a domain to which access had previously appeared shut against investigation.

Wherever the experimental method has hitherto carried its torch, it has brilliantly illuminated the most striking phenomena in the science of the earth. It suffices to mention the name of Daubrée, the direct descendant of the illustrious geologists of the Scottish school, to indicate the extent of the field of the mineral sciences already explored by the method of experiment. It has been successfully applied to the interpretation of metalliferous deposits and of metamorphic rocks, and to the study of the fractures and deformation of the earth's crust, of the schistosity of rocks, and of certain features in mountain structure.

Geology, after having passed through the successive phases of observation and analysis, has therefore entered upon that of experiment and synthesis, in which it strives to imitate the creative power of Nature, thus crowning the scientific edifice by processes which allow us to catch a glimpse of the operation of causes the knowledge of which is the final aim of physical and natural science. It was this crowning of the work which Leibnitz foresaw when he wrote, two centuries ago:—"He will perform, in our opinion, an important work, who shall carefully compare the products extracted from the depths of the earth with those of the laboratory; for then will be brought vividly before our eyes the striking resemblance which subsists between the productions of Nature and those of Art. Although the Creator, inexhaustible in resource, has at command divers means of effecting His will, it nevertheless pleases Him to maintain a constancy in the midst of the variety of His works; and it is already a great step towards a knowledge of things to have discovered even one means of producing them; for Nature is only Art on a large scale."

SOME RECENT ADVANCES IN THE THEORY OF CRYSTAL-STRUCTURE.

THE growth of modern theories concerning the structure of crystals is perhaps not so closely followed by English chemists as might be expected from the inherent interest of the subject, in spite of the attention which is now devoted to all questions of atomic and molecular arrangement in space.

It is in the morphology of crystals that the geometrical arrangement of the atoms or molecules (in the solid) finds, if anywhere, a geometrical expression, and yet little or no account is taken of this subject in text-books of chemistry or physics, so that it is difficult for the student to discover what views are held by modern authors. Moreover, crystallographic observations and theories are generally published in journals specially devoted to mineralogy which are not easily accessible to all who are interested in such questions.

It seems, therefore, advisable to draw attention to the progress which has recently been made in the theory of crystal-structure, and more especially to papers by Prof. Sohncke, of Munich, published in Groth's *Zeitschrift für Krystallographie und Mineralogie*, a journal which is a complete storehouse of information relating to the study of crystals.

Sohncke's theory, which was published in 1879,[1] has now emerged from the purifying fire of recent criticism in an emended form in which perhaps it will more readily excite the interest of chemists.

In order to make it clear in what respects the theory of Sohncke in its latest form differs from those which have been previously advanced it will be necessary to give a brief sketch of the theory of Bravais, of which Sohncke's system is an extension.

The Abbé Haüy,[1] having found that all crystals of the same substance may be reduced by cleavage to the same solid figure, whatever their external form, argued that the cleavage solid has the form of the ultimate particles into which any crystal may in imagination be separated by repeated subdivision, and that this is therefore the form of the structural unit: it is not, of course, necessary or even probable that the latter should be identical with the chemical molecule. Hence a crystal is to be regarded as constructed of polyhedral particles, having the form of the cleavage fragment, placed beside one another in parallel positions. A crystal of salt, for example, which naturally cleaves parallel to the faces of the cube, is constructed of cubic particles.

Upon the relative dimensions of the structural unit depends the form assumed by the crystals of a given substance.

It will be found that this theory not only accounts for the existence of cleavage, but further defines the faces which may occur upon crystals of a substance having a given cleavage figure; for, if once it is assumed that a crystal-face is formed by a series of the particles whose centres lie in a plane, it follows that all such planes obey the well-known law which governs the relative positions of crystal-faces.

A natural advance was made from the theory of Haüy, without detracting from its generality, by supposing each polyhedral particle in Haüy's system to be condensed into a point at its centre of mass, so that the positions of the molecules, and therefore of the crystalline planes, remain the same as before; but the space occupied by a crystal is now filled, not by a continuous structure resembling brickwork, but by a system of separate points.

It will be found that in such a system of points, if the straight line joining any pair be produced indefinitely in both directions, it will carry particles of the system at equal intervals along its entire length; in other words, all the structural molecules of a crystal must lie at equal distances from each other along straight lines. The interval between particles along one straight line will in general be different from those along another, but the molecular intervals along parallel straight lines will always be the same.

Bravais,[2] therefore, following in the steps of Delafosse and Frankenheim, treated the subject as a geometrical problem, and inquired what are the possible ways in which a system of points may be arranged in space so as to lie at equal distances along straight lines—in other words, so as to constitute what may be called a *solid network* (*assemblage*, *Raumgitter*).

The geometrical nature of a network may be best realized as follows. Take any pair ($O\ C_1$) of points in space, draw a straight line through them, and place points at equal distances along its entire length ($C_2, C_3, \ldots$); such a line may be called a *thread* of points (*rangée*). Parallel to this line, and at any distance from it, place a second thread of points ($A_1\ a_1$), identical with the first in all respects; in the plane containing these two threads place a series of similar equidistant parallel threads ($A_2\ a_2$, &c.) in such positions that the points in successive threads lie at equal intervals upon straight lines whose direction ($O\ A_1$) is determined by the points upon the first two threads. Such a system of points lying in one plane may be called a *web* (*réseau*). Now, parallel to this plane, and at any distance from it, place a second web ($B_1\ b_1$), identical with the first.

[1] "Entwickelung einer Theorie der Krystallstruktur." (Leipzig.)

[1] "Traité de Cristallographie." (Paris, 1822.)

[2] "Études cristallographiques." (Paris, 1866.)

Finally, parallel with these, place a series of similar equidistant webs in such positions that the points in successive planes lie at equal intervals upon straight lines whose direction ($O\,B_1$) is determined by the points in the first two webs.

In this way a *network* of points is constructed, in which the line joining any two points is a *thread*, and the plane through any three points is a *web*.

The space inclosed by six adjacent planes of the system having no other points of the network between them is a parallelopiped ($O\,A_1\,B_1\,C_1$), from which the whole system may be constructed by repetition, and which may be taken to represent the structural element (*molécule soustractive*) of Haüy.

The complete investigation of all possible solid networks led Bravais to the conclusion that these, if classified by the character of their symmetry, fall into seven groups, which correspond exactly to the seven systems into which crystals are grouped in accordance with their symmetry.

It follows, then, that two (not, however, independent) features of crystals are fully accounted for by a parallelopipedal arrangement of points in space—namely, the symmetry of the crystallographic systems and the law which governs the inclinations of the faces (law of rational indices).

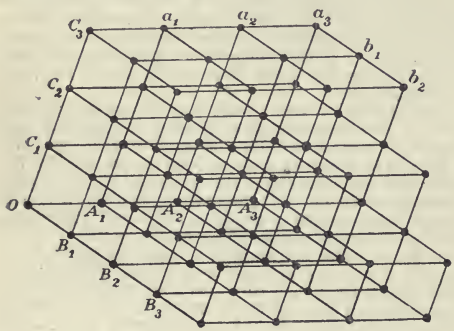


FIG. 1.

There are, however, subdivisions of the various systems consisting of the merohedral or partially symmetrical crystals belonging to them, which are not explained by the geometry of a network; these consequently were referred by Bravais, not merely to the arrangement of the molecules in space, but also to the internal symmetry of the molecule itself.

Hence the theory of Bravais, while able to a certain extent to explain the form of crystals, requires an auxiliary hypothesis if it is to explain those modifications which are partially symmetrical or merohedral.

Sohncke, treating the problem in a different manner, and reasoning from the fact that the properties of a crystal are the same at any one point within its mass as at any other but different along different directions, inquired in how many ways a system of points may be arranged in space so that the configuration of the system round any one point is precisely similar to that round any other. Such a configuration may be called a *Sohncke system* of points in space (*regelmässiges Punktsystem*).

From his analysis of this problem, it appears that there are sixty-five possible Sohncke systems of points, and that these may be grouped according to their symmetry into seven classes corresponding to the seven crystallographic systems; and further that there are within each class minor subdivisions, characterized by a partial symmetry corresponding to the hemihedral and tetartohedral forms of crystallographers.

It may be expected, then, that the theory of Sohncke contains within itself the essential features of a Bravais network of structural molecules, and also the auxiliary hypothesis regarding the arrangement of parts within the molecule which is required to account for merohedrism.

Now, on closer examination the arrangement of Sohncke does prove to be a simple extension of that of Bravais.

Each of Sohncke's arrangements may in fact be regarded as derived from one of the parallelopipedal networks of Bravais if for every point of the latter be substituted a group of symmetrically arranged satellites. It is not necessary that any particle in a group of these satellites should actually coincide with the point of the Bravais network from which the group is derived; and the points of the Sohncke system do not themselves form a network; it is only when all the points in each group of satellites are condensed into one centre that a Sohncke system coincides with a Bravais network.

To any particle of one of the satellite groups corresponds in every other group a particle similarly situated with regard to the point from which the group has been derived. Every such point may be said to be homologous with the first.

It will then be found that each complete set of homologous points is itself a Bravais network in space, and that consequently any Sohncke system may be regarded as a certain number of congruent networks interpenetrating

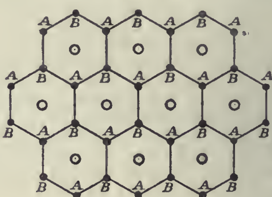


FIG. 2.

one another: the number of such networks is in general equal to the number of points which constitute each group of satellites.

The relation of a Sohncke system to the network from which it is derived may be illustrated by a bees'-cell distribution of points in one plane, *i.e.* by points which occupy the angles of a series of regular hexagons. Thus in the adjoining figure the dots form a Sohncke system in one plane, since the configuration of the system round any one point is similar to that round any other; but they do not form a Bravais web, since the points do not lie at equal distances along straight lines.

If, however, points, represented in the figure by the circles O, be placed at the centres of the hexagons, they will by themselves constitute a web, and the hexagonal system may be derived from this web by replacing each of its points by a group of two satellites, A and B. Or, from the second point of view, the arrangement may be regarded as a triangular web, A, completely interpenetrated by a similar web, B.

It is a remarkable feature of the Sohncke systems that some among them are characterized by a spiral disposition of the particles along the threads of a right- or left-handed screw: now this spiral character, which does not belong to any of the Bravais networks, supplies a geometrical basis for the right- or left-handed nature of some merohedral crystals which possess the property of right- or left-handed rotatory polarization.

The theory of Sohncke as sketched above appeared to

be expressed in the most general form possible, and to include all conceivable varieties of crystalline symmetry.

It has, however, recently been pointed out by Wulff[1] that the partial symmetry of certain crystals belonging to the rhombohedral system—that, namely, of the minerals phenacite and dioptase—is not represented among the sixty-five arrangements of Sohncke.

Other systems of points in space have also been studied by Haag[2] and Wulff, which do not exactly possess the properties of a Sohncke system, and yet might reasonably be adopted as the basis of crystalline structure, since they lead to known crystalline forms.[3] These, however, and all other systems of points which have been proposed to account for the geometrical and physical properties of crystals, may be included in the theory of Sohncke after this has received the simple extension which is now added by its author.

In Bravais's network all the particles or structural elements were supposed to be identical, and in Sohncke's theory also there is nothing in their geometrical character to distinguish one particle from another.

In Fig. 2, the hexagonal series of dots may, as was said above, be regarded as composed of a pair of triangular webs, A and B; now these, although identical in other respects, are not parallel, for the distribution of the system round any point of A is not the same as that round any point of B until it has been rotated through an angle of 60°.

It is possible, however, to conceive similar interpenetrating networks which differ not only in their orientation but even in the character of their particles. The centre of each hexagon, for example, may be occupied by a particle of different nature from A and B to form a new web, O. The three webs are precisely similar in one respect, since their meshes are equal equilateral triangles; moreover, if the *position* of the points alone be taken into account, the whole system would form a Bravais web, *i.e.* if the particles of O were identical with those of A and B. If, however, as is here supposed, the set O consists of particles different in character from A and B, the distribution round any point of O is totally distinct from that round any point of A or B. The points O are geometrically different from the points A B. The web A is interchangeable with B, but O is interchangeable with neither.

Now, it is precisely an extension of this kind which must be given to Sohncke's earlier theory if it is to embrace all the crystalline arrangements which have been alluded to above. The interpenetrating networks are no longer to be regarded as consisting necessarily of identical particles; the structural units of a crystal may be of more than one kind.

The above figure represents a Sohncke system, A B, of particles of one sort interpenetrated by a Bravais web, O, of another sort; but there is no reason why two or more different Sohncke systems, no one of which is identical with a Bravais network, may not interpenetrate to form a crystal structure.

In its most general form, then, the theory may now be expressed—

A crystal consists of a finite number of interpenetrating Sohncke systems which are derived from the same Bravais network. The constituent Sohncke systems are in general not interchangeable, and the structural elements of one are not necessarily the same as those of another.

Or, since each Sohncke system consists itself of a set of interpenetrating networks, the theory may be thus expressed—

A crystal consists of a finite number of parallel interpenetrating congruent networks: the particles of any one network are parallel and interchangeable; these networks group themselves into a number of Sohncke systems in each of which the particles are interchangeable but not necessarily parallel.

The number of kinds of particles which constitute the crystal may therefore be equal to the number of Sohncke systems involved in its construction.

The structural units are no longer, as they were in the theory of Bravais, necessarily identical, but may represent atomic groups of different nature.

The system in Fig. 2 consists of two sets of particles, A B and O; and, if a large enough number of these be taken, any portion of the system (*i.e.* any crystal constructed in this manner) consists of the particles united in the proportion of two of the first group to one of the second. Such an arrangement, then, may represent the structure of a compound, O A_2.

"When, for example, a salt in crystallizing takes up so-called water of crystallization which is only retained so long as the crystalline state endures, the chemical molecule salt + water cannot be said to exist except in the imagination, for the presence of such a molecule cannot be proved. To obtain an easily intelligible example, without, however, pronouncing any opinion as to whether it may be realized, imagine the centred hexagons in the figure to be constructed in such a way that each corner consists of the triple molecule $3H_2O$, and each centre consists of the molecule R. The chemical formula would then be $R + 6H_2O$, and yet a molecule of this constitution would not really exist; on the contrary, the structural elements in the crystallized salt would be of two sorts—namely, R and $3H_2O$."[1]

Hence it is geometrically possible that the structural elements of a crystal may be different atomic groups which are held in a position of stable equilibrium by virtue of being interpenetrating networks.

Whether such systems are chemically and physically possible must be left for future criticism to decide.

Finally, we may call attention to a remarkable declaration of faith which has recently been made in Germany by one who is a recognized leader in crystallographic and mineralogical science.

Prof. Groth[2] has suggested that there may be something more than a chance similarity between the theory of Sohncke and the views of the eminent French crystallographer Mallard, whose classical research upon the optical anomalies of crystals has been the means of dividing the students of this subject into two adverse camps. The explanation of Mallard has up to the present time found little favour among those German mineralogists who have made similar investigations. Prof. Groth has now, however, declared himself in favour of Mallard, being apparently induced to do so by the support which is given to his views by the theory of Sohncke.

Mallard has ascribed the optical anomalies of various substances to a complete or partial intergrowth of two or more crystals which combine in such a manner as to simulate a symmetry of higher order than that which naturally belongs to them. Now, since Mallard regards each crystal as composed of a Bravais network, it is evident that his views are not far removed from those of Sohncke, whose system is based upon the possible intergrowth of two or more networks. H. A. MIERS.

[1] *Zeitschr. f. Kryst.* xiii. (1887) p. 503.
[2] "Die regulären Krystallkörper." (Rottweil, 1887.)
[3] Cf. W. Barlow, NATURE, xxix. (1884) pp. 186, 205.

[1] Sohncke, *Zeitsch. f. Kryst.* xiv. p. 443.
[2] "Ueber die Molekularbeschaffenheit der Krystalle." (Festrede, München, 1888.)

THE EARTHQUAKE AT BAN-DAI-SAN, JAPAN.

AS it may interest our readers to know the present state of matters at the scene of the great earthquake which occurred lately at Ban-dai-san, Japan, we think it well to publish the following narrative just received by Dr. George Harley, F.R.S., in a private letter from his son, who has recently visited the locality of the sad disaster.

The letter is dated December 2, 1888, from on board the Peninsular and Oriental s.s. *Verona*, while in the Inland Sea on its voyage back from Japan to China.

Mr. Vaughan Harley says that on October 20 last, having procured the services of an interpreter, he started by train from Yokohama to Tokio, where he obtained a permit from the Japanese Foreign Office to visit the Ban-dai-san valley. From Tokio he went by train to Kuragano, where he engaged, for himself and interpreter, a couple of *jinrickshas*, with two coolies for each. On the following morning he started at 4.45 a.m.,—that is to say, before daylight. It being then early winter in Japan, the day did not break till 6.45. The weather at the time was both cold and rainy; but so long as the roads were good, the coolies, running tandem-fashion, managed to get along at an average rate of from 6 to 7 miles an hour, and accomplished 50 miles a day. On arriving at Inawashiro Lake, after having engaged a guide, he proceeded direct to Ban-dai-san, where the scene that met his eyes, though magnificent, was truly awe-inspiring. It was a veritable valley of devastation. For the whole side of a mountain—3 miles in circumference—had been completely blown away, and hurled as if it had been the mere outside wall of a house, into the valley below, completely burying beneath it four villages and their surrounding farms, along with all their inhabitants. Such was the stupendous force of the explosion, that the mere wind-shock produced by its concussion knocked down, as if they had been nothing more than ninepins, the whole of the trees growing on the opposite mountain-side. The river in the valley, too, was so dammed across by the huge mass of detached mountain as to have formed itself into a small lake, the waters of which now occupy the place where formerly well cultivated crops grew.

The catastrophe which brought about these physical changes appears to have been due to the sudden explosion of superheated pent-up steam, either alone or in conjunction with volatile gases, set free by the decomposing chemical action of heat and water on the constituents of the subjacent mineral strata. The whole surrounding ground is at present full of hot springs, giving forth volumes of steam, while from every crack and crevice in the earth issues, either continuously or spasmodically, clouds of hot watery vapour, so that one has to be very careful where he places his feet. Not only the fact of the presence of these hot springs, but likewise of the still frequent occurrence of earthquakes, shows that the same agent or agents that rent the mountain in twain are still actively at work. Even in the morning of the day following his visit (at 5 a.m.) there was a shock of earthquake, which, although it was strong enough to admit of his feeling the earth quiver beneath him, the people spoke of as being such a mild one as to merit no attention. He says, moreover, that the appearance presented by the standing half of the cleft mountain, with its surrounding clouds of steam, was, to his way of thinking, far grander, and vastly more awe-inspiring, than are either the geysers of Iceland or the yet greater and more numerous ones he had seen in the volcanic district of the Yellowstone Park in North America. For here the scene he witnessed not only plainly pointed to the cause, but gave him ocular demonstration of its stupendous power, and made him feel that, if superheated steam could thus easily, apparently, rend asunder a solid mountain of rock, there could be no difficulty in understanding why the live volcanoes scattered over the globe were looked upon as safety-valves for the effects of the various chemical decompositions brought about by heat and water in the molten minerals within the bowels of the earth. For were there no outlets even to the superheated steam—heated by the vast internal fires up to a point when it possibly resolves itself into its elements—he could readily enough imagine, from what he saw, that its sudden explosion might suffice to shatter the earth's crust into fragments—just, perhaps, as takes place in some of the heavenly bodies, fragments from which ever and anon fall, in the shape of meteorites, upon the surface of our globe. Having got back to the tea-house at 7.15 p.m., leg-tired and foot-sore, but thoroughly satisfied with all he had seen and learned, immediately after a hot bath—a natural one, for there is no need of artificially heating bath-water here, Nature does that amply for them—and supper, he went to bed, the bedstead being the floor, as is usual in Japan. Next morning, he started on his return journey to Yokohama, and arrived in good time for the sailing of the *Verona* on the 25th for Hong Kong, where he immediately posted his letter, in order to catch the homeward mail.

NOTES.

THE Vice-Chancellor of Cambridge University has appointed Prof. Stokes, P.R.S., Rede Lecturer for the present year.

MR. ISAAC ROBERTS, the eminent photographic astronomer, has presented to Dunsink Observatory a photographic reflecting telescope with a mirror by With of 15 inches aperture. The generous donor is erecting the instrument at his own expense, and it will be employed in furthering the study of star parallax—a study with which Dunsink has been so long associated.

ON Thursday evening last, the first meeting of the Institution of Electrical Engineers was held in the rooms of the Institution of Civil Engineers, at George Street, Westminster. Mr. Edward Graves, retiring President, occupied the chair. He opened the proceedings by announcing that the last legal steps had been taken to change the name of their body from "Society of Telegraph Engineers and Electricians," by which name it had hitherto been known, to "Institution of Electrical Engineers." Sir William Thomson, President for the year, then delivered his inaugural address on "Ether, Electricity, and Ponderable Matter."

THE forty-second annual general meeting of the Institution of Mechanical Engineers will be held on Wednesday, January 30, Thursday, January 31, and Friday, February 1, at 25 Great George Street, Westminster, by permission of the Council of the Institution of Civil Engineers. The chair will be taken by the President at 7.30 p.m. on each evening. The President, Mr. Edward H. Carbutt, having been in office for two years, will retire, and will induct into the chair the President-elect, Mr. Charles Cochrane.

THE annual general meeting of the Anthropological Institute of Great Britain and Ireland will be held on Tuesday, the 22nd inst., at half-past eight o'clock p.m. Mr. Francis Galton, F.R.S., will take the chair, and deliver the Presidential address.

AT a meeting of the Council of the Sanitary Institute on January 9, Mr. G. J. Symons, F.R.S., in the chair, it was decided that two courses of twelve lectures for sanitary officers should be held, the first course to begin in March, the second in October.

AT the Central Institution, Exhibition Road, during the spring term, Prof. Armstrong will give about ten lectures on some of the more important current problems in chemistry, on Mondays, at 4.30 p.m., commencing Monday, January 21. The following subjects will be dealt with as far as time permits: the nature of chemical change; the interdependence of chemical change and electrolysis; the molecular composition of gases, liquids, and solids; the nature of solutions; physical constants; laws of substitution and isomeric change as bearing on the problem of the nature of chemical change; valency; geometrical isomerism and allo-isomerism.

ZOOLOGISTS will regret to hear of the death of Dr. Heinrich Alexander Pagenstecher. His "Allgemeine Zoologie," in four volumes, the first of which appeared in 1875, the last in 1881,

is well known. For a good many years he was a Professor of Zoology at the University of Heidelberg. In 1882, after having retired from public life, he was persuaded to accept the post of Director of the Museum of Natural History at Hamburg. His death—which occurred on January 5—was due to heart disease. He was in his sixty-fourth year.

THE death is announced from Southsea, after a very short illness, of Sir William O'Shaughnessy Brooke, F.R.S., in the eightieth year of his age. From 1852 till 1862 he was Director-General of Telegraphs in India. He was created a Knight Bachelor in 1856 for his distinguished services in establishing the electric telegraph service in our Indian possessions.

THE Rev. Churchill Babington, D.D., F.L.S., died on Sunday morning last at Cockfield Rectory. He was well known as a botanist, and contributed to Sir J. Hooker's *Journal of Botany and Kew Miscellany*. Dr. Babington was one of the honorary Fellows of St. John's College, Cambridge.

AT a meeting of the Royal Botanic Society, held last Saturday, the Secretary reported that the recent fogs had done much damage to the plants in the conservatories, causing many of them to shed both leaves and flower-buds. More especially had this been the case with Australian plants, which, from enjoying in their own country a large amount of sunlight, were found less capable than any others of contending against the vicissitudes of London weather. Mr. G. J. Symons, F.R.S., said he believed that fogs were increasing not only in London, but generally. Plants, however, suffered not only from the absence of light, but from the pores of their leaves becoming filled up with the sulphurous, sooty matters contained in London fogs.

INTELLIGENCE received at San Francisco from Hawaii states that Kilauea, the largest volcano in the island, is in a state of eruption.

A NEW mineral of exceptional chemical interest has been discovered by Mr. Sperry, chemist to the Canadian Copper Company, of Sudbury, Ontario, Canada. It is an arsenide of platinum, $PtAs_2$, and is the first mineral yet found containing platinum as an important constituent, other than the natural alloys with various metals of the platinum group. A considerable quantity of the mineral, which takes the form of a heavy brilliant sand composed of minute well-defined crystals, has been thoroughly investigated by Prof. Wells, who names it "sperrylite," after its discoverer; and the crystals have also been measured and very completely examined by Prof. Penfield. The sand is generally found to contain fragments of chalcopyrite, pyrrhotite, and silicates, which may be removed by treatment first with aqua regia to remove sulphides, and afterwards with hydrofluoric acid to remove silicates. After this treatment the sperrylite sand is seen to have remarkably increased in brilliancy, every grain showing extremely brilliant crystal faces, of a tin white colour, resembling that of metallic platinum itself. It is very heavy, possessing at 20° a specific gravity of 10·6. Strangely enough, however, although so heavy, the sand shows a marked tendency to float upon water, owing to its not being easily wet by that liquid; even when the grains do sink, they almost invariably carry down bubbles of air along with them. This peculiar property is retained even after boiling with caustic potash and washing with alcohol and ether, and cannot therefore be attributed to any surface impurities. Sperrylite is only slightly attacked by the strongest aqua regia, even after boiling for days, and it also remains unchanged when heated in a bulb tube to the temperature of melted glass. Heated in an open tube, however, it gives off a portion of its arsenic as a sublimate of the trioxide, the residue then fusing. When dropped upon a piece of red-hot platinum foil it melts, evolving white fumes of inodorou arsenious oxide, and forming a porous excrescence in colour resembling metallic platinum upon the surface of the foil. Analyses show that sperrylite contains 52·5 per cent. of platinum; mere traces of rhodium and palladium, in quantity less than 1 per cent., being also present. Prof. Penfield shows that the crystalline form is cubic, the habit being of the pyritohedral type of hemihedrism, very similar to the various members of the pyrites group, in which an atom of iron, nickel, or cobalt, is united to two atoms of sulphur, arsenic, or antimony. The forms generally developed are the cube {100}, octahedron {111}, pyritohedron π {210}, and occasionally the rhombic dodecahedron {110}. It is very curious that in the treatment with aqua regia, the cube and octahedron faces remain unattacked, while the acids exert a decided action upon the pyritohedral (pentagonal dodecahedral) faces, entirely destroying their power of reflecting light. This similarity between sperrylite and the pyrites of the iron group is rendered all the more important in view of the fact that the platinum and iron groups both occur in the same vertical row (the eighth) in Mendelejeff's periodic classification.

WE referred last week to a proposal for a Meteorological "Congress," to be held in Paris during this year. It should be clearly understood that the meeting in question has originated with the French Meteorological Society, in connection with the Exhibition of 1889, and is quite distinct from the meetings organized by the Committee appointed by the Congress of Rome. This Committee, as stated in Mr. Scott's letter (NATURE, vol. xxxviii. p. 491), has decided to convene a meeting of directors of the meteorological services, at some future time, instead of an official Congress, such as those held at Vienna and Rome; but no such meeting has been arranged by the Committee for the year 1889.

THE Weekly Weather Report issued by the Meteorological Council has been enlarged and considerably improved with the new year. The statistical pages remain unaltered: they refer exclusively to the temperature, rainfall, and sunshine within the United Kingdom. The other portion, which until the end of last year consisted of a brief summary of the weather for each day, accompanied by copies of the daily charts of pressure and temperature over North-Western Europe, has undergone a great change. The area of the charts has been extended so as to cover the whole of Europe, the Mediterranean, Tunis, and Algeria, the hours represented being 8 a.m. for temperature and weather, and 8 a.m. and 6 p.m. for barometer and wind. The enlargement has necessitated the addition of two pages to the Report, the whole forming a most interesting record of the weather over this wide area. The first number is chiefly remarkable for the view it gives us of the Continental anticyclone which spread from Moscow, where the barometer exceeded 31 inches, westward to the Atlantic; the changes in position and intensity being in sympathy with the movements of cyclonic disturbances in the Mediterranean and in the Arctic regions. Towards the close of the week the decreasing intensity of the high-pressure system became favourable to cyclonic weather on our own shores, which afterwards completely dispelled the thick fogs and severe frost. The great cold felt in this country belonged to the same area as that which spread over the Continent, the thermometer being as much as 20° below zero at zero, and gradually rising thence to the shores of the Atlantic.

THE Report of the Smithsonian Institution for the year 1887-88 has been issued. The Secretary, Prof. Samuel P. Langley, refers, as was to be expected, in terms of the highest appreciation, to the character and work of his predecessor, Prof. S. F. Baird. The Institution, as usual, has been doing much excellent work, but Prof. Langley complains that the funds at its disposal are not adequate to its wants. The amount bequeathed by Mr. Smithson, and accepted by Congress, is no longer so valuable as it was half a century ago. "I do not

now refer," says Prof. Langley, "merely to the fact that we measure all things by another scale in 1888 from what we did in 1836; or that, owing to the immense increase of public wealth, the capital of the original bequest, which then was greater than any but a few private fortunes, has become relatively so inconsiderable to-day. More than this is meant. It is meant that the actual purchasing power of each dollar is, for our purposes, notably less; that it is being forced upon us that we cannot print as many books, or pay as many *employés*, or make as many researches, as when the scheme of expenditure was first fixed, and that, consequently, a scheme which was wise then, because not only desirable but feasible, is not necessarily so now." Prof. Langley expresses a hope that an increased income may be provided by the Government and by private benefactors.

We have received the "Bibliography of Astronomy for the Year 1887," compiled by William C. Winlock, and published by the Smithsonian Institution. As a reference list it will be found to be very useful. Most of the various contributions to astronomy published during the year 1887 in the many scientific journals and Transactions of Societies, as well as many more elaborate publications, have been inserted. A few of the titles, as the author says, have been taken from reviews or book-catalogues, and, by means of an alphabetical arrangement of the different subjects of astronomy, references to them can be very easily obtained.

Two weeks ago we printed a brief account of a method employed by Prof. Pickering for enumerating nebulæ photographed in a given part of the heavens, and comparing them with those given in pre-existing catalogues, and by this means increasing the number of known nebulæ. The following is a rather more detailed account of the subject. The region selected for these experiments extends from 5h. 10m. to 5h. 50m. in right ascension, and from $-10°$ to $5°$ in declination. The instrument employed was a Bache telescope, having a photographic doublet, with an aperture of 8 inches and a focal length of 44 inches; and, by means of this, negatives 10° square were obtained, the definition included in a central circular area of 7° in diameter being very good. The work of examining the plates very carefully was intrusted to Mrs. Fleming. Each plate was laid on an inclined plane similar to a retouching desk used in photography, and, by means of a strong magnifying glass, thoroughly studied. Whenever a marking on the negatives appeared to resemble a nebula, the co-ordinates of it were accurately noted, but great care was taken to make sure that the marking was not due to a piece of dust or a defect in the film. The approximate right ascensions and declinations of these objects were next determined from the configuration of the adjacent stars on the charts of the *Durchmusterung*. Then, by comparing the measurements obtained on the plates, and those on the charts, several of the markings coincided, thereby showing that they had been both photographed and catalogued, while in some cases those which had been catalogued had not been photographed, and in other cases some not catalogued were photographed. From the twenty-seven negatives taken fourteen of the objects on them were contained both in the photographs and in the catalogue. Four in the catalogue were not photographed, while twelve, which were not catalogued, were detected on the negatives, and so were probably new. In some instances the back of the plate was covered with a film of shellac and lampblack so as to absorb the light from the rear surface of the plate. The region covered in the above experiments was about four thousandths of the entire sky, so that by photographing the whole heavens a great number of new clusters and nebulæ would be brought to light. From these experiments a very interesting result is disclosed, especially as regards the four nebulæ which were in the catalogue and were not photographed, which suggests that these nebulæ gave different spectra in the ultra-violet from those that were photographed and not catalogued

We have received Part 4, vol. ii., of the Journal of the College of Science, Imperial University, Japan. It contains papers on the determination of the thermal conductivity of marble, by Kenjirō Yamagawa; on the combined effects of torsion and longitudinal stress on the magnetization of nickel, and on the magnetization and retentiveness of nickel wire under combined torsional and longitudinal stress, by H. Nagaoka; and on the specific volume of camphor of borneol, by Mitsuru Kuhara.

A descriptive Catalogue of the Sponges in the Australian Museum, Sydney, by Dr. R. von Lendenfeld, has been printed by order of the trustees of that institution. Dr. Lendenfeld was intrusted with the compilation of this work in 1885. Early in the following year he changed his residence from Sydney to London, so that some delay was rendered inevitable. He considers, however, that the loss of time was amply made up by the advantage he enjoyed of being able to study the collections in the British Museum, and to compare the types of other authors with the Australian Sponges.

The Executive Committee for the reception of the British Association on their visit to Bath met on Thursday last, when it was announced that, after paying all expenses, there remained a surplus of £950. It was decided, with one dissentient, to recommend to the subscribers that the balance be retained intact to form the nucleus of a fund for building an art gallery in Bath.

A few weeks ago (says the *Cheltenham Examiner*), Mr. Francis Day, of Kenilworth House, Pittville, presented a fine collection of Indian birds to a Museum at Cambridge. Mr. Day has now given the remainder of his zoological collection to the British Museum. The gift consists of about 1500 specimens of Indian fishes, 500 of which are stuffed, and the remainder preserved in spirit. There are also a large quantity of English fishes, both stuffed and in spirit; about 1000 specimens of Crustaceans, collected from all parts of India and Burmah, and some British Crustacea, including some from the collection of Prof. Malm. Among the Indian fishes are specimens from the celebrated collection of Dr. Jordan, and the British fishes include especially interesting hybrid *Salmonidæ*, from Sir James Maitland's fish-farm at Howietoun in Scotland, in which Mr. Day takes great interest. There is, in addition, a miscellaneous collection of zoological specimens from the East, including a large crocodile.

The following arrangements have been made at the Royal Institution of Great Britain:—On Tuesday next, January 22, Prof. G. J. Romanes will begin the second part of his course of lectures entitled "Before and after Darwin," the subjects being "The Evidences of Organic Evolution and the Theory of Natural Selection." On Thursday next, January 24, Prof. J. W. Judd, F.R.S., will begin a course of four lectures on "The Metamorphoses of Minerals." At the first Friday evening meeting of the season, January 25, at 9 p.m., Prof. G. H. Darwin, F.R.S., will give a discourse on "Meteorites and the History of the Stellar Systems."

The evening lectures at the Royal Victoria Hall for the next few weeks are arranged as follows:—January 22, Mr. J. D. MacClure, "Shooting Stars"; January 29, Mr. F. W. Rudler, "Coal"; February 5, Prof. Reinold, "Torpedoes"; February 12, Commander Cameron, "Africa and the Horrors of Slavery."

The additions to the Zoological Society's Gardens during the past week include a Vulpine Phalanger (*Phalangista vulpina* ♂)

from Australia, presented by Mr. F. Buckland; a Common Paradoxure (*Paradoxurus typus* ♀) from India, presented by the Rev. J. De Gruchy; a Tawny Owl (*Syrnium aluco*), European, presented by Mr. T. Gunn; a Stump-tailed Lizard (*Trachydosaurus rugosus*) from New Holland, presented by Mr. C. Elliot; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. C. L. Curtis; a Bonnet Monkey (*Macacus sinicus* ♀) from India, two Red-backed Pelicans (*Pelecanus rufescens*) from West Africa, a Masked Parakeet (*Pyrrhulopsis personata*, yellow var.) from the Fiji Islands, deposited; five Clotbey's Larks (*Ramphocorys clotbeyi*), five Algerian Shore Larks (*Otocorys bilopha*), two Rosy Bullfinches (*Erythrospiza githaginea*) from Algeria, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *JANUARY* 20–26.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 20

Sun rises, 7h. 56m.; souths, 12h. 11m. 24·4s.; sets, 16h. 26m.: right asc. on meridian, 20h. 11·7m.; decl. 20° 1′ S. Sidereal Time at Sunset, 0h. 27m.

Moon (at Last Quarter January 24, 16h.) rises, 19h. 0m.*; souths, 2h. 29m.; sets, 9h. 45m.: right asc. on meridian, 10h. 27·3m.; decl. 13° 31′ N.

Planet.	Rises.		Souths.		Sets.		Right asc. and declination on meridian.	
	h. m.		h. m.		h. m.		h. m.	° ′
Mercury..	8 42	...	13 12	...	17 42	...	21 12·5	... 17 44 S.
Venus.....	9 37	...	15 7	...	20 37	...	23 7·3	... 6 31 S.
Mars	9 24	...	14 41	...	19 58	...	22 42·2	... 9 11 S.
Jupiter....	5 50	...	9 46	...	13 42	...	17 45·7	... 23 3 S.
Saturn....	17 55*	...	1 26	...	8 57	...	9 24·1	... 16 26 N.
Uranus ...	0 0	...	5 23	...	10 46	...	13 22·2	... 7 59 S.
Neptune..	12 7	...	19 50	..	3 33*	...	3 51·2	... 18 25 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Meteor-Showers.

	R.A.	Decl.	
	°	°	
Near κ Ursæ Majoris ...	134 ...	48 N.	
From Coma Berenices ...	180 ...	24 N. ...	Swift; streaks.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·5 ...	81 17 N. ...	Jan. 23,	20 52	*m*
S Persei	2 14·9 ...	58 5 N. ...	,, 26,		*M*
Algol	3 1·0 ...	40 32 N. ...	,, 21,	23 17	*m*
			,, 24,	20 6	*m*
λ Tauri...	3 54·6 ...	12 11 N. ...	,, 25,	1 18	*m*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	,, 22,	23 0	*M*
R Canis Majoris ...	7 14·5 ...	16 11 N. ...	,, 20,	20 17	*m*
		and at intervals of		27 16	
T Hydræ	8 50·3 ...	8 43 S. ...	Jan. 20,		*M*
W Virginis	13 20·3 ...	2 48 S. ...	,, 22,	22 0	*m*
V Boötis	14 25·3 ...	39 21 N. ...	,, 20,		*M*
R Boötis	14 32·3 ...	27 13 N. ...	,, 22,		*M*
T Vulpeculæ ...	20 46·8 ...	27 50 N. ...	,, 21,	1 0	*m*
			,, 22,	3 0	*M*
Y Cygni	20 47·6 ...	34 14 N. ...	,, 21,	5 40	*m*
		and at intervals of		36 0	
δ Cephei	22 25·0 ...	57 51 N. ...	Jan. 23.	4 0	*M*
			,, 26,	22 0	*m*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

The following letter from Mr. Stanley to Tippoo Tib has been received in Brussels:—"Boma of Banalya-Murenia, August 17, to the Sheik Hamed Ben Mahomed [Tippoo Tib] from his good friend, Henry Stanley. Many salaams to you. I hope you are in good health, as I am, and that you have remained in good health since I left the Congo. I have many things to say to you but I hope I shall see you face to face before many days. I reached this place this morning with 130 Wangwana, and three soldiers and sixty-six natives belonging to Emin Pasha. This is now the eighty-second day since we left Emin Pasha on the Nyanza, and we have only lost three men all the way. Two of them were drowned, and the other ran away. I found the white men whom I was looking for. Emin Pasha was quite well, and the other white man, Casati, was quite well also. Emin has ivory in abundance, cattle by thousands, and sheep, goats, fowls, and food of all kinds. We found him to be a very good and kind man. He gave numbers of things to all our white and black men, and his liberality could not be exceeded. His soldiers blessed our black men for their kindness in coming so far to show them the way, and many of them were ready to follow me at once out of the country. But I asked them to stay quiet a few months that I might go back and fetch the other men and goods I had left at Yambunga, and they prayed to God that he would give me the strength to finish my work. May their prayer be heard. And now, my friend, what are you going to do? We have gone the road twice over. We know where it is bad and where it is good, where there is plenty of food and where there is none, where all the camps are and where we shall sleep and rest. I am waiting to hear your words. If you go with me it is well. If you do not go it is well. I leave it to you. I will stay here ten days, and then I go on slowly. I move from here to a big island two hours' march from here, and above this place there are plenty of houses and plenty of food for the men. Whatever you have to say to me my ears will be open with a good heart, as it has always been towards you. Therefore if you come, come quickly; for on the eleventh morning from this I shall move on. All my white men are well; but I left them all behind, except my servant William, who is with me.—(Signed) Stanley.' This letter, which was brought by a messenger to Stanley Falls, reached Brussels by post on Tuesday evening, and is the only one from Mr. Stanley which has reached the coast. The remainder of the letters brought by the messenger remain at Stanley Falls, and will arrive in Europe two or three months hence.

Another of the few remaining mysteries of African geography has just had a little light shed upon it. For many years a lake has been conjecturally placed upon our maps some 15° to the east of the Cameroons, under the name of Liba. No white traveller has ever seen it. Quite recently, however, Dr. Zintgraff, who has been exploring in the Cameroons interior, has obtained information from some natives of the region in which Lake Liba is placed, that leads him to the conclusion that the so-called Lake Liba is probably only a lake-like expansion of a river of that name which exists in the country of his informants. Should the statements of the natives be confirmed, it would seem that the lake, or rather river, to which it belongs is connected neither with the Congo nor the Shari.

Further light has been thrown upon the important question of the supposed waterway between Macluer Inlet and Geelvink Bay, in New Guinea, the existence of which was reported by Captain Strachan. It appears that Dr. A. Meyer's explorations, the results of which seem incompatible with Captain Strachan's conclusions, have recently received important confirmation from the investigations of certain Dutch officials. Lieut. Ellis, who explored the north and north-east coast of New Guinea from May to November 1887, was unable to find the reported water connection, or to gain any information about it from the natives. His own investigations and the inquiries instituted by him force him to the certain conclusion that no such connection exists; and in this he is supported by the opinion of Dr. Host, another explorer.

Dr. Schweinfurth is at present engaged in exploring the little-known region of the Menakha Mountains. Towards the end of last year he left Aden for Hodeida, on the Red Sea, for the purpose of visiting these mountains and the town of Sana. Dr. Schweinfurth carried letters from the Porte recommending him to the care and protection of the authorities; and as he is liberally supplied with funds from Berlin, he hopes to make a thorough exploration of the district, which has been but little visited by Europeans.

The number of the *Zeitschrift* of the Berlin Geographical Society containing the geographical bibliography of the past year has just been issued. As usual it contains a practically exhaustive list of all publications, papers, and maps that have appeared in the various departments of geography.

THE STRASSBURG BOTANICAL INSTITUTE.

IN the American *Botanical Gazette* for December 1888 (vol. xiii. No. 12) there is a paper by Mr. William R. Dudley on the Botanical Institute at Strassburg. This paper is valuable and interesting as showing the sort of provision for botanical study that is thought right and necessary in Germany. The Institute forms part of the new University buildings of Strassburg. Mr. Dudley gives plans of the ground floor and first floor, and from these it appears that a considerable portion of the building is reserved as a residence for the Director and his family, and that two rooms are allotted to the Director's assistant, usually a young man who has recently taken his degree as a doctor. On the ground floor, besides the living-rooms, are a larger and smaller lecture-room, a "*Lehrsammlung*" or illustrative museum, and a "preparation-room," which is used in the preparation of lectures, and is also found useful by those who wish to carry on work in connection with the museum. On the first floor a large part of the space is given up to laboratories. It includes also an herbarium, a library, a weighing-room, a chemical-room, a dark room, and a small greenhouse.

After some introductory statements, Mr. Dudley continues as follows:—

No doubt the architect who designed this building is accountable for cutting it up into symmetrical squares; any German architect who failed in this would be sure to die unhappy. Nevertheless, for the sequence of the rooms and for the details, De Bary was responsible, and, taking everything into consideration, it is considered in Germany their best single laboratory for botany.

Its chief characteristics are the abundance of all necessary appliances and apparatus, cleanliness and orderly disposition of all its supplies, good light from huge windows and white wall-surfaces. Wall-cases are numerous, and the contained glassware, reagents, &c., nicely arranged. Drawers are abundant—this one containing only reagent tubes, that glass plates, another pipettes, burettes, &c. Running water is convenient, of course, and distilled water and three grades of alcohol where they can be readily obtained by students if necessary. There are several sterilizing boxes in the large laboratories; also constant-temperature boxes provided with thermostats. The chemical-room is provided with a hood for fumes and for the steam generated by the steam sterilizing cylinders. Gas is provided at each table, and a separate room is set apart for delicate instruments, such as balances. Indeed the association and dissociation of rooms and apparatus, the conveniences, the absence of unnecessary things and showy effects, indicate the intelligence and discernment of a worker and a master.

The tables are broad, very heavy, and designed so as to prevent warping or seaming. They are convenient for two beginners or a single special student. Each person is provided, at the outset, with about a dozen common reagents and fluids. The microscopes for laboratory use are chiefly Hartnack. Most of the private microscopes in the laboratory at the time I was there were from Seibert, an excellent Wetzlar manufacturer, not well known in America, and one or two from Zeiss. The stock of reagents in the cases is large, and, if necessary, new ones will be cheerfully ordered. The University requires of special students working every day in the laboratory, a payment of fifteen dollars, which covers all necessary expenses.

Strassburg University had about 1000 students during the winter semester of 1887–88, and 104 professors, *privat-docents*, and assistants. It is, therefore, neither one of the largest, nor one of the smallest, of Germany's twenty-one Universities.

The Botanical Laboratory had six advanced and five beginning students, and I do not think the number was affected by De Bary's illness. To instruct or counsel these were four instructors: the Professor; the associate Professor, Dr. Zacharias; the *privat-docent*, Dr. Wortman; and the assistant, Dr. Jost—all contributors, in a greater or less degree, to science, and of course well-trained men. At least three of the advanced students were working quite independently during De Bary's illness, although it was the latter's custom to inquire nearly every day after the work of the advanced students, when he was in health. But the German Government, which employs and pays these instructors, is not afflicted with that particular kind of malaria which enters into the management of almost every American institution, and gives it alternate chills and fever over fall and rise in numbers. Numbers are a matter of indifference to it. A very distinguished German Professor once said to me: "The truth is, we teach whatever we please, we do as much or as little as we please, and the Government does not interfere with us." Yet these men teach enthusiastically, and accomplish in scientific research ten times as much as the American Professor, who is "personally conducted" by a whole Board of Trustees. The German Government *does* "personally conduct," however, in certain very important matters. In the first place, it provides a suitable *corps* of assistants, and makes it sure, therefore, that the Professor has *not* too great a burden of teaching on his hands. It provides ample appropriations; it appoints its Professors for merit, and it sends up its students from the secondary schools with an excellent and uniform training.

The advanced students were mostly engaged in bacteriological investigations, although one was working out certain biological questions of fern development. Prof. Zacharias was engaged in histological work, Dr. Wortman in physiology, and Dr. Jost completed a paper during the winter on the morphology of certain mistletoes.

In the "*Lehrsammlung*" are numerous beautiful preparations, some made by De Bary, and at once recognizable as the originals of well-known figures in his published works; and some by former pupils, some of whom are now famous men. These preparations are frequently used in illustrating the lectures, all of which were held late in the afternoon or in the evening.

The herbarium collection is not relatively large, and is situated rather remote from the other rooms. Had De Bary been a systematist, he would, no doubt, have placed his herbarium centrally. Instead, the large laboratories, the rooms which have seen so many distinguished investigators, and witnessed so many scientific discoveries under the guidance of the great Director, are the rooms around which the others are clustered.

The library, stocked with a fairly good number of the important serials, together with a few standard works in the principal departments of botany, is placed nearer the laboratory; and in this, every Monday evening, meets the "Botanical Colloquium," made up of the advanced students of the laboratory and the instructors. Certain members give carefully prepared abstracts and reviews of the current botanical literature, which are followed by spirited discussions. After an hour or more of arduous and profitable labour of this kind, by means of which each member is enabled to keep quite abreast of advanced lines of work, they adjourn to a more convivial place, and spend the remainder of the evening in the relaxation natural to the German. By eleven o'clock all their vast learning, and especially the hard facts of the recent Colloquium, are in a state of saturated solution, and by next morning are quite ready for use.

INDUSTRIAL EDUCATION.

MAY I ask you to publish and invite criticism on the inclosed Bill, which has been read a first time in the Kensington Parliament? It is put forward as not antagonistic to, but rather as including (see Clause 8), the academic schemes of technical education with which we are familiar. I write as one who was at a primary school, who has worked at the bench, who has great reason to be grateful to the Science and Art Department, who has been a master at a public school, a manager of works, and an employer of labour. JOHN PERRY.

10 Penywern Road, South Kensington, S.W.,
December 28, 1888.

A Bill for Technical Industrial Education.

Whereas it is expedient to make provision for Technical Industrial Education in England and Wales:

Be it therefore enacted, &c.

(1) This Bill may be cited as the Technical Education Bill 1889, and shall not extend to Scotland or Ireland.

(2) "Apprentice" means any boy of less than 18, or any girl of less than 17 years of age employed, whether under indentures or not, in any place which, under the Factories or Workshops Acts, is denominated a factory or workshop, or in any warehouse, shop, office, or other place of business, or for wages, or other remuneration, in any place of employment. But apprentice so defined shall not include any menial or

domestic servant. "Master" means the employer of any apprentice as hereinbefore defined.

"School Authority" means the School Board exercising jurisdiction in the district in which the place of employment is situated, or any elected body which may take over the powers o such School Board; and in places where there is no School Board, it means the County or Borough Council under the Local Government Act, 1888, or the Municipal Corporations Acts.

"Technical Education" is an education in the scientific and artistic principles which govern the ordinary operations in any industry.

"Technical School" means a place for technical education, whether established and maintained—

(*a*) By the School Authority, and open to all apprentices;

(*b*) By voluntary effort, and open to the apprentices of more than one master;

(*c*) Or by a master for his own apprentices.

"Inspector" means the Inspector of Factories in whose district the place of employment is situated, or if there be no such inspector, then the School Board Visitor for such district.

(3) The Education Department shall forthwith and from time to time prescribe regulations in conformity with the rules for the time being of the Science and Art Department, in the subjects in respect of which Parliamentary grants are made by the Science and Art Department, for the formation and instruction of classes of elementary school children who have passed the Fourth Standard, and thereupon the School Authority may form such science and art classes, and provide such instruction accordingly, and earn such grants, and may assign such grants to the teachers of such classes, or may otherwise provide for their remuneration.

(4) Every master shall provide each of his apprentices with technical education at a technical school.

(5) Every apprentice shall devote at least two hours a day, five days in the week, during working hours, to study at a technical school.

(6) The School Authority shall annually in January prescribe the time for such study, having regard to the usual working hours in places of employment in their district, and shall publish a table of the times so prescribed. A printed copy of such table shall be conspicuously exhibited by the master in every such place of employment in such positions for such times and in such type and form as the School Authority shall prescribe.

(7) The School Authority shall have power to establish and maintain such technical schools as may be necessary to accommodate and provide technical education for all apprentices in their district whose masters do not otherwise efficiently provide for the technical education of their apprentices. The master of each apprentice shall pay the prescribed fees for his tuition at such schools.

(8) The technical schools established and maintained by the School Authority may provide technical education for persons other than apprentices.

(9) The course of studies at such schools, and fees payable for the same, shall be prescribed from time to time by the School Authority, subject to the sanction of the Education Department.

(10) The inspector shall inform himself as to the sufficiency of the technical education given to apprentices in his district, and report thereon to the School Authority and the Education Department at such times and in such manner as they shall respectively prescribe.

The duties, powers, and penalties relating to the office of inspector, specified in the Factories and Workshops Act, 1878, shall be applicable to any inspector under this Act, and to any place of employment within the provisions hereof.

The inspectors shall be paid by the School Authority such remuneration for their services under this Act as the Education Department shall approve.

(11) All offences under this Act shall be prosecuted, and all fines under this Act shall be recovered on summary conviction before a Court of Summary Jurisdiction in manner provided by the Summary Jurisdiction Acts.

The provisions of the Factories and Workshops Act, 1878, and the Acts amending the same as to legal proceedings and appeals, shall be deemed to be incorporated in, and made applicable to, this Act.

The punishment for any offence under this Act shall be a fine not exceeding £5.

(12) The expenses of carrying this Act into execution shall be defrayed by the School Authority, who shall have power to provide for such expenditure by moneys raised, precepts issued, or rates levied under their powers. Separate statements of such expenditure shall be furnished annually to the Education Department.

ZOOLOGICAL NOTES FROM TORRES STRAITS.

Caudal Respiration in Periophthalmus.—At the Birmingham meeting of the British Association, in 1886, Dr. S. J. Hickson pointed out that the species of Periophthalmus which he had observed in the Celebes always rested with its tail immersed in water, although the body was out of the same. I do not know whether any experiments have been made on this fish, but I have made a few which tend to show that this remarkable animal largely respires by means of its caudal fin. The experiments were made on specimens obtained from a Mangrove swamp on the Island of Mabuiag (Jervis Islands), and may be summarized as follows:—A specimen totally submerged in the sea was perfectly well and lively after forty-two hours. A second specimen lived a day and a half in a vessel containing just sufficient water to keep the tail-fin submerged, but not enough for respiration by means of the gills. (It is possible that the fish would have lived longer, if the sea-water had been continually renewed.) Fish with the caudal fin coated over with gold size, when put in a vessel of sea-water, only lived, on an average, from twelve to eighteen hours, although they could utilize their gills for respiration; others kept under similar circumstances, but not anointed with gold size, lived a day or two, apparently in perfect health. On submitting the caudal fins to the microscope, the circulation of the blood appeared to be exceptionally vigorous. I hope to be able to further test these observations on a future occasion.

The Employment of the Sucker-fish (*Echeneis*) *in Turtle-fishing.*—The only two references to the employment of the sucker-fish in turtle-fishing which I have by me are those in Dr. Günther's "Introduction to the Study of Fishes," and the "Narrative of the Voyage of H.M.S. *Rattlesnake*," by J. Macgillivray. The latter (vol. ii. p. 21) states that he was informed that the natives of Morulug (Prince of Wales Island), Torres Straits, catch a small species of turtle in the following manner:—"A live sucker-fish (*Echeneis remora*), having previously been secured by a line passed round the tail, is thrown into the water in certain places known to be suitable for the purpose; the fish while swimming about makes fast by its sucker to any turtle of this small kind which it may chance to encounter, and both are hauled in together!" Dr. Günther (*l.c.* p. 461) throws doubt upon the habitual utilization of the *Echeneis* for this purpose. In the Straits there are two periods for turtle-fishing, the one during October and November, which is the pairing season, and when turtle are easily speared owing to their floating on the surface of the water; the other, during the remaining months of the year when the turtle frequent the deeper water and the channels between the reefs. It is then that the sucker-fish—or, as the natives term it, "Gapu,"—is utilized. I have, at present, no means of determining the species of *Echeneis* common in the Straits. I believe it to be *E. naucrata*, as the species here attains a greater length than *E. remora*. When going out turtle-fishing, a Gapu is caught, and the more experienced natives have no great difficulty in procuring one when it is required. A hole is made at the base of the caudal fin by means of a turtle-bone, and the end of a very long piece of string is inserted in the hole and made fast. The end of a second, quite short, piece of string, is passed through the mouth and out by the gills. By means of these two strings the fish is retained, while slung over the sides of the canoe, in the water. When a turtle is sighted deep down in the water, the front piece of string is withdrawn, plenty of slack being allowed for the hind string. The Gapu on perceiving the turtle immediately swims towards it, and attaches itself to the reptile's carapace. A man, with a long rope attached to an upper arm, dives into the water and is guided to the turtle by the line fastened to the Gapu's tail. On reaching the turtle, the man gets on its back, and passes his arms behind and below the fore-flappers, and his legs in front and below the hind-flappers. The man is then rapidly drawn up to the surface of the water bearing the turtle with him. On the arrival of the

diver the Gapu usually shifts its position from the carapace to the plastron of the turtle. At the end of the day's fishing the Gapu is eaten. The natives have a great respect for the Gapu, and firmly believe the fish possesses supernatural powers. For example, when there is something the matter with the bow of the canoe, the Gapu is said to attach itself to the neck or the nuchal plate of the turtle; when the lashings of the outrigger to the thwart poles are insecure, the Gapu is believed not to stick fast to the turtle, but to continually shift its position; if the strengthening ties in the centre of the hold of the canoe are faulty, the Gapu is stated to attach itself to the turtle and then immediately to swim away. More than once I was told, "Gapu savvy all the same as man; I think him half devil." The sucker-fish is not used to haul in the large green turtle. I was repeatedly told that it would be pulled off, as the turtle was too heavy. The above information was gathered from several sources, and checked by means of much questioning.

Amphioxus.—A species of Amphioxus, apparently very similar to *A. lanceolatus*, was not uncommon at one spot at Mabuiag, at a depth of from 3 to 4 fathoms. A species of this animal is catalogued as follows by Mr. Krefft, in his list of "Australian Vertebrata, Fossil and Recent": "*Branchiostoma lanceolatum*. Dredged in Bass's Straits, by H.M.S. *Herald*, at a depth of from 10 to 12 fathoms." I am not aware whether it has been found elsewhere in Australian waters.

ALFRED C. HADDON.

Thursday Island, November 12, 1888.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Sedgwick triennial prize has been awarded to Mr. Alfred Harker, Fellow of St. John's College. The subject of the essay is "The Petrology of the Igneous Rocks associated with the Cambrian (Sedgwick) of Carnarvonshire."

SCIENTIFIC SERIALS.

IN the number of the *Journal of Botany* for December 1888, Mr. S. Le M. Moore has an interesting article on photolysis in *Lemna trisulca*, in which he contests some of Stahl's conclusions as to the effect of day and night on the relative positions of the chlorophyll-grains on the cell-walls. The remaining articles, both in this number and in that for January 1889, are chiefly of interest to geographical or systematic botanists. Messrs. Britten and Boulger's "Biographical Index of British and Irish Botanists" has now advanced as far as the letter G.

IN the *Botanical Gazette* for November 1888, Miss E. L. Gregory completes her account of the development of cork-wings on certain trees, the trees described in the present instalment being species of *Acer* and *Liquidambar*.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 22, 1888.—"Report of Researches on Silicon Compounds and their Derivatives. Part I." By J. Emerson Reynolds, M.D., F.R.S., Professor of Chemistry, University of Dublin.

The present investigation was undertaken some years ago with a view to examine the action of the silicon haloids—but more especially of silicon tetrabromide—on various compounds containing nitrogen, as our knowledge of the relations of silicon and nitrogen is extremely limited.

It was ascertained at an early stage of the inquiry that the bromide of silicon is much superior to the chloride as a reagent with nitrogenized compounds, but since the bromide had apparently not been obtained in any quantity even by its discoverer, Serullas, considerable time had to be devoted to working out a method for the production of a sufficiently large supply of this material. The method adopted is described in the full paper.

In the purification of the crude tetrabromide a new *chlorobromide*[1] *of silicon* was discovered, which boils at 141° C. This proved to be the compound $SiClBr_3$, which was required to complete the series of possible chlorobromides of silicon.

The first group of nitrogen compounds subjected to the action of silicon tetrabromide included the primary thiocarbamide or sulphur urea, obtained by the author in 1869, and the allyl-, phenyl-, and diphenyl-thiocarbamides.

All these are shown to unite with silicon tetrabromide, and afford the highly condensed compounds—

$$(H_4N_2CS)_8SiBr_4,$$
$$(C_3H_5,H_3N_2CS)_8SiBr_4,$$
$$(C_6H_5,H_3N_2CS)_8SiBr_4,$$
$$((C_6H_5)_2H_3N_2CS)_8SiBr_4.$$

These are more or less vitreous solids, with the exception of the allylic compound, which is a transparent and singularly viscous liquid. All are dissolved and decomposed by water and by alcohol.

The action of alcohol on the compound $(H_4N_2CS)_8SiBr_4$ was studied in detail, and it was shown that not only do ethyl bromide, thiocyanate, and diethylic silicate result, but that the representatives of two new classes of thiocarbamide derivatives are formed.

The first of these is a beautiful *tetrathiocarbamide* compound whose formula proved to be—

$$(H_5N_2CS)_4NBr,$$

which may obviously be written—

$$(H_4N_2CS)_4H_4NBr.$$

This body separates from alcohol in fine masses of crystals resembling sea anemones in appearance, which melt at 173°–174°, and begin to decompose at 178°–180°. The synthesis of this substance was effected by heating ammonium bromide with thiocarbamide.

Several homologues of the above *tetrathiocarbamidammonium bromide* were produced by synthetic methods; some of these contain chlorine or iodine instead of bromine. The following are examples of the compounds found in the course of this part of the investigation:—

$$(H_4N_2CS)_4H_4NBr,$$
$$(H_4N_2CS)_4H_4NCl,$$
$$(H_4N_2CS)_4H_4NI,$$
$$(H_4N_2CS)_4(CH_3)H_3NBr,$$
$$(H_4N_2CS)_4(C_2H_5)_2H_2NBr,$$
$$(H_4N_2CS)_4(C_2H_5)_3HNCl.$$

By the action of silver nitrate on the tetrathiocarbamidammonium bromide the crystalline *dithiocarbamide* compound with silver bromide was obtained—

$$(H_4N_2CS)_2AgBr.$$

This was subsequently produced by the direct union of thiocar bamide with the pure silver haloid. The compound—

$$(H_4N_2CS)_2AgCl$$

was also obtained in fine crystals, as were other similar substances.

A *trithiocarbamide* compound is also formed during the action of ethyl alcohol on $(H_4N_2CS)_8SiBr_4$, but it is much more soluble than that which first separates. It is also crystalline, and its analysis and reactions lead to the formula—

$$(H_5N_2CS)_3Br,C_2H_5Br.$$

Hitherto only mono- and di-thiocarbamide derivatives have been known, but the results above stated in outline prove that tri- and tetra-thiocarbamide compounds are formed in presence of silicon tetrabromide and certain other agents, which latter form addition products with the condensed amide.

So far, cases were only dealt with in which silicon tetrabromide combined with nitrogenized groups without loss of its halogen. The next stage of the inquiry involved the investigation of certain interactions in which the tetrabromide loses all its halogen. One of the chief results obtained in that direction forms the subject of a separate communication.

[1] The chlorine required for the production of this compound was derived from the crude bromine (which always contains chloride of bromine) used in preparing the tetrabromide.

Mathematical Society, January 10.—J. J. Walker, F.R.S., President, in the chair.—Mr. Basset made a few remarks on the steady motion and stability of dynamical systems.—Dr. Glaisher, F.R.S., gave several forms of expression of Bernoulli's numbers derived from the consideration of lemniscate functions.—The President (Sir J. Cockle, F.R.S., in the chair) read a paper on results of ternary quadratic operators on products of forms of any orders.—Mr. Jenkins communicated a note by Mr. R. W. Christie on a theorem in combinations.

EDINBURGH.

Royal Society, December 3, 1888.—A restatement of the theory of organic evolution, by Prof. Patrick Geddes. In the introduction to this paper Prof. Geddes gives to the four customary divisions of biology more general meanings. He considers that morphology, besides being a description of individual forms, deals with specific and generic ones. Taxonomy is thus the higher and more generalized morphology. Embryology includes the description of the development of species and genera in addition to that of individuals. Morphology and embryology, then, deal with the descriptions of the form, and the development of the form, not only of individuals but of races. Similarly, physiology is applied to the description of the functions of the individual, and also to those of species, genera, and higher groups. As the description of the relations of organs characterized the physiology of the individual, so that of interspecific adaptations is the physiology of the race. As morphology and embryology are related, so are physiology and ætiology. Ætiology gives the laws of variation of individual and race. It deals not merely with functions in a balanced state in the individual, and perfected adaptation between races, but also with the origin of both of these in the temperament of the unit organism, and of the sum of organisms. This question has to be separated from the deeper one of the origin of organismal temperament in the influence of environment or otherwise. The following diagram shows these relationships:—

	Structure.		Function.	
	Embryology.	Morphology.	Physiology.	Ætiology.
Group ...		Linnæus	Darwin	
Class ...				
Order ...				
Genus ...				
Species ...				
Species unit (pair, &c.)			....	......>
Individual				
Organ ...				
Tissue ...				
Cell ...				

From this division of the field of biology, a clearer and more exact estimate of Mr. Darwin's position can be had. It is plain that the theory of natural selection, which Mr. Romanes rightly describes as rather that of "the origin of adaptations," is part of the higher physiology, or the relation of races to one another. It is thought by many that Mr. Darwin dealt with problems of ætiology—that he described the origin of the functions in the individual. But he openly deferred the consideration of the laws of variation, and confessed entire ignorance of them. He, indeed, at different times, had two impressions of the import of natural selection. Like others, he sometimes makes the mistake of thinking that an account of adaptations, which species acquire, explains their origin. At other times he clearly sees that there must be a science of variation—an ætiology—which shall tell of the origin of variations acted on by natural selection to form the raw material of adaptations. Both pre- and post-Darwinian writers have dealt with the explanations of variations as arising from temperament. The former have theorized in a general way; the state of their knowledge not allowing them to prove that variation is definite. This point of view must again be taken, and all recent results read from it. The object of the present paper is to show how this may be accomplished throughout the organic world, as the author has already done in finding a definite *rationale* of sex and reproduction. Prof. Geddes then took up the matter in detail for the vegetable world, under such headings as: inflorescence; floral structure; floral colour; the antithesis between floral and grassy types; variations in the leaf; thorns and spines; evergreens; corelations between the reproductive and vegetative systems. The classes of the animal kingdom were treated *seriatim*, the definite lines of variation being traced from the synthetic types in each. He next showed, and illustrated with masses of detail, that throughout a great number of species there are individuals with vegetative and others with reproductive diathesis; and similarly in every genus. Some species are more vegetative, some more reproductive in character; and so, further, of orders and large groups. The vegetative or self-maintaining activities are opposed to, and balanced by, the reproductive or species-maintaining ones. The history of the individual life, or of the development of the race, is a series of alternations between predominating vegetation with subordinate reproduction, and prevailing reproduction with diminished vegetation. The differentiation of sex, the development of parental care and of sociality, are the most obvious results of the reproductive, the race-maintaining diathesis; and these play at least as important a part in organic progress as struggle for individual advantage. In conclusion, Prof. Geddes contrasted his own views of the process of nature, as a materialized ethical process, with that of Prof. Huxley, expressed in his *Nineteenth Century* article, where he considers organic evolution an intellectual but not a moral process. A second paper is to follow, carrying out the argument into the ethical, social, and economic relations of humanity.

BERLIN.

Meteorological Society, December 4, 1888.—Dr. Vettin, President, in the chair.—Dr. Andries developed an original theory as to the constitution of the sun, by which he explained a large number of phenomena. During the discussion which ensued, the theory was attacked from various sides.—Prof. von Bezold made a report on Prof. Kiessling's book, "Untersuchungen über die Dämmerungserscheinungen" ("Researches on the Phenomena of Twilight"), after he had briefly alluded to the recent and more comprehensive work of the English Commission on the Krakatão eruption, which had appeared simultaneously with that of Prof. Kiessling. He pointed out that these two works complement each other, inasmuch as Prof. Kiessling had confined himself entirely to the optical phenomena arising out of the eruption, describing them fully, and illustrating them by physical experiments, while the Commission had dealt comparatively briefly with these phenomena.—Dr. Less spoke on falls of snow during high temperatures. On the morning of November 20 the temperature was 9° C.; it reached a maximum of a little over 11° between 9 and 10 a.m., and then fell irregularly with repeated showers of rain to about 3° C. At 9.45 a.m., when the temperature was above 11°, one of the watchers in the Meteorological Institute announced that he had observed some few flakes of snow falling with the commencing rain. Since the speaker could not find anybody from among his acquaintances who could confirm the above observation, he addressed himself to the public at large by means of the newspapers: he thus obtained very valuable and reliable reports, not only from various parts of Berlin, but also from outlying districts, of snow having fallen, either in solitary large flakes or in larger quantities, at temperatures as high as 9° to 11° C. Dr. Less had once before in this year (1888) observed the same phenomenon, on May 8, when the temperature of the air was 12° C. On going over the literature of this subject in the synoptic weather reports for Germany for the years 1876 to 1888, he came upon twenty-eight cases in which snow had fallen, either in larger quantities or as solitary flakes, when the temperature was above 5° C. He explained the formation of the snowflakes as the result of low-lying currents of air whose temperatures were much lower than those at the earth's surface. Out of the twenty-eight cases quoted above, eleven were accompanied by marked and wide-spread thunder during the ensuing twenty-

four hours. This circumstance may be taken as supporting Sohncke's theory of aërial electricity, according to which the electricity during a storm results from the friction of drops of ice and water, and this can only take place when cold currents of air at comparatively low levels flow over warm, moist masses of air.

Physical Society, December 14, 1888.—Prof. von Helmholtz, President, in the chair.—Dr. Thiessen gave an account of experiments which he had carried out in order to measure the amount by which gravity varies at different heights. The method he employed was that of Jolly, but with the introduction of a modification, in order to eliminate the irregularities due to differences of temperature at the higher and lower stations. Scale-pans were attached to each arm of the balance—one close up to the beam, the other some distance below it—and the weight was interchanged between the pans, both at the upper and lower stations, thus eliminating the influence of differences of temperature and of any inequality of the balance. The upward force of the air had no influence on the results, notwithstanding the varying volumes of the weights used. The distance between the upper and lower scale-pans was 11·5 metres, and the weight used was 1 kilogramme. Twenty-four determinations were made, which gave as a result that the kilogramme, when in the lower pan, weighed 2·8 milligrammes more than when it was weighed in the upper pan. After making some corrections, and, among these, one necessitated by the fact that the weight in its lower position was 4 metres below the general surface of the earth, it was found that the weight of 1 kilogramme varies by 0·28 milligramme for each 1 metre of difference in altitude.—The President gave an account of a paper by Prof. Hertz, which he had yesterday communicated to the Berlin Academy. It contained a description of further experiments on electrodynamic waves, and their analogy with waves of light. Weak induction-discharges between small metallic cylinders with rounded ends were employed, and a similar apparatus for the detection of the electrodynamic waves. The action was not propagated more than 2 or 3 metres through space; when it fell on a metallic surface it was reflected, interference phenomena were observed, and from these the length of half a wave was found to be 30 centimetres. When a metallic parabolic mirror, 1 metre across its opening, was placed behind the apparatus used to produce the discharge, the action was propagated to a distance of 8 metres; and the action was greatly increased when a second concave mirror was placed behind the receiving apparatus. When a conductor was interposed, the action ceased, while non-conductors allowed the waves to pass. By interposing perforated metallic screens, it was found that the waves are propagated in straight lines; the waves passed through a dry wooden partition. Polarization of the waves could be determined in several ways. When the receiver was placed at right angles to the apparatus producing the waves, no action between them could be detected, the vertically-produced waves not being picked up by the horizontally-placed receiver. When the two pieces of apparatus were placed parallel to each other, and a wooden cube, with a number of insulated metallic wire rings wrapped round it, was placed in the path of the electrodynamic waves, it produced the same effect as does a tourmaline plate on polarized light. When the wires were vertical—that is to say, parallel to the exciting apparatus—the action was not propagated through the cube; but it was, on the other hand, when the wires were horizontal. When the receiver with its mirror was placed horizontally, so that it did not record any action as reaching it, and the wire arrangement, described above, was placed in the path of the waves, no change took place in the receiver when the wires on the cube were either vertical or horizontal, but the receiver was affected when the wires were placed at an angle of 45°. The laws of reflection of electrodynamic waves at metallic surfaces were found to be the same as those for the reflection of light at plane mirrors. Finally, Prof. Hertz has determined the refraction which the waves undergo in a prism made of pitch, and finds that the refractive index of this substance for electric waves is 1·68.—Dr. Ritter demonstrated by experiments the action of the ultra-violet rays of light on electric discharges in accordance with the experiments of Hertz, Wiedemann, and Eberts.

STOCKHOLM.

Royal Academy of Sciences, January 9.—On the researches and studies made at the zoological station of the Academy at Christineberg in Bohuslan, during the past year, by Prof. S. Lovén. He gave an account of papers by Dr. Aurivillius on the disguise amongst the Oxyrhynchous Crabs, by Dr. Virén on a Nereid Annelid (*Nereis fucata, forma inquitina*), by Herr Lönnberg on cestodes in marine fishes and birds.—Researches on the periodic system of the elements, by Dr. T. R. Rydberg.—Baron Nordenskiöld exhibited some uncommonly large crystals of magnetic iron from the Nordstjerne mine near Vestanfors, and gave an account of some remarkable Swedish localities with crystallized magnetite. He also showed four meteorites, for the collection of the State Museum, received from the British Museum. Amongst these were (1) a sample of a small, highly-interesting block of iron, which fell near Rowton, in Shropshire, August 20, 1876; (2) a fragment of a meteorite which fell in Hisen, in Japan.—On some transcendents, which appear at the repeated integration of rational functions, by Dr. A. Jonquière, of Bern.—On natural etching figures and other phenomena of solution on beryllium, from Muovinsk, by Herr W. Peterson.—Researches on minerals from Fiskernæs, in Greenland, by Herr N. V. Ussing.—Mineralogical notes, II., 3-4, by Herr G. Flink.—Anatomical studies on Echidna, by Miss C. Westling.—On the dimorphism of the *Rhizopoda reticulata*, by Dr. A. Goës.—The insect fauna of Greenland; I. Lepidoptera and Hymenoptera, by Prof. Chr. Aurivillius.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Memoirs and Memoranda in Anatomy, vol. i.: Cleland, Mackay, and Young (Williams and Norgate).—Molekularphysik, Erster Band: Dr. O. Lehmann (Williams and Norgate).—Thomas Jefferson and the University of Virginia: H. B. Adams (Washington).—Transactions of the Sanitary Institute of Great Britain, vol. ix. (Stanford).—Life Register (West, Newman).—Essai d'une Théorie du Soleil et des Étoiles Variables: A. Brester (Delft).—Industrial Education in the South: Rev. A. D. Mayo (Washington).—Kew Observatory, Richmond, Report for the year ending December 31, 1888 (Harrison).—L'Écoulement des Glaciers: Dr. A. A. Odin (Lausanne).—Arnold Toynbee: F. C. Montague (Baltimore).—Journal of Physiology, December (Cambridge).—Journal of Chemical Society, December (Supplementary Number) and January (Gurney and Jackson).—Himmel und Erde, 1 Jahrg. Heft 2, 3, 4 (Berlin, Pactel).—Annalen der Physik und Chemie, 1889, No. 1 (Leipzig, Barth).

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THURSDAY, JANUARY 24, 1889.

MR. GRANT ALLEN'S NOTIONS ABOUT FORCE AND ENERGY.

Force and Energy: a Theory of Dynamics. By Grant Allen. Pp. 161. (Longmans, Green, and Co., 1888.)

THERE exists a certain class of mind, allied perhaps to the ancient Greek Sophist variety, to which ignorance of a subject offers no sufficient obstacle to the composition of a treatise upon it. It may be rash to suggest that this type of mind is well developed in philosophers of the Spencerian school, though it would be possible to adduce some evidence in support of such a suggestion.

In the volume before us Mr. Grant Allen sets to work to reconstruct the fundamental science of dynamics, an edifice which, since the time of Galileo and Newton, has been standing on what has seemed a fairly secure and substantial basis, but which he seems to think it is now time to demolish in order to make room for a newly excogitated theory. The attempt is audacious, and the result—what might have been expected. The performance lends itself indeed to the most scathing criticism; blunders and misstatements abound on nearly every page, and the whole structure is simply an emanation of mental fog.

Thus much it is necessary to say in order to give an adequate idea of the nature of the book; but, having said as much as this, it is possible to speak quite otherwise of the friendly tone and apparently candid modesty in which the preface has been written. The preface, indeed, almost disarms a critic, or at any rate it causes him to use the blunt end of his lance; and were it not necessary to call attention to the erroneousness of such of the doctrines as are new, and expose their hollowness to a number of unlearned persons who are always eager to see some flaw found in a universally accepted theory the difficulties of which they have never mastered, and which they therefore vainly hope will turn out incorrect, one would gladly accept the apology and explanation of the preface and let the work sink into oblivion unnoticed.

Its pretentious form, however, renders criticism necessary; and indeed criticism is expected by the author, though, as he naïvely confesses, it will not undermine his own opinion of the truth and value of his work.

The book is in two parts: Part I. "Abstract or Analytic"; Part II. "Concrete or Synthetic." The second part consists of more or less popular illustrations of the doctrines inculcated in the first part: it need not therefore much concern us. The first part starts with chapters on "Power," "Force," "Energy"; later on it has three chapters called respectively, "The Persistence of Force," "The Conservation of Energy," "The Indestructibility of Power." These last headings are not very promising; and the performance does not belie the promise.

Chapter II. leads off with the following definition:—

"A Force is a Power which initiates or accelerates aggregative-motion, while it resists or retards separative motion, in two or more particles of ponderable matter (and possibly also of the ethereal medium)."

In other words, the author agrees to limit the term "force" to that which is commonly known as attraction. Very good. Now take Chapter III.:—

"An Energy is a Power which resists or retards aggregative motion, while it initiates or accelerates separative motion, in two or more particles of ponderable matter, or of the ethereal medium."

In other words, he is going to denote that which is commonly known as repulsion by the name "energy." He by no means always adheres to the limitations imposed by this definition, and frequently he means by the term energy the same as is ordinarily understood by the term, though he does so most happily in cases where the result can be looked upon as a separation of some bodies.

Thus, for instance, the upward motion of a cannon-ball he would style an energy, because it separates the ball from the earth; the strain in a bow he would call an energy, because it can separate the arrow from the bow; heat he would call energy, because it usually expands things; but the *horizontal* motion of a cannon-ball or a railway train he will have a difficulty in calling an energy, and, in fact, is unable to do so without a flagrant mistake of principle in the case of the ball, and some manifest special pleading in the case of the train. Still more unable is he properly to apply the term energy to the *downward* motion of a falling body. We shall see what he makes of this obvious difficulty shortly. Nevertheless, this is the main doctrine in the book—viz. that anything tending towards aggregation is a force, while anything tending to disgregation is an energy.

Passing, then, to the next chapter, "The Species of Force," he subdivides force into attraction between large masses, or gravitation—between molecules, or cohesion—between atoms, or chemical affinity—and, lastly, between electric charges. It may as well be said, once for all, that throughout the book there are many lame and hesitating references to electricity and ether, which are so vague as to be quite harmless, and which it will be the most charitable plan to simply ignore. We had better ignore most of the chemical and molecular statements also, for the same reason, and attend only to those which deal with ordinary lumps of matter. But even here it will not do to criticize the language too closely, or one would have enough to do. This, for instance, is one of the first sentences about gravitation: "When an aërolite comes within the circle of the earth's attraction, it is Gravitation which makes them leap towards one another." "The circle of the earth's attraction" and the sudden "leap" of the aërolite when it comes within this circle, are phrases which scarcely express quite accurately the facts of the case!

The next sentence throws a flood of light upon the state of the writer's mind when he formed his conception of the difference between force and energy, and explains also, I venture to surmise, why his doctrine found so blithe an acceptance with Mr. Edward Clodd, as Mr. Clodd's own book and the preface to this one inform us. It is the orbital energy of the moon which counteracts the aggregative power of gravity. "If the moon were to lose its orbital Energy, Gravitation would pull it to the earth."

This sentence is not indeed untrue, but it is significant as showing that it is the old puzzle of centrifugal force or

of radial acceleration that is at the bottom of this whole "new theory." The puzzle was solved completely long ago, in the clearest possible manner, and the "Principia" is the witness to it; but it is still felt to be a difficulty by beginners, and I suppose there is no offence in applying this harmless epithet to both Mr. Grant Allen and Mr. Clodd, so far as the truths of dynamics and physics are concerned.

"Energy" and "Force" are always opposed to one another, and the meaning of the author is well illustrated by the following quotation:—

"Again, when two masses are in a state of aggregation, the Force of gravitation resists any attempt to sever them. If a cannon-ball lies upon the ground, it cannot be raised without an expenditure of Energy, and the amount of the Energy required to lift it to a given height is the measure of the resistance offered by Gravitation."

That is to say, the energy required to lift a body to a given height is a measure of the force of gravitation. This reminds one of some of the sentences in Tyndall's "Heat."

The next chapter, "The Species of Energy," classifies energy as molar, molecular, atomic, and electrical, just as in the case of force. "Of Molar Energies employed in resistance to aggregation, the most familiar instance is that of orbital movement"—moon and earth, &c.

Now we come to the *crux* of how horizontal motion can be an "energy" in the sense defined by Mr. Allen.

"On a smaller scale, the Energy of a bird in flying, or of a cannon-ball fired horizontally, is largely employed in counteracting gravitation"!!

May I inform the author of an elementary fact? A cannon-ball fired horizontally falls just as quickly, and reaches the ground (if flat) in precisely the same time, as if it were merely dropped; its energy of motion has no power of counteracting gravitation. The same is really true of the moon: it falls towards the earth just as much in each second as if it had no orbital velocity; in no case does motion influence the effect produced by a given force. But this is just the fact which, if he had been able to recognize it, would have saved him the trouble of writing this treatise.

I said I would charitably omit reference to the molecular, chemical, and electrical statements, but I cannot resist one quotation from the next page: "Large masses of water before freezing part with their Energy in the visible form of heated mist."

Chapter VII. is headed "The Kinds of Kinesis," by which is meant apparently the varieties of motion. Motion is subdivided, not into rotation and translation, but thus:—

"Motion has three Kinds; . . . it may be separative, or it may be aggregative, or it may be continuous and neutral. Each species of Kinetic Energy has a form of each kind."

The author now finds it necessary to grapple with the difficulty which we guessed he would sooner or later feel, viz. how it is possible to bring falling motion, or motion of bodies towards one another, under his category of "Energies or separative Powers." A body thrown up, and a body thrown horizontally, he has already tackled by simply committing some convenient errors of fact. He surmises that such bodies fall less than they would if dropped, and thus that their energy of motion counteracts gravitation; but the case of a body thrown down is not to be thus managed, so he proceeds to get over the difficulty in three ways. First, by raising a cloud of words; second, by asserting that when a body strikes the earth, although its potential energy of separation has disappeared, yet the heat of its collision separates atoms just as much, and results in the same ultimate amount of separation again (a statement which in no sense can be considered true), so that the motion which intervened between the start and stop of the falling body "we are justified in regarding as essentially a transitory form of separative Power."

"Throughout we see that aggregative Energy" (*i.e.* energy apparently aggregative) "is merely Potential Energy in course of transformation to another form. While the really aggregative Power of Force is causing these bodies to combine, the Energy of their motion represents for a while their original separateness, and is finally transformed into a similar separateness between other bodies."

So, while motion *from* the earth is true kinetic energy, motion *towards* it is a transitory form of potential energy, and represents for a while the original separateness of the bodies!

When a doctrine requires a statement like this to bolster it up, it is wise to take the need as a sure indication that we are somewhere off the track, and had better get back to the turning whence the path which has led us into such a jungle diverged. It is a pity the author did not take the hint thus clearly vouchsafed to him. His difficulties about understanding normal acceleration and the generality of Newton's second law were natural and excusable, though hardly the subject to write a book about; but after encountering and being worsted by this last thicket, it was very unwise to go on plunging madly forward, and exhibit his scratches as signs of victory.

But he has not yet made his last struggle into still more hopeless entanglement. Here is his third attempt at extrication:—

"We see that the Energy of a falling body does not consist in its mere downward movement, but rather in that accelerating motion which is capable of being transformed into heat when the masses aggregate."

If this statement means anything, it means that the kinetic energy of a down-moving body consists, not in its velocity, as is the case with an upward-moving body, but in its acceleration, and that it is this acceleration which ultimately gets turned into heat!

"So the Energy of Kinesis is seen to be a mere transferential mode from one kind of separation to another." "Motion is the redistribution of Separations."

One may at least acknowledge the ingenuity as well as the gallantry with which the author endeavours to get clear of his impenetrable jungle.

In passing, here is a curious definition of friction, given in an explanatory note.

"From the point of view of the Force involved, friction means the cohesion which must be overcome; but from the point of view of the Energy employed, friction means the separative power of heat which overcomes."

Next we come to the extraordinary but fortunately short chapter entitled "The Persistence of Force," which

"must be carefully distinguished from the opposite principle of the Conservation of Energy."

From the examples given it appears that the author means by "the persistence of force" the universal prevalence and unalterable character of gravitation. This, if you please, is a principle "opposite" to the "conservation of energy."

It may be asked how on earth the author manages to make out any doctrine of conservation for what he is pleased to style "Energy." It may be asked, but scarcely answered. First, there is a momentous difference of language to be attended to, a difference of which we are told the concrete and practical results are enormous. "While Forces *persist*, Energies are *conserved*." This is a most satisfactory beginning, and makes one feel quite smooth and comfortable. But unfortunately the author is not original in the rest of this chapter, and his illustrations of conservation of energy do not flow from his definition, but are quite common-place. I cannot resist one little extract from this chapter wherein the property of "inertia" has an altogether new light thrown upon it, a light even more brilliant than that just thrown upon "friction."

> "Two molecules of water vapour are prevented from aggregating under the relatively feeble attraction of cohesion at a distance, by their inertia—that is, by the relatively strong cohesion of surrounding or intervening matters (just as a mass on a table is prevented from aggregating with the earth by the cohesion of its boards). Two atoms having affinities for one another are similarly prevented from aggregating by inertia. . . . So also two electrical units are prevented from aggregating in the Leyden jar by the electrical neutrality of the glass partition."

Chapter X., "The Indestructibility of Power," asserts that

> "the total amount of Power, aggregative or separative, in the Universe is constant, and no Power can ever disappear or be destroyed. This sums up the two preceding generalizations of the Persistence of Force and the Conservation of Energy in one still wider generalization."

Chapter X. is very short.

The chapter on "Liberating Energies" is a dissertation on pulling a trigger, and quite mistakenly supposes that some expenditure of energy is essential to the performance of this act.

> "A ball suspended by a thread is released by the separative Energy of a knife or scissors."

The separative Energy of a knife is a good phrase.

> "The stronger Force necessarily outweighs the weaker, and as Forces cannot increase or decrease in intensity, the only manner, &c."

Forces cannot increase or decrease in intensity? No, certainly not; this is proved by the existence of the phrase "persistence of force." Well, this is logical; at any rate, after the bookish manner of argument, and that is some comfort.

> "Electrical Liberating Energies are those which release Electrical Units from the interference of a Force antagonistic to Electrical Affinity."
>
> "They are such as close the circuit of a battery, or bring a discharging tongs to a Leyden jar."
>
> "The usual vagueness of electrical science prevents any definite treatment of these phenomena."

Are we, then, to conclude that the author, in all this treatise, has hit on no germ of truth—nothing but what was well known before, or what is erroneous? I fear that, with the possible exception of the idea of classifying energies by reference to the sizes of the bodies concerned, this must be our conclusion. He has regarded the universe from the point of view of action at a distance, and has been struck with one or two salient features: (1) the universality of gravitation at great distances, and of cohesion at small; (2) the existence of what has been styled "repulsive motion"—that is, a motion which simulates the effects of repulsion, as when two particles or two molecules, rushing together, swing each other round and separate again; or when a heated gas expands. One knows that, in old text-books, heat was often spoken of as a repulsive force. And a sufficient velocity imparted to a satellite *does* keep it clear of the earth.

Struck with these facts, he has proceeded to take gravitative separation as the typical and fundamental form of energy; motion being a form of energy only because it tends to separation (in some cases, at any rate), so that he defines motion, in the last chapter of Part I., as "the redistribution of separations," while energy he defines as "a Power which separates." On the other hand, he has taken gravitation and cohesion as his typical forms of force; and, because these tend to pull bodies together, he has defined force as "a Power which aggregates," and has proceeded to write a treatise on the subject, showing how force and energy are opposed to one another.

The thing which strikes one most forcibly about the physics of these paper philosophers is the extraordinary contempt which, if they are consistent, they must or ought to feel for men of science. If Newton, and Lagrange, and Gauss, and Thomson, to say nothing of smaller men, have muddled away their brains in concocting a scheme of dynamics wherein the very definitions are all wrong; if they have arrived at a law of conservation of energy without knowing what the word energy means, or how to define it; if they have to be set right by an amateur who has devoted a few weeks or months to the subject, and acquired a rude smattering of some of its terms,—what intolerable fools they must all be!

But this does seem the attitude of many literary men, and that must be one reason why they dislike and despise science. If such a view were just or true, dislike and contempt would be the only reasonable attitude.

A scientific man may often feel harassed by being unable to express in literary form what he has to say; but, though this is an evil, it is surely a lesser evil than to have the knack of writing and no matter to write. It is as when the Sophists proceeded to teach rhetoric, heedless of whether either their pupils or themselves possessed any real knowledge about which to be eloquent.

Mr. Grant Allen has apologized for his Mistake in the preface, and one has no quarrel with him. One might, if it were worth while, have a quarrel with a certain class of literary men for the shallow and flippant way in which they occasionally refer to Science; but it is not worth while.

The disciple is not above his master in this respect.

Here is one quotation of many which might have been chosen from Mr. Herbert Spencer:—

"Newton described himself as unable to think that the attraction of one body for another at a distance could be exerted in the absence of an intervening medium. But now let us ask how much the forwarder we are if an intervening medium be assumed. This ether, whose undulations, according to the received hypothesis, constitute heat and light, and which is the vehicle of gravitation,—how is it constituted? We must regard it in the way that physicists do regard it, as composed of atoms which attract and repel each other—infinitesimal, it may be, in comparison with those of ordinary matter, but still atoms. And remembering that this ether is imponderable, we are obliged to conclude that the ratio between the interspaces of these atoms and the atoms themselves is incommensurably greater than the like ratio in ponderable matter; else the densities could not be incommensurable. Instead, then, of a direct action by the sun upon the earth, without anything intervening, we have to conceive the sun's action propagated through a medium whose molecules are probably as small relatively to their interspaces as are the sun and earth compared with the space between them; we have to conceive these infinitesimal molecules acting on each other through absolutely vacant spaces which are immense in comparison with their own dimensions. How is this conception easier than the other? We still have mentally to represent a body as acting where it is not, and in the absence of anything by which its action may be transferred; and what matters it whether this takes place on a large or a small scale?" ("First Principles," chap. iii. § 18).

Omitting any reference to the absurd reasoning about "incommensurable" densities in this quotation, and about the ether being "imponderable," as if Mr. Herbert Spencer or anyone else knows anything whatever on the subject, I wish to call attention to the words regarding the structure of the ether "in the way physicists do regard it"! If they do, if they are unable to see that action at a distance across a small space is just as inexplicable as action at a distance across a large one, and if the ether they imagine is not thought of as in some sense or other a *continuum* for this very reason; if, in fact, they are unable to appreciate, in all the years they have been thinking on the subject, what is obvious on the face of it to someone who steps in, so to speak, for the first time, —what singularly incompetent persons they must be!

That seems to be the real upshot and natural meaning of many of these criticisms of science from the outsider's point of view. O. J. LODGE.

ROCKS AND SOILS.

Rocks and Soils: their Origin, Composition, and Characteristics. By Horace Edward Stockbridge, Ph.D., Professor of Chemistry and Geology in the Imperial College of Agriculture, Sapporo, Japan; Chemist to the Hokkaido Cho. (New York: John Wiley and Sons. London: Trübner. 1888.)

CHEMIST to the Hokkaido Cho! It is not the least striking feature of our time that there should be an Imperial College of Agriculture at Sapporo whose Professors publish researches in New York and London. This is not exactly a novel experience, for events crowd upon us thick and fast in these days; but those of us who can look back forty years must be struck when confronted with the Chemist of the Hokkaido Cho. Dr. Stockbridge is not, be it understood, the alchemist to an Eastern potentate, nor yet one of the astrologers, Chaldæans, or soothsayers of a modern Belshazzar, but an agricultural chemist and geologist discoursing upon rocks and soils, nitrates and microbes, and suggesting processes by which atmospheric nitrogen is fixed in the soil by the action of living organisms. The great Mikado, "virtuous man," has, we know, transplanted full-grown and fully-equipped knowledge from the West to his remote dominions; and so successfully, that it has rooted, and now is become an article for exportation—as witness the volume before us. To some of our readers it may appear unnecessary to dilate upon a fact which springs naturally out of the most recent developments of civilization. We need not now despair of openings for aspiring young chemists under the protection and pay of the King of Dahomey or of Ashantee, or of an Imperial Institute at Khartoum or some other part of the Dark Continent; and truly the missionaries of science are in a fair way to rival those of religion in their ubiquity.

The volume before us is of attractive appearance. It is, however, hard upon the reader who takes it up in order to learn something about rocks and soils to be carried through the entire history of the planet on which his lot is cast. Deeply interesting as are the cosmic questions bearing upon the original nebulous mist, "in glowing gaseous condition," they scarcely affect even scientific agriculture. Besides, it is open to doubt whether an agricultural chemist and geologist is within his province in explaining the differences between white stars, red stars, and habitable planets which have gone through phases thus indicated. Such information belongs to the domain of the astronomer and the physicist, and the agricultural study of rocks and soils should be taken up at a later date of the earth's history. It is not our object to criticize Dr. Stockbridge's book severely, but it appears to us that if he had cut out 100 pages at the beginning, and added 100 pages at the end in harmony with his concluding sections, his work would have been more useful.

The two features of this book which seem to us the most important are, first, Dr. Stockbridge's views as to the "fixation of atmospheric nitrogen independent of ammoniacal condensation and of nitrification." The compounds thus formed in the soil are, we are told, complex insoluble amides resembling those existing in living organisms, and must have resulted through the vital activity of the micro-organisms present in the soil. If soils have the power of fixing atmospheric nitrogen through the action of living organisms, they possess a means of recruiting fertility independent of plant action, and of so fundamental a nature that, supposing such action to take place, the question of the source of nitrogen and the supply of nitrogen in soils would be set at rest. Another novel view is that propounded with reference to dew-formation. Here, we have a subject which is not very clearly related to that of rocks and soils. So far as the soil is a vehicle of plant nutrition, its conditions as related to moisture are of course important, and it is in this connection that the theory of dew as propounded by Dr. Stockbridge finds a place in his work. It is not necessary here to explain Dr. Wells's explanation of the fall of dew.

It is sufficient to state that it is unsatisfactory to our author, who holds that dew on the leaves of plants is (we presume he means occasionally and not universally) derived from the plant itself rather than from condensation from the atmosphere. Dew on growing vegetables is produced by the condensation of the transpired moisture from the plant on its own leaves. This explanation is proved by direct experiment, and we are not disposed to deny its truth. It is probable, and, in fact, more than probable, that plants which are giving off large quantities of water into a cold or overcharged atmosphere should have a portion of their own moisture thrown back upon them. This fact is asserted in Marshall Ward's translation of Sachs's "Physiology of Plants," when he says, "Much of the water we find early in the morning on the margins of the leaves of many field and garden plants in the form of large drops, and which are generally taken for drops of dew, is really water excreted from the plants themselves." That the air is really the cooling medium by which the moisture rising from the warm soil or the growing plant is condensed is no doubt often true. The mist which stretches over the meadows at sundown is moisture condensed in the cool air, and thus becomes precipitated upon vegetation, and not only on vegetation, but upon everything else. Where we think Dr. Stockbridge has overstated his case is when he writes in italics, "*The declaration is here made that dew is the condensed exhalation of the plant.*" The statement is too general, and the assertion has too much of the character of a supposed new discovery on the part of the author. The real facts of the case are, that dew is produced in some cases from condensed exhalations from the plant, or from condensed moisture rising from the soil; but also from the precipitation of moisture from higher sections of the air during the night; especially when the sky is clear. The collection of water in the form of hoar frost upon leafless trees or lawns must be derived from the condensation of atmospheric moisture upon the tree, or upon the grass, cooled by radiation; and we have no doubt that grass radiates heat on a moonlight night more rapidly than does bare ground. Dr. Stockbridge lays too much stress on the fact that the earth is warmer than the air when dew is falling. This he asks us to believe is fatal to the theory that the earth condenses or can condense atmospheric moisture. The radiating power of the earth is very great, and exceeds that of the air, which, in fact, absorbs and retains much heat which otherwise would be immediately lost in space. Dr. Stockbridge argues that the surface of the earth is invariably warmer than the air at the dew point, but this is not likely to be the case. Even the temperature of grass land is affirmed to be always warmer than the air, and hence it is contended that in no instance can the earth or vegetation be the condenser. We are disposed to think that observation will throw more light on this point than such experiments as are quoted or were made by Dr. Stockbridge.

It is probable that dew may be precipitated at times by a colder air on a warmer surface, and at other times by a cold soil or cold expanse of leaf from a warm atmosphere. Whether the leaf of a grass or the air above it acts the part of "the cold pitcher" is not always to be predicated. but in either case dew would be the result. We may point out that, while Dr. Stockbridge is disposed to assert that the soil is always warmer than the air, other authorities are of opinion that the surface, especially of grass, is colder by many degrees than the air. A thermometer laid upon grass would, we believe, recede further and record a lower minimum than one suspended 2 feet above the grass. The freezing of dew on grass during summer nights, which is always an unpleasant sight to gardeners and farmers, appears to be accounted for by radiation of heat from the grass surface, while the substance of the grass cuts off radiated heat from below. The absence of dew under shade also is apparently due to radiation from the earth being checked, and the cooling process of the surface of the earth or its vegetable covering being prevented.

Dr. Stockbridge's book suffers from careless reading of the proofs. This work, the author tells us, he was compelled to depute to others. In one place (p. 183) the word "soil" is evidently used instead of air, thereby reversing the author's obvious meaning, and the word "not" is interpolated, which further confuses the sentence hopelessly. Names of authors quoted are mis-spelt in several cases. The book, especially in the earlier pages, is somewhat bombastic and provincial in its style, and, as we have before stated, many of the earlier pages might have been omitted with advantage. The tone of the writing becomes more modest, precise, and student-like as the author approaches the topics which we are informed upon the title-page he professes.

JOHN WRIGHTSON.

OUR BOOK SHELF.

The Kingdom of Georgia. By Oliver Wardrop. (London: Sampson Low, 1888.)

THE author of this work sees no reason why Georgia should not become as popular a resort as Norway or Switzerland. Perhaps he takes rather too sanguine a view of the energy even of the British tourist, but everyone who reads his book will certainly wish to have a chance of visiting the country he describes. A brighter or pleasanter book of the kind we have not seen for many a day. The style is fresh and sparkling, and Mr. Wardrop has the secret of awakening and maintaining the interest of his readers without any attempt at picturesque fine-writing. He conveys a remarkably vivid impression of the splendid natural scenery of Transcaucasia; but it is in describing the Georgian people that he displays most effectively his powers as an observer. He has the warmest appreciation of the intelligence, bravery, and generosity of the Georgians, and, in the course of his narrative, the extent to which these and other qualities affect their social life is shown with much force and animation. There are valuable chapters on the history, the political condition, and the language and literature of Georgia; and an excellent bibliography of works relating to the country is given as an appendix. The book is also supplied with good maps and illustrations.

The British Journal Photographic Almanac for 1889 Edited by J. Traill Taylor. (London: Henry Greenwood and Co., 1889.)

THIS work, as a compendium of photographic art science, could scarcely be more complete. It contains about 300 closely-printed pages, consisting of articles written by men who are eminent in connection with photography in its various and ever-increasing branches. Great ad-

vance has been made in this year's volume, both as regards size and the number of articles included; the pages of the calendar which were formerly devoted to the dates of meetings of Societies have here been left blank for the sake of persons desiring to make notes or memoranda.

Valuable hints on all topics are given both to amateurs and professionals, no single department of the work, as far as we can find, having been neglected.

A brief summary of the year's work is given by e editor, touching upon the gradual merging of the brown and purple tones into those of darker and more engraving-like type, the advancement made in flash-light photography, and the new method of platinum-printing. The summary concludes with an obituary of those who have passed away since the last issue.

Next follow series of articles, commencing with one on "Iron Printing," by the editor, and continuing with those contributed by Abney, Burton, Perry, Piazzi-Smyth, and many others.

Twenty pages are devoted to an epitome of progress during the year 1888, and then are added a list of useful receipts, standard formulæ, reference tables, &c.

The Photographer's Diary and Desk-book for 1889. Compiled by the Editor of the *Camera*. (London: Published at the Office of the *Camera*, 1889.)

OF the various diaries brought out for the present year, that issued by the proprietors of the *Camera* will be sure to give great satisfaction to photographers, both amateur and professional. This differs from other photographic diaries in two respects: in the first place, it is much larger, there being plenty of room for notes on experiences of various kinds, results of manipulating, developing difficulties, and many other details well worth recording, which are often so useful and valuable for reference. In the second place, there is a great amount of useful information condensed in the first fifty pages. Besides various tables and processes of developing, printing, &c., information similar to that included in almanacs and other diaries is inserted; the tables and standard formulæ relating to photography being printed in larger type, to enable the worker, when in the dark room, to refer to them. This diary is a very complete and useful publication, and, as a book of reference, is most handy.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Climate of Siberia in the Mammoth Age.

A SHORT time ago I was discussing with my friend Mr. Henry Seebohm the various problems connected with the distribution and migration of birds in Siberia, about which he has collected so many facts. One fact which he mentioned to me seemed to have a much wider interest than a merely ornithological one, and to illustrate from an unexpected quarter a conclusion which you have allowed me to urge in your columns, and which forms a notable postulate in my recent work on "The Mammoth and the Flood." I mean in reference to the climate of Siberia during the Mammoth age. The views I have advanced on this subject are not my own. I have merely followed in the footsteps of almost every recent Continental authority, especially the authorities with the greatest claims to attention—namely, the Russian naturalists who have visited Northern Siberia. They maintain—and I think the position is unassailable—that during the Mammoth period that district which is now a bare *tundra*, on which neither in summer nor winter could herds of pachyderms find food or shelter, was marked by a temperate climate, and was probably occupied by forests to the very borders of the Arctic Ocean.

This view, which is supported by so many facts, was finally established when it was shown by Schmidt and others that rooted trunks of trees are found in the beds containing Mammoth remains far north of the present range of trees, and that southern forms of fresh-water mollusks, such as the *Cyrena fluminalis*, are also found preserved in the same beds in Siberia far to the north of any place where they will now live. These facts are consistent only with the former existence of a temperate climate in Siberia.

It is interesting to meet with support for this position from the present avifauna of the Palæarctic region. Mr. Seebohm, who has an unrivalled collection of skins, illustrating the ornithology of this region from Britain to Japan, assures me (and, in fact, he showed me the evidence) that certain birds—notably the jay, the nuthatch, the marsh-tit, coal-tit, and long-tailed tit, the great and little spotted woodpecker of England and Japan, and in one case of Northern China—are virtually undistinguishable. Similarly, the hazel grouse of Japan resembles that of the Pyrenees, and the nutcracker of Japan and China is like that of Western Europe. While this is so, the forms of these same birds found in the intervening district of Siberia differ very materially, and have, I believe, in almost every case, been treated as specifically distinct. This is assuredly a very interesting fact. Both Britain and Northern Japan are in the same zoological province—namely, the Palæarctic region, over which there is a singular constancy of types and forms, and yet we find that in certain birds the forms at either extremity of the province are closely allied, while the intermediate form differs. This is at one with the fact that the climate of the two extremities is very similar, and that of the intervening district is very much more severe in the winter. We can hardly doubt that the general adherence to a normal type which marks the fauna and flora of the Palæarctic region (and which was even more marked, and amounted, so far as we know, to identity, in the Mammoth period) is due to the fact that formerly, and in every probability in the Mammoth period, an equality of conditions prevailed throughout. This equality has been maintained at the extremities of the region, with the result of maintaining the old forms and types unaltered; while it has changed and grown more severe in the intervening region, with the corresponding result of altering the types there. The conservatism of forms at either end of the province proves unmistakably a conservatism of conditions. This is assuredly a very interesting independent proof, if proof be now needed, that the climate of Siberia was once much more temperate throughout, and like that of Britain and Japan, and this doubtless in the Mammoth age.

I may add that it seems to me very nearly certain that this change of climate in Siberia was the cause of the conversion of what were once sedentary birds there into birds that migrate to South Africa and elsewhere—a migration which has been very well illustrated by Mr. Seebohm. That the date of the commencement of this migratory tendency is not very remote in time is shown by the fact that the birds have not been more differentiated, notwithstanding the very various conditions prevailing in their several winter-quarters. I believe myself that in the Mammoth period, when the climate of Siberia was temperate, there was no need for these tremendous migrations, which were, no doubt, originally induced by the necessity for finding food in winter; but that most, if not all, of these migratory birds were then either stationary in Siberia, or were only local migrants, like so many of our own birds are now.

Mr. Seebohm, in his recent work on the Charadriidæ, has invoked the Glacial epoch to account for the facts presented by that singularly distributed genus. I know of no Glacial epoch in Siberia before the present. The last epoch there, as we can test and prove by the presence of the undecayed carcasses in the frozen ground, was the period when the Mammoth lived. It was when that period closed (and as I claim to have proved closed very rapidly) that the present Arctic conditions of the Siberian climate were introduced, and I would urge it was from this date that the *present* laws controlling the migration of Siberian birds arose.

This seems an inference of some importance; and when the ornithological history of the eastern half of the Palæarctic region is written in detail, it will very probably be shown that the

peculiar sub-specific types found there are many of them *new* forms, which have arisen since the Mammoth age, having been altered from the old ones, which live on under old conditions in the West of Europe and the Japanese Archipelago.

I should like to specify one particular bird in regard to which this notion seems to point a special moral. This is the British red grouse, the only peculiar bird of these islands. Its nearest ally on the Continent, and a very near ally, is the willow grouse. I have little doubt that the willow grouse of the Continent is an altered form, and that our red grouse is the parent, since the evidence we have, and it is not slight, goes to show that England has preserved better than Scandinavia the climatic conditions of the Mammoth period. The white marks and other characteristics of the willow grouse are evidences of the effect of colder winter-quarters, as they are in the case of the ptarmigan.

Lastly, I cannot avoid emphasizing once more the conclusion which I have pressed in my book that this milder climate in Siberia during the Mammoth age entirely does away with the necessity for invoking quite transcendental seasonal migrations for the fauna there which have been postulated by Prof. Dawkins and others. That the Mammoth, with its immature young, should be able to pass to and fro between the south of Siberia and the New Siberian Islands and Kamchatka, between summer and winter, has always seemed to me incredible. If they could compass the journey they would either find a temperate conditions of things, which is alone consistent with their finding food, when there would be no occasion for them to migrate, or they would find the conditions which prevail now, when no pachyderms could find food *even in summer*, since they are physically incapable of browsing the short herbage of the *tundra*. Nor could the trees and the southern mollusks, like the *Cyrena*, migrate, even if the young Mammoths could. This theory of migrations finds no support, so far as I know, among those who have studied the problem on the ground; and it is put entirely out of court when we realize that, Siberia having had a temperate climate, there was no necessity to migrate.

A similar argument applies to the theory invoking the transport of the Mammoth carcasses by means of the Siberian rivers, which has always seemed to me untenable when the conditions are faced. I would mention that in Baron Toll's recent journey to the New Siberian Islands, situated a long way north of the Siberian coast-line, and entirely out of the reach of any possible river portage, he not only found remains of a carcass of a Mammoth preserved in the flesh, but found them in a bed situated to the north of a ridge. This fact may be put beside those already mentioned by Wrangell and others long ago, that the carcasses and skeletons and *cachés* are found chiefly on the hillocks and higher ground of the *tundra*, out of the reach of river-floods altogether, and found most frequently near the small rivulets and feeders of the greater streams, which could not float them, and found also near those flowing south. This theory of portage and that of seasonal migrations have been nursed and maintained in this country in spite of evidence of every kind, because they are supposed in some way to buttress the theory of uniformity, as taught by Lyell and Ramsay. An appeal to them and to similar complicated physical causes becomes not merely unwarrantable, but unscientific and illogical, when we realize that from one end of Siberia to the other the climate was sufficiently temperate when the Mammoth lived there to enable trees to grow and vegetable food to be found everywhere, and the physical surroundings of the country were probably such as may be measured by those still prevailing in Britain or Japan.

HENRY H. HOWORTH.

Bentcliffe, Eccles, January 10.

The Crystallization of Lake Ice.

ON returning recently from North Wales, I was very pleased to meet with a description, by Mr. James C. McConnel (NATURE, December 27, p. 203), of the elaborate experiments performed by himself and Mr. Dudley A. Kidd on glacier and lake ice at St. Moritz. An experiment I had made on the ice of Llyn Creigenen, a small lake to the north-west of Tyrau Mawr, seems to me to be, in some measure, confirmatory of the results obtained in connection with the crystallization of lake ice.

By sharply striking the ice, which was only about half an inch thick, with the rounded end of a stick, fractures were produced, which invariably adopted the form of a six-rayed star-like figure. The beautiful regularity of these figures, in regard to the number, position, and perfect straightness of their rays, at once reminded me of the well-known percussion- and pressure-figures produced in mica plates by Reusch and Bauer. Mr. Grenville Cole, who was with me at the time, repeated the experiment, and obtained precisely similar results. We found that over a certain area a large number of these figures could be produced, in each of which there were corresponding parallel rays—that is to say, every percussion-figure was similarly disposed with regard to a fixed line. Outside this area, the figures produced, although preserving the characters of those first made, exhibited a change in the direction of the rays. In this way we could determine the boundaries of a number of adjacent areas, separated from one another by definite lines of demarcation. These areas averaged about two feet across.

We were unable to conceive of any conditions of stress which would, in a homogeneous solid plate, give rise to such phenomena—fractures of such beautiful regularity, and so constant in character. Consequently, we thought of crystallization; but this would necessitate the recognition of ice-crystals of very large dimensions—a conclusion obviously at variance with the existing notions concerning the crystalline characters of ice. We thought, however, that the matter might be worthy of investigation, and, on returning to town, were pleased to find that large crystals of lake ice had been found at St. Moritz by Messrs. McConnel and Kidd, which, however, did not attain the size of those we noticed on Llyn Creigenen. But the fact that on the St. Moritz Lake the ice attained a thickness of over one foot shows that the temperature must have been lower, and the conditions more rigorous, than in North Wales at the time of our visit.

If it is possible at all to obtain large crystals of ice, I should say the conditions for such on Llyn Creigenen were of the most favourable character. For three days previously, the temperature varied very little from zero C., and, from the slight wind that prevailed at the time, the lake was well sheltered by the hills which rise abruptly around; indeed, the lake was unusually free from disturbing influences of any kind.

If these sheets of ice were gigantic crystals, it is in the highest degree probable that the surface of the ice coincided with the basal plane, as was the case with the columnar crystals observed by Prof. Heim in the lake ice of the Swiss lowlands. For want of a polariscope we were prevented from investigating the matter further in the field; but in some small well-sheltered pools on Tyrau Mawr we found it easy to produce the same phenomena of percussion-figures, whilst the ice in the marshy places amongst the grass gave fractures of a most irregular kind. We found, in several places, skeleton-crystals like ornamented equilateral triangles, measuring some inches across.

THOMAS H. HOLLAND.

Normal School of Science, South Kensington.

Use of the *Remora* in Fishing.

WITH reference to Mr. A. C. Haddon's interesting account of the use of the *Remora* or sucker-fish by the natives of Torres Straits in fishing for turtles (NATURE, January 17, p. 285), I may call attention to the paper on this subject read by our corresponding member, Mr. Frederick Holmwood, C.B., late H.B.M. Consul at Zanzibar, before the Zoological Society of London, on June 17, 1881 (see P.Z.S., 1881, p. 411), which Mr. Haddon does not seem to be acquainted with. Mr. Holmwood has fully described the mode of the use of the *Remora* by the native fishermen of Zanzibar in catching turtles and fishes. It is curious to find a somewhat similar method of employing the *Remora* practised by the islanders of Torres Straits.

P. L. SCLATER.

3 Hanover Square, London, W., January 19.

A Remarkable Rime.

UNDER this heading a letter appears from Lutterworth (p. 270), but no mention is made of the colour of the water obtained on melting some of the rime collected from the trees. In the neighbourhood, far removed from any large town, the rime crystals, on melting, gave water tasting very sooty, and looking as though the liquid had been used to wash Indian ink brushes in, it being quite black with sooty particles.

M. H. MAW.

Barrow-on-Humber, Hull, January 22.

HUMAN VARIETY.[1]

IT would have been a pleasure to me in this address, given at the conclusion of my office as your President, to have cast a retrospect over the proceedings of our Institute during the four years that I have had the honour to hold it. But the subjects that have come before us are so varied that it seemed difficult to briefly summarize them in a manner that should not be too desultory.

On the whole, I thought it might be more useful if I kept to a branch of anthropometry with which many inquiries have made me familiar, and took the opportunity of urging certain views that seem to be worthy the attention of anthropologists.

Before entering upon these more solid topics, let me mention that the laboratory of which I spoke in my last address has been in work during the past year, and that about 1200 persons have been already measured at it in many ways, some more than once. I lay on the table a duplicate of one of the forms of application to be measured, and of one of the filled-up schedules. It will be observed that I now have the impressions made in printers' ink of the two thumbs of each person who is measured, being desirous of investigating at leisure the possibilities of employing that method for the purpose of identification, not forgetting the success that attended Sir W. Herschel's use of it in India, but conscious at the same time of practical difficulties. There is no doubt that the thumb or finger marks vary so much that a glance suffices to distinguish half a dozen varieties, while a minute investigation shows an extraordinary difference in small, though perfectly distinct, peculiarities. Neither is there any room for doubt that these peculiarities are persistent throughout life; nor, again, that so satisfactory a method of raising a very strong presumption of identity would be valuable in many cases. It will suffice to quote the following. A newspaper was lately sent me from the distant British settlement of North Borneo, where, owing to the wide and rapid spread of information nowadays, attention had been drawn to an account of a lecture I gave on one of the Friday evenings last spring, at the Royal Institution. It was on "Personal Description and Identification," and a writer in the *British North Borneo Herald* commented upon the remarks there made on finger imprints. He spoke of the great difficulty of identifying coolies either by their photographs or measurements, and that the question how this could best be done would probably become important in the early future of that country. I also am assured that the difficulty of identifying pensioners and annuitants has led to frequent fraud from personation, involving in the aggregate a very large sum of money annually, as there is good reason to believe. If finger imprints could be practically brought into use, such frauds would be extremely difficult. I am still unable to speak positively as to the best way of making them, but the plan adopted at the laboratory is as follows. A copper plate is smoothly covered with a very thin layer of printers' ink, a printers' roller being used, and the plate being cleaned every day. When the layer is thin, no ink penetrates into the delicate furrows of the skin, but the ridges only are inked, and these leave their impression when the inked thumb is pressed on paper. In this way a permanent mark is registered. A little turpentine cleans the fingers effectually afterwards. But for purposes of identification a simpler process is necessary, one by which a person suspected of personation could furnish an imprint for comparison with the registered mark without having recourse to the troublesome paraphernalia of the printer. Such a process is afforded by slightly smoking a piece of smooth metal or glass over the candle, pressing the finger on it, and then making the imprint on a bit of gummed paper that is slightly damped. The impression is a particularly good one, and is sufficiently durable for the purpose. The iron used for the ironing of clothes is excellent for the purpose; even a smooth penny can be used. As for the gummed paper, luggage labels can be used; even the fringe to sheets of postage stamps is broad enough to include as much of the impression as is especially wanted—namely, where the whorl of ridges takes its origin.

[1] Address delivered at the anniversary meeting of the Anthropological Institute, on Tuesday, January 22, by Mr. Francis Galton, F.R.S., President.

I hope at some future time to recur to this subject.

Correlation.—The measurements made at the laboratory have already afforded data for determining the general form of the relation that connects the measures of the different bodily parts of the same person. We know in a general way that a long arm or a long foot implies on the whole a tall stature—*ex pede Herculem;* and conversely that a tall stature implies a long foot. But the question was as to whether that reciprocal relation, or correlation as it is commonly called, admitted of being precisely expressed. Correlation is a very wide subject indeed. It exists wherever the variations of two objects are in part due to common causes; but on this occasion I must only speak of such correlations as have an anthropological interest. The particular problem I first had in view was to ascertain the practical limitations of the ingenious method of anthropometric identification due to M. A. Bertillon, and now in habitual use in the criminal administration of France. As the lengths of the various limbs in the same person are to some degree related together, it was of interest to ascertain the extent to which they still admit of being treated as independent. The first results of the inquiry, which is not yet completed, have been to myself a grateful surprise. Not only did it turn out that the expression and the measure of correlation between any two variables are exceedingly simple and definite, but it became evident almost from the first that I had unconsciously explored the very same ground before. No sooner had I begun to tabulate the data than I saw that they ran in just the same form as those that referred to family likeness in stature, and which were submitted to you two years ago. A very little reflection made it clear that family likeness was nothing more than a particular case of the wide subject of correlation, and that the whole of the reasoning already bestowed upon the special case of family likeness was equally applicable to correlation in its most general aspect.

It may be recollected that family likeness in any given degree of kinship—say that between father and son—was expressed by the fact that any peculiarity in the father appears in the son, reduced on the average to just one-third of its amount. Conversely, however paradoxical it might at first sight appear, any peculiarity in a son appears in the father, also reduced on the average to one-third of its amount. The regression, as I called it, from the stature of the known father to the average son, or from the known son to the average father, was here from 1 to $\frac{1}{3}$; from the known brother to the unknown brother it was $\frac{2}{3}$; from uncle to nephew, or from nephew to uncle, it was $\frac{2}{9}$; and in kinship so distant as to have insensible influence, it was from 1 to 0. Whether the peculiarity was large or small, these ratios remained unaltered. The reason of all this was thoroughly explained, and need not be repeated here. Now the relation of head-length to head-breadth, whose variations are on much the same scale, is of the same kind as the above. They are akin to each other in the same sense as kinsmen are. So it would be in the closer relation between the lengths of the corresponding limbs, left arm to right arm, left leg to right leg. The regression would be strictly reciprocal in these cases. When, however, we compare limbs whose variations are on different scales, these differences of scale have to be allowed for before the regression can assume a reciprocal form. The plan of making the requisite allowance is perfectly

simple, but I cannot explain it without using technical terms. In some cases this allowance is large; thus the length of the middle finger varies at so very different a rate from that of the stature that 1 inch of difference of middle-finger length is associated on the average with 8·4 inches of stature. On the other hand, 10 inches of stature is associated on the average with 0·6 inch of middle-finger length. There is no reciprocity in these numerals; yet, for all that, when the scale of their respective variabilities is taken into account as above mentioned, the values at once become strictly reciprocal. I shall be able to explain this better later on.

In every pair of correlated variables the conditions that were shown to characterize kinship will necessarily be present—namely, that variation in one of the pair is on the average associated with a proportionate variation in the other, the proportion being the same whatever may be the amount of the variation. Again, when allowance is made for their respective scales of variability, the proportion is strictly reciprocal, and it is always from 1 to something less than 1. In other words, there is always regression.

Variety.—The principal topic of my further remarks will be the claims of variety to more consideration from anthropologists than it has hitherto received. They commonly devote their inquiries to the mean values of different groups, while the variety of the individuals who constitute those groups is too often passed over with contented neglect. It seems to me a great loss of opportunity when, after observations have been laboriously collected, and been subsequently discussed in order to obtain mean values from them, the very little extra trouble is not taken that would determine other values whereby to express the var ety of the individuals in those groups. Much experience some years back, and much new experience during the past year, has proved to me the ease with which variety may be adequately expressed, and the high importance of taking it into account. There are numerous problems of especial interest to anthropologists that deal solely with variety.

There can be little doubt that most persons fail to have an adequate conception of the orderliness of variability, and think it useless to pay scientific attention to variety, as being, in their view, a subject wholly beyond the powers of definition. They forget that what is confessedly undefined in the individual may be definite in the group, and that uncertainty as regards the one is in no way incompatible with statistical assurance as regards the other. Almost everybody is familiar nowadays with the constancy of the average in different samples of the same large group, but they do not often realize the way in which the same statistical constancy permeates the whole of the group. The Mean or the Average is practically nothing more than the middlemost value in a marshaled series. A constancy analogous to that of the Mean characterizes the values that occupy any other fractional position that we please to name, such as the 10th per cent., or the 20th per cent.; it is not peculiar to the 50th per cent., or middlemost.

Greater interest is usually attached to individuals who occupy positions towards either of the ends of a marshaled series, than to those who stand about its middle. For example, an average man is morally and intellectually a very uninteresting being. The class to which he belongs is bulky, and no doubt serves to keep the course of social life in action. It also affords, by its inertia, a regulator that, like the fly-wheel to the steam-engine, resists sudden and irregular changes. But the average man is of no direct help towards evolution, which appears to our dim vision to be the primary purpose, so to speak, of all living existence. Evolution is an unresting progression; the nature of the average individual is essentially unprogressive. His children tend to resemble him exactly, whereas the children of exceptional persons tend to regress towards mediocrity. Consider the interest attached to variation in the moral and intellectual nature of man, and the value of variability in those respects. For example, in the Hebrew race, whose average worth shows little that is worthy of note, but which is mainly of interest on account of its variety. Its variability in ancient and modern times seems to have been extraordinarily great. It has been able to supply men, time after time, who have towered high above their fellows, and left enduring marks on the history of the world.

Some thoroughgoing democrats may look with complacency on a mob of mediocrities, but to most other persons they are the reverse of attractive. The absence of heroic gifts is a heavy set-off against the freedom from a corresponding number of very degraded forms. The general standard of thought and morals in a mob of mediocrities must be mediocre, and, what is worse, contentedly so. The lack of living men to afford lofty examples, and to educate the virtue of reverence, would leave an irremediable blank. All men would find themselves at nearly the same dead average level, each as meanly endowed as his neighbour.

These remarks apply with obvious modifications to variety in the physical faculties. Peculiar gifts, moreover, afford an especial justification for division of labour, each man doing that which he can do best.

The method I have myself usually adopted for expressing and dealing with the variety of the individuals in a group, has been already explained on more than one occasion. I should not have again alluded to it had I not had much occasion of late to test and develop it, also to devise an unpretentious little table of figures, that I call a "table of normal distribution," which has been of singular assistance to myself. I trust it may be equally useful to other anthropologists. It is appended to these remarks, and I should like after a short necessary preface to say something about it. The table and its origin, and several uses to which it has been applied, will be found in a book by myself, that will be published in a few days, called "Natural Inheritance" (Macmillan and Co.). All the data to which I shall refer will be found in that book also, except such as concern correlation. These accompanied a memoir read by me only a month ago before the Royal Society, and will be published in due course in their Proceedings.[1]

It has already been said that the first step in the problem of expressing variety among the individual members of any sample, is to marshal their measures in order, into a class. We begin with the smallest measure and end with the greatest. The object of the next step is to free ourselves from the embarrasment due to the different numbers of individuals in different classes. This is effected by dividing the class, whatever its size may be, into 100 equal portions, calling the lines that divide the portions by the name of grades. The first of these portions will therefore lie between grades 0° and 1°, and the hundredth and last portion between grades 99° and 100°. We have next to find by interpolation the values that correspond to as many of these grades as we care to deal with. It is of no consequence whether or no the number in the class is evenly divisible by 100, because we can interpolate and get the values we want, all the same. This having been done, the value that corresponds with the 50th grade will be the middlemost. It is practically the same for ordinary purposes as the mean value or the average value; but as it may not be strictly the same, it is right to call it by a distinctive name, and none simpler or more convenient occurs than the letter M. So I will henceforth use M to denote the middlemost or median value, or, in other words, that which corresponds to the 50th (centesimal) grade.

The difference between the extreme ends of a marshaled

[1] For abstract, see NATURE, January 3. p. 238. For tables of percentiles, see vol. xxxi. p. 223. For hereditary stature, see vol. xxxiii. p. 295.

series is no proper measure of the variety of the men who compose it. However few in number the objects in the series may be, it is always possible that a giant or a dwarf, so to speak, may be among them. The presence of either would mislead as to the range of variety likely to be found in another sample taken from the same group. The values in a marshaled series run with regularity only about its broad and middle part; they never do so in the parts near to either of its extremities. In a series that consists of a few hundreds of individuals, the regularity usually begins at about grade 5°, and continues up to about grade 95°. Therefore it is out of this middle part, between 5° and 95°, or better, in a still more central portion of it, that points should be adopted between which variety may be measured. Such points are conveniently found at the 25th and the 75th grades. Just as the grade 50° divides the class into two equal parts, so the grades 25° and 75° subdivide it into quarters, and the difference between those values affords an irreproachable basis for the unit of variety. The actual unit is half the value of that difference, because the value at 25° tends to be just as much below that at 50°, as the value at 75° is above it. Therefore the average of these two values is a better measure than their sum. Briefly, if we distinguish the measure at 25° by the letter Q_1, and that at 75° by Q_3, then the unit of variety is $\frac{1}{2}(Q_3 - Q_1)$, and this unit we will henceforth call Q. As M measures the average, so Q measures the variety, and they are independent of one another. In strength, for example, the relation of Q to M in the particular group of adult males on which I worked was as 1 to 10; in the statures of the same group it was as 1 to 40; in breathing capacity as 1 to 9; in weight as 1 to 14.

The arithmetic mean or average is a muddle of all the values in the series; it is by no means so clear an idea as the middlemost value M. Therefore, although the peculiarities of an individual are commonly considered in the light of deviations from the average value, I prefer to reckon them as deviations from M. Practically the two methods are identical, but I find the latter more convenient to work with, and believe it to be the better of the two in every way.

Deviation is identical with variation, and the well-known law of frequency of error gives data whence the *relative* values of the deviations at the several grades may be calculated for any normal series. If we know the deviation at any one grade, then the *absolute* value of those at every other grade can be calculated; consequently the variety of the whole series is thereby expressed.

The small table of distribution, of which I spoke, gives the values at each grade when Q is equal to 1. Then the value at 25° is − 1, and that at 75° is + 1. If we desire to determine Q in any such series, the only required datum is the deviation at some one known grade, since, by dividing that deviation by the tabular value, we get Q at once. Or, conversely, if we know the Q of the series, and wish to calculate the deviation at any given grade, we multiply Q by the tabular deviation. Thus, in stature, which varies in an approximately normal manner, the value of Q is about 1·7 inch, therefore to find the deviation in stature at any grade, we multiply 1·7 inch by the tabular value.

If we know the *measures* at any two grades of a normal series, we are easily able to calculate both Q and M, and can thence derive the measures at any other desired grades. I have long since pointed out the possibility of a traveller availing himself of this method; but, for the want of a table of distribution, the calculation would probably puzzle him. With the aid of this table the calculation is made most readily. Let us suppose that the traveller is among savages who use the bow, and that he desires to learn as much as he can about their strengths. He selects two bows; the one somewhat easy to draw, and the other somewhat difficult, and at leisure, either before or after the experiment, he ascertains exactly how many pounds weight they severally require to draw them to the full. Then by exciting emulation, and by offering small prizes, he induces a great many of the natives to try their strengths upon them. He notes how many make the attempt, and how many of them fail in either test. This is all the observation requisite, though common-sense would suggest the use of three and not two bows, in order that the data from the third bow might correct or confirm the results derived from the other two. Let us work out a case, not an imaginary one, but derived from tables I have already published, and of which I will speak directly. Let the problem be as follows:—

30 per cent. of the men failed to exert a pulling strength of 68 pounds; 60 per cent. failed to pull 77 pounds. What is the Q and the M of the group?

Consider this 30 per cent. to be the exact equivalent of grade 30°, and the 60 per cent. of grade 60°. The reason why the percentage of failure, and the number of the grade are always equivalent will be found in a footnote to the table, and I need not stop to speak of it. Now, the tabular value at grade 30° is − 0·78; that at 60° is + 0·38; the difference between them being 1·16. On the other hand, the difference between the two test values of 68 pounds and 77 pounds is 9 pounds. Therefore Q is equal to 9 pounds divided by 1·16; that is, to 7·8 pounds. M may be obtained by either of two ways, which will always give the same answer. We may subract 0·38 × 7·8 pounds from 77 pounds, or we may add 0·78 × 7·8 pounds to 68 pounds. Each gives 74 pounds. Observation gave precisely these values both for Q and for M. The data were published in the Journal of this Institute as a table of "percentiles," and were derived from measures made at the International Health Exhibition. The value of M is given directly in the table, but that of Q happens not to be given there; it may easily be found by interpolation. That table affords excellent material for experimental calculations on the principle of this test, and for estimating its trustworthiness in practice.

It contains a variety of measures referring to eighteen different series, all corresponding to the same grades—namely, to 5°, 10°, 20°, and onwards for every tenth grade up to 90° and ending with 95°. The measures refer to stature, height sitting above seat of chair, span, weight, breathing capacity, strength of pull, strength of squeeze, swiftness of blow, keenness of eyesight, in each case of adult males and of adult females separately. I have since found that when the deviations are all reduced in terms of their respective Q values, by dividing each of them by its Q, that the average value of all the deviations at each of the grades in the eighteen series closely corresponds to the normal series, though individually they differ more or less from it, some in one way, some in another. On the whole, the error of treating an unknown series as if it were a normal one can rarely be very large, always supposing that we do not meddle with grades lower than 5° or higher than 95°.

It will be of interest to put the comparison on record. It is as follows:—

Grades	5°	10°	20°	30°	40°	50°
Observed	− 2·44	− 1·87	− 1·24	− 0·77	− 0·40	0
Normal − below 50° + above 50° ...	2·44	1·90	1·25	0·78	0·38	
Observed	+ 2·47	+ 1·92	+ 1·21	+ 0·75	+ 0·38	0
Grades	95°	90°	80°	70°	60°	50°

The "observed" are the mean values, made as above described, of the eighteen series; the "normal" are taken from the table of distribution given further on.

An ingenious traveller might obtain a great number of approximate and interesting data by the method just described, measuring various faculties of the natives, such as their delicacy of eyesight and hearing, their swiftness in running, their accuracy of aim with spear, arrow, boomerang, sling, gun, and so forth, either laterally or else vertically, distance of throw, stature, and much else. But he should certainly use three test objects, and not two only.

It should be remarked that, if the distribution of deviation was constant throughout any large class of faculties, though the Q might differ in different sub-classes of it, then, even though the distribution of that faculty was very far indeed from being normal, an appropriate table of distribution could be drawn up to solve such problems as those mentioned above. I have as yet no accurate data to put this idea to a practical test.

There are three convenient stages of expressing the variety of the various measures in a series, each reaching considerably nearer to precision than the one before. The first is to give only Q and M; the second is to record the measures at the grades 10°, 25°, 50°, 75°, and 90°; the third is the more minute method, adopted in the table of percentiles—viz. to give the measures at 5°, 10°, 20°, &c., 80°, 90°, and 95°. It may in some cases be found worth while to go further, say to 1° and 99°, or even also to 0° and 99°·9. So much for the expression of variety.

The use of Q is by no means limited to the objects just named. It is a necessary datum wherever the law of frequency of error has to be applied, and the properties of this law are applicable to a very large number of anthropological problems, with more accuracy of result than might have been anticipated when the series are only approximatively normal. This has been practically shown by the agreement among themselves of several inquiries to which I will shortly allude, and it is theoretically defensible by two considerations. The one is that the law of frequency supposes the amount of error or of deviation to be the same in symmetrically disposed grades on either side of 50°, their signs being alone different, *minus* on the one side of 50° and *plus* on the other. Now, in an observed series there may be, and often is, a want of symmetry, but if the deviate, say at 70°, is as much greater than the normal as the deviate at 30° is less than the normal, then the effects of these two upon the final result will be much the same as if there had been exact symmetry at those points. The other consideration is that any nonconformity of the observed deviates with the theoretical ones towards the end of the series has but a small and perhaps insensible effect on the broad general conclusions We need care little for any vagaries outside of the grades 5° and 95°, if the intervening portion gives fairly good results. The latter portion forms nine-tenths of the whole series, and even considerable irregularities in the remaining tenth are of small relative importance.

One great use of Q is to enable us to estimate the trustworthiness of our average results. We require to know both Q and the number of observations before we can estimate the degree of dependence to be placed on M. If there was only one observation, then the degree of dependence would be equal to Q; in other words, the error of M would be just as likely as not to exceed Q. If there were two, two hundred, two thousand, or any other number of observations, the error of M would then be reduced, but not in simple proportion. It would be as likely as not to exceed a value equal to Q divided by the square roots of those numbers. When we desire to ascertain the trustworthiness of the difference between the M values of two series, as between the mean statures of the professional and artisan class as derived from certain observations, the properties of the law of frequency of error must again be appealed to. Anthropologists are much engaged in studying such differences as these; but from their disregard of the simple datum Q, and from not being familiar with its employment, there is usually a lamentable and quite unnecessary vagueness in the value to be attached to their results. This is especially the case in comparisons between the average dimensions of the skulls of various races, which often depend upon the measurement of only a few specimens. An almost solitary exception to this needless laxity will be found in a brief but admirably-expressed memoir by Dr. Venn, the well-known author of the "Logic of Chance." It is upon Cambridge anthropometry, and was published in the last number of the Journal of this Institute. It deserves to be a model to those who are engaged in similar inquiries.

Another class of investigations in which a knowledge of Q is essential was spoken of some time back—namely, questions of correlation in the widest sense of the word. These problems have nothing to do with the relations of the M value, but are solely concerned with those of the deviations from M at the various grades. It is true that a knowledge of M is requisite in order to subtract it from the measures, and so to get at the deviations. But after this is done, M is put aside. It has no part in the work of the problem; it is only after the results have been arrived at without its use that it is again brought forward and added to them. Numerous properties of the law of frequency of error in which Q is the datum were utilized in my inquiries into family likeness in stature, and in all cases they brought out consistent results. An excellent example of this was seen in the success of the methods employed to determine the variety in families of brothers. Four different properties of the law had to be applied to partly different samples of the same group in order to determine the value of the Q of stature in fraternities, and they respectively gave 1·07, 0·98, 1·10, and 1·10 inch, which, statistically speaking, are much alike. Certain properties of the law of frequency of error were also applied to family likeness in eye colour, with results that gave by calculation the total number of light-eyed children in families differently grouped according to their parentage and grandparentage, and according to three different sets of data, as 623, 601, and 614 respectively, the observed number being 629. Other properties of the same law have been applied to determine the ratio of artistic to non-artistic children in families whose parentages were known to be either both artistic, one artistic, one not, or neither artistic. They gave to 1507 children the ratios of 64, 39, and 21, respectively, as against the observed values of 60, 39, and 17.

Lastly, as regards the correlation of lengths of the different limbs. It has already been shown that the correlation lies between the deviations, and has nothing to do with the values of M. Now, to express this relation truly, so that it shall be reciprocal, the scale of deviation of the correlated limbs, say, for example, of the cubit and of the stature, must be reduced to a common standard. We therefore reduce them severally to scales in each of which their own Q is the unit. The Q of the cubit is 0·56 inch, therefore we divide each of its deviations by 0·56. The Q of the stature is 1·75 inch, so we divide each of its deviations by 1·75. When this is done the correlation is perfect. The value of regression is found to be 0·8, whether the cubit be taken as the "subject" and the mean of the corresponding statures as the "relative," or *vice versâ*.

The value of the regression has been ascertained for each of many pairs of the following elements, and a comparison was made in each case between the correlated values as observed and those calculated from the ratio of regression. The coincidence was close throughout, quite as much so as the small number of cases under examination, 350 in all, could lead us to hope. The elements were nine in all, viz. head-length, breadth of head, length of right leg below the knee, of left cubit, of left middle finger, of the height sitting above the chair, of

stature, of the differences between the two foregoing (which indicate the total length of the lower limbs), and of the span. Anthropologists seem to have little idea of the wide fields of inquiry open to them as soon as they are prepared to deal with individual variety and cease to narrow their view to the consideration of the average.

Enough has now been said to justify the claims with which I started, and which take this final form. First, wherever it is likely to be of use, that, in series of which the M is calculated, the measures at a certain number of selected grades should also be calculated and given, sufficient to enable the rest of the series to be found with adequate accuracy by interpolation. Secondly, that the value of Q should always be given, as well as that for M, for two reasons. The one is, that they suffice between them to give an approximate determination of the whole series, more closely approximate as the series is more closely of the normal type; and, secondly, because Q is an essential datum before any application can be made of the law of frequency of error. The properties of this law are, as we have seen, largely available in anthropological inquiry. They enable us to define the trustworthiness of our results, and to deal with such interesting problems as those of correlation and family resemblance, which cannot be solved without its help.

Table of ordinates to normal curve of distribution, in which the unit = the probable error, and the grades, which are the abscissæ, run from 0° *to* 100°.

Grades.	0	1	2	3	4	5	6	7	8	9
0	∞	−3·45	−3·05	−2·79	−2·60	−2·44	−2·31	−2·19	−2·08	−1·99
10	−1·90	−1·82	−1·74	−1·67	−1·60	−1·54	−1·47	−1·42	−1·36	−1·30
20	−1·25	−1·20	−1·15	−1·10	−1·05	−1·00	−0·95	−0·91	−0·86	−0·82
30	−0·78	−0·74	−0·69	−0·65	−0·61	−0·57	−0·53	−0·49	−0·45	−0·41
40	−0·38	−0·34	−0·30	−0·26	−0·22	−0·19	−0·15	−0·11	−0·07	−0·04
50	0·00	+0·04	+0·07	+0·11	+0·15	+0·19	+0·22	+0·26	+0·30	+0·34
60	+0·38	+0·41	+0·45	+0·49	+0·53	+0·57	+0·61	+0·65	+0·69	+0·74
70	+0·78	+0·82	+0·86	+0·91	+0·95	+1·00	+1·05	+1·10	+1·15	+1·20
80	+1·25	+1·30	+1·36	+1·42	+1·47	+1·54	+1·60	+1·67	+1·74	+1·82
90	+1·99	+1·99	+2·08	+2·19	+2·31	+2·44	+2·60	+2·79	+3·05	+3·45

This table is an inverse rendering of the values derived by interpolation from the ordinary table of the probability integral, but its unit is changed from that of the modulus to that of the probable error, Q. and the (centesimal) grades are reckoned from 0° to 100°. In the usual way of reckoning, the 50th grade should have been reckoned as 0°, and the deviations should have run on the one side down to − 50°, and on the other up to + 50°.

Referring to what was said some way back, that if 30 per cent. fail to pull 60 pounds, then 60 pounds must be taken as the measure corresponding to grade 30°, the reason is as follows. The 30th grade separates the man who ranks 30th in a class of 100 men from his neighbour who ranks 31st. It does so for the same reason that grade 1° separates the man who ranks 1st from the man who ranks 2nd. Now, the 30th man failed in the test, and the 31st succeeded. Therefore the grade corresponding to bare success lies between them, and is the same as grade 30°.

SUPPOSED FOSSILS FROM THE SOUTHERN HIGHLANDS.

ON Monday, the 14th instant, the Royal Society of Edinburgh held a special meeting for the purpose of hearing a discussion on the crystalline rocks of the Scottish Highlands. The subject was brought forward by the Duke of Argyll, who had found in the quartzite beds which cross Loch Fyne near Inveraray certain markings which he believed to be of organic origin. His attention was first called to some ferruginous stalk-like incrustations on the surfaces of fragments of quartzite, his impression being that these markings were the remains of plants, and were embedded in the rock. The importance of the discovery of organic remains in any of the rocks that form the Central and Southern Highlands of Scotland will at once be recognized by geologists. Since the recent work of the Geological Survey in Sutherland and Ross, and the demonstration thereby afforded that the apparent upward succession on which Murchison relied, from the base of the lowest quartzite up into the upper or eastern or younger gneisses, is deceptive, there has been, perhaps, a tendency to assume that the extraordinarily complicated structure that supervenes to the east of the quartzites and limestones of Sutherland extends across the whole of the rest of the Highlands, and that the crystalline schists of these regions are made up of all kinds of crushed and sheared igneous or sedimentary masses, out of which it may be impossible to make anything like intelligible order. But those observers who have themselves examined the schists of the central and southern counties of the Highlands are tolerably confident that such assumptions have no warrant in the actual structure of the ground. On the contrary, they regard the greater proportion of the schistose and altered rocks of these districts as unquestionably of sedimentary origin. They feel persuaded that sooner or later they will be found to yield fossils, and that any day may bring to light a series of corals, shells, graptolites, or trilobites, which will furnish a palæontological basis for settling the geological age of the rocks, and placing them in their true position with regard to the Palæozoic formations of the rest of the country.

The announcement that the Duke of Argyll had found what seemed to be organic remains in the Inveraray quartzites awakened accordingly much interest among geologists. His Grace soon discovered, however, that what he had at first believed to be fossils were only external markings due to the precipitation of hydrous peroxide of iron round the decaying stems of mosses, heaths, or other plants. These markings occurred indifferently on pieces of quartzite, mica-schist, gneiss, &c., and in no instance were found within the stone, but always on the surface. But in turning over the exposed blocks of quartzite the Duke found numerous ferruginous markings which undoubtedly occurred all through the interior of the rock. After quarrying away portions of the solid rock, collecting a large series of specimens, and comparing them with others obtained from the quartzite of Sutherland, he deemed himself in a position to announce the probably organic nature of these markings; and the paper which he communicated last week to the Royal Society of Edinburgh gave the results of his inquiries. The bodies which he regards as fossils are compared by him to the "annelid burrows" which form so prominent a feature in the quartzites of Sutherland and Ross. He recognizes in the Inveraray rock similar ovate sections, the position and form of each tube being marked by a ferruginous ring, which is well defined along its inner margin, but fades outward into a general discoloration of the stone. He points out that in the Inveraray rock, as in that of the North-West Highlands, there is a general tendency of these ovate bodies to lie in one prevalent direction; and though he admits that the rocks have been considerably disturbed and crushed, he cannot trace among them any evidence of such stupendous movements as have been described from Sutherland. Accordingly he is disposed to look upon the parallelism of the stripes into which he thinks the original tubes have been flattened as evidence of the direction in which the worms burrowed through the still soft sand.

Perhaps the most original and valuable part of the Duke's paper was the account which he gave of his own experiments on the habits of the common lob-worms of our present shores. He had watched the operations of these creatures on the beach of dark silt at Inveraray; had cut out portions of the silt with the burrows and mounds intact, and had these removed to his own drawing-room to enable him to watch them more attentively. He had likewise injected plaster of Paris into the vertical or winding passages made by the worms in the silt, and had thereby obtained casts of the interior of these tunnels. He exhibited a very interesting and valuable collection of specimens illustrating these researches.

Mr. Geikie, the Director-General of the Geological

Survey, opened the discussion, and regretted that, though he had enjoyed the advantage of seeing the large collection of specimens made by the Duke of Argyll from the Inveraray quartzite, and also of examining the rock *in situ*, he was still unconvinced that the markings were really of organic origin. It was possible, he thought, to trace a series of stages from single crystals or irregular groups of crystals of pyrites through variously shaped aggregates into the "ovate bodies" of the Duke. In the more solid, massive, and uncrushed portions of the quartzite, these aggregates could be seen quite fresh, and probably not far from their original shape. But wherever the rock had undergone shearing (and this was the case throughout most of its mass), its component particles had been drawn out in the direction of movement, the original irregular, rounded or egg-shaped aggregates of sulphide of iron had been flattened and elongated, becoming eventually mere strips that run parallel to each other. The trend of these strips exactly coincided with that of the long axes of the foliation-minerals in the surrounding rocks, and were regarded by Mr. Geikie as pointing to the results of shearing in the rock-mass and not to the burrowings of worms. The ferruginous rings seemed to him to be due to an oxidation and leaching out of the pyritous matter of the little mineral aggregates, as so often happens among the Carboniferous and Jurassic sandstones that contain ferruginous concretions. While he could not admit that the markings in the quartzite of Inveraray had yet been shown to be of organic origin, he thought it quite possible that the precipitation of the iron-disulphide had originally taken place in presence of decomposing organic matter, as in many blue muds of the present day, and that portions of such pyritous mud had been drifted into the sandy deposit which is now quartzite.

Mr. B. N. Peach, of the Geological Survey, was also unable to recognize organic forms among the Inveraray markings. He thought that the parallelism of these markings where they were most elongated, and their coincidence with the general line of shearing movement in the rock, cast doubt upon their having any connection with worm-burrows.

Mr. Murray, of the *Challenger* Expedition, who thinks that sandstone deposits generally are sub-aërial formations, was disposed to refer the so-called annelid tubes of the Sutherland quartzite not to the borings of marine worms, but to the remains of terrestrial plants that grew upon sand-dunes.

In a paper which followed this discussion, Mr. Geikie gave an account of the evidence supplied by the rocks of the Highlands of remarkable deformation by mechanical movements. Illustrating his remarks by a large series of specimens, he showed how the Cambrian conglomerate of Assynt had its pebbles of quartz drawn out and its original sandy mud converted into a fine micaceous schist; how the conglomerates of the Central Highlands had their quartz-pebbles flattened like buttons and drawn out in the direction of movement, while their envelope of original sand and mud had been changed into a quartz-schist; how the granular quartzite of Sutherland had been crushed and rolled out into a thoroughly schistose mass; how the coarse Archæan pegmatites had been likewise crushed down until their material had, as it were, flowed onwards so as now to show a close parallel to the "fluxion-structure" of many porphyries, and even to assume a finely laminated or shaly structure, and lastly, how the highly crystalline basic dykes of the most ancient gneiss of the north-west had been sheared and rearranged until they passed into the most perfect forms of sericite-schist. He adverted to the obviously sedimentary origin of the great mass of the rocks constituting the Highlands east of the line of the Great Glen, and mentioned that the recent work of the Geological Survey in tracing the great belt of limestones from the coast of Banffshire through the Grampians into Argyllshire afforded now a good horizon, from which it might be hoped the general structure of the Highlands might be worked out. He exhibited specimens of quartzite from Perthshire and other districts containing various markings, some of which there could be little doubt were of organic origin. He also showed a singularly interesting series of specimens which he had recently received from Dr. Reusch, of the Geological Survey of Norway, displaying recognizable trilobites and corals embedded in a finely crumpled micaceous schist, exactly similar in character to much of the schist that constitutes wide regions in the Scottish Highlands. These specimens afforded much encouragement to search for fossils in the calcareous and ferruginous layers and concretions that occur so frequently among our finer mica-schists and phyllites.

THE LAW OF STORMS IN CHINA.

THE law of storms in Hong Kong was investigated by aid of the lithographed paths of the typhoons in 1884 and 1885, published in "Observations and Researches made in 1886" and in those of 1886 and 1887 now in course of publication. Only those within 300 miles of the Observatory were considered in this connection. The angles between the wind and the radius vector, *i.e.* the line joining the Observatory with the centre of the typhoon, were measured and mean values derived, and the same was done for Victoria Peak (1816 feet above the sea) and for the lower clouds.

No connection could in any case be traced between the distance from the centre and the direction of the wind, but the latter depends upon the bearing of the centre. As pointed out in "The Law of Storms in the Eastern Seas" (NATURE, vol. xxxv. p. 136), and elsewhere, the wind has a tendency to blow along the southern coast of China when a typhoon is raging in the China Sea, so that the wind in such cases veers only about half as much while the typhoon moves westward as in other cases, and this is the reason why the angle between the wind and the radius vector is larger than usual when the centre is situated to the south of Hong Kong.

When there is a typhoon anywhere between north and east within 300 miles of the colony—which, however, is not common—the wind at the Peak (about north-north-west) blows away from the centre—much more so than the clouds, which in fact describe almost a circle round the centre in that case; and this remarkable feature or something very like it has been found to obtain also at Ben Nevis with the centre of a depression in the north-east.

The angle between the wind and the radius vector is, at the Observatory, 81° north of the centre, 55° to the west, 56° to the south, and 58° to the east. At the Peak, it is 91° to the north, 87° to the west, 81° to the south, and 78° to the east. At the level of the lower clouds, it is 92° to the north, 85° to the west, 67° to the south, and 86° to the east.

The observations made at South Cape (Formosa) were treated similarly, with the following result: to the north of the centre the angle was 50°, to the west 56°, to the south 64°, and to the east 47°.

The angle observed on board ship in the China Sea in typhoons—say in about 16° N. lat.—is on an average 47°, as previously published; at South Cape (22° N. lat., the same as Hong Kong) 54°; at Hong Kong 62°; at Victoria Peak, above Hong Kong, 84°; and at the level of the lower clouds 82°. The angle previously obtained from observations made on board ship, and also at coast stations in about 32° N. lat., was 75° on an average, but so far north it seems to be more variable than in the China Sea, where it has been found remarkably constant, between (say) 12° N. and 20° N. But from the figures given it is seen that the angle increases with the latitude and with elevation above sea-level. The woodcut represents a typhoon in the neighbourhood of Hong Kong, or rather

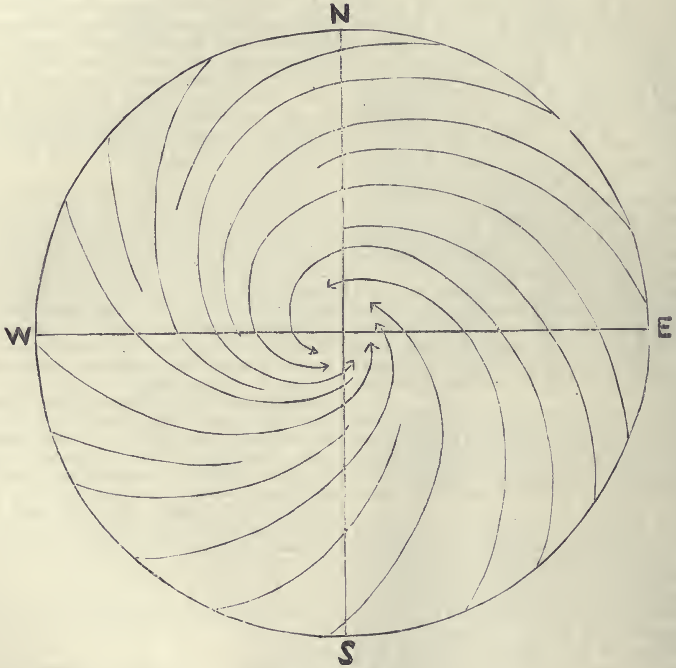
a mean of those observed during the four years. The diameter of the circle is 600 miles.

The average force of the wind according to Beaufort's scale (0–12), at various distances from the centre, expressed in nautical miles, is shown in the following table, but owing to the typhoons differing so much in size the figures representing the mean values are often widely different from the values obtained from observations made in a particular typhoon. That is not the case with the direction of the wind, which depends upon the bearing but not upon the distance from the centre or the size of a typhoon.

Distance.	Observatory.	Peak.	South Cape.	China Sea.
60	8	9	9	11
160	5	6	6	—
250	4	5	4	6

This table proves the wind to be strongest over the open sea, and also, though to a less extent, at some height above sea-level. The force of the wind increases at a greater rate on approaching the centre at sea, and that is to a certain extent likewise the case at South Cape, which is far from the mainland and soon reached by typhoons arriving fresh from the Pacific Ocean, where most typhoons originate, although some of them are formed to the westward of the Southern Philippines.

The force of the wind is much greater behind the centre than in the anterior semicircle both at sea and on shore, and the consequence is that the strongest blow is not experienced till the barometer begins to rise. For instance, at an average distance of about 160 miles, the mean force in Hong Kong is 7 to the north, 3 to the west, 6 to the south, and 5 to the east. At South Cape (Formosa) it is 6 to the north, 5 to the west, 7 to the south, and 6 to the east. To the north of a typhoon the wind is remarkably fresh along the southern coast of China, even when the centre is over 300 miles away. It makes an impression as if the trade-wind was blowing in the middle of summer, while a typhoon moves westward in the China Sea. In Southern Formosa, where typhoons moving north-westward predominate both in number and in intensity, the wind is strongest to the south or southeast of the centre.



A Typhoon in Hong Kong.

At Victoria Peak the force of the wind does not depend upon the bearing of the centre, or at any rate only slightly so. It follows that the wind-force registered there just before the approach of a typhoon considerably exceeds that registered at the Observatory. The difference in force is only about one on Beaufort's scale, when the centre is north or west of the colony, and while the centre is situated to the southward it usually blows harder at sea-level than on top of the Peak. W. DOBERCK.

THE STATE OF VESUVIUS.

THE "Note" in NATURE (p. 184) on the state of the Vesuvian volcano has been copied by many newspapers, and I have received a number of letters asking for further information. To satisfy this desire, I give the following particulars as to what occurred subsequent to December 15, 1888, and to the information above mentioned.

During the remainder of the month of December, the vent was extremely active, ranging from the second to the fourth degree of activity, so that the cone of eruption was often quite red, after a burst, from the large number of lava cakes falling on its sides. This constant ejection of fragments of red-hot pasty lava rapidly increased the

height and size of the eruptive cone, making its slope exceedingly steep. So rapid was its growth, that the most casual observers noticed it from Naples, and discussed it. In fact, from November 1 to January 6 at least 20 metres was added to the height of Vesuvius, whilst the size of the base of the cone of eruption was proportionally increased.

On January 1, 1889, the eruptive cone burst on the north side, allowing the lava to issue and flow down, turning east and west so as to fill up, in part, the crescentic depression between the annulus or crater ring of July and August 1886, and the cone of eruption which is situated eccentrically to the former. As I have shown elsewhere, outflows of lava from the cone of eruption are always very limited, the violence of the outburst being generally proportional to the distance of the lateral opening below the summit.

After this relief the activity fell to the first degree, but soon again rose to the third. On Sunday, the 6th, I was standing at the summit of the mountain on the 1872 crater plain, preparing my apparatus for a photograph of the cone of eruption, when suddenly (about 3 p.m.), at about half-way down the side of the eruptive cone, and facing me, a slight puff of dust occurred, followed by the oozing forth of some lava. This rapidly increased in quantity as it carried forward the fragments forming the sides of the aperture. I immediately changed my lens to an instantaneous one, and took two negatives. The explosive activity increased, so that I was standing in a constant hail of red-hot lava fragments. These it required constant vigilance to avoid, and my face and hands were scalded by the radiant heat from the rapidly advancing lava, and tormented by the whirlwinds that always occur under such conditions. My two porters abandoned me, so that I had just time to remove my apparatus two minutes before lava flowed over the spot where I stood. In consequence of these unfavourable conditions, I lost some of my coolness, and allowed my camera-cloth to partly hide the lens. I was therefore greatly disappointed to find only part of this splendid scene registered for the eyes of others who had not had the good fortune to see that interesting spectacle.

The point of rupture was a few degrees east of south, and nearly opposite the cleft of January 1. The opening at beginning could not have been more than 10 metres from the top of the vent, showing the great height of the lava in the volcanic chimney. The outflow was very rapid, for, half an hour after, the place where I took the photographs could not be approached by 40 or 50 metres, having been all inundated with lava. Part of the fluid rock rapidly reached the edge of the 1872 crater plain, and flowed some distance down the slope of the great cone in the direction of Torre Annunziata, and another portion flowed out by another gap a little farther east in the remaining edge of the 1872 crater ring. The supply, however, soon stopped, and late in the evening had already become consolidated. After this, the activity, as seen from Naples, slightly diminished, but the next evening it was again at the third degree. Cloud-cap somewhat interrupted the view up till last night (January 12), when it was again observed to be at the third degree, and the light emanating from the lava was very white, showing the high temperature.

So far, the great cone has resisted fracture, but the south-west fissure, to which I have already drawn attention, is more active, and from this side of the crater plain there is very great fumarolic activity. When, therefore, the hydrostatic pressure overcomes the resistance, it will probably be in this direction that a lateral outburst will take place.

H. J. JOHNSTON-LAVIS.

Naples, January 13.

VOLCANIC SEA WAVE.

THE following account from the Berlin *Annalen der Hydrographie*, 1888, p. 518, with reference to the wave observed in the regions about the north-east of New Guinea, already briefly noticed in NATURE (vol. xxxviii. p. 491), is of interest.

The data given are too vague to permit of definite conclusions as to the probabilities of the disturbances felt at Sydney and Arica having originated in a volcanic eruption in New Guinea, but it may be observed that, assuming that the volcanic centre was from 200 to 400 miles north of Hatzfeldt Harbour, in which direction sounds were heard at 6 a.m. on the 13th, followed in forty minutes by a wave, the disturbance recorded at Arica at 5 p.m. on the 14th would have travelled the intervening distance of 10,000 geographical miles at a speed of 416 miles an hour, a velocity which agrees very fairly with the probable mean depth of ocean traversed.

To Sydney, on the other hand, assuming the first disturbance to have occurred at 6 a.m. on the 15th, the speed would only have been about 60 miles an hour, which is much too low a velocity for the depth.

It will be observed that the waves both at New Guinea and Arica were of short period, and in this respect quite unlike the long-distance waves emanating from Krakatão in 1883.

W. J. L. WHARTON.

"With regard to the extraordinary tidal wave that was observed in the Bismarck Archipelago, and on the coast of New Guinea, on the 13th of March, Heft iii. of the 'Notices of Kaiser Wilhelm's Land and the Bismarck Archipelago' relates as follows:—

"After the Expedition which had been undertaken for the discovery of Herren von Below and Hunstein, who had attempted an exploration to the west coast of New Pomerania (New Britain), had returned without finding any trace of them, a second Expedition, consisting of seven officers and fourteen Miocese, under command of the surveyor, V. Brixen, was despatched on the 17th of March from Finsch Hafen to the west coast of the above-mentioned island. This discovered, on the 18th of March, the spot where Below's Expedition had landed, which was easily recognized by the objects lying there partly covered with sand—a tent, torn pieces of clothing, and bent bits of metal. A part of the Expedition then repaired to a ruined village near the place where the missing persons (according to the account of the two Miocese who had been saved) had encamped during the night of the 12th-13th of March on the shore. At this place the land falls very steeply, about 25 metres, to the sea, and there is only a narrow strip of flat coast between the declivity and the sea. The tidal wave had even occasioned a landslip, large stones and trees being torn away from the slope, so that here escape could have been scarcely possible, and, according to the two Miocese, the catastrophe happened before daybreak. With the exception of a few bamboos cut by a knife, no trace of an encampment was perceptible. An excavation, attempted on the 19th of March, led to no result. Sea-sand, stones, and things washed up by the sea, covered the former level of the shore for more than 4 feet. On the 20th of March parties were despatched into the interior in a north-easterly and southerly direction, who came upon the encampment of the natives who had escaped from the above-named village. As these confirmed, by gestures and signs, the accounts of the Miocese, hardly any doubt can remain that Below and Hunstein had fallen victims to the tidal wave. On the 21st of March a large cross, therefore, was erected at the place of the misfortune, and, to provide for necessity, two boxes with provisions and drink were buried under a

large and marked tree near the landing-place. The tidal wave on this portion of New Pomerania had rendered completely desolate a coast formerly covered with dense forest for a breadth of about 1 kilometre. Large spaces had been reduced to a swamp covered with trees heaped above one another, broken coral rocks, sea-sand, and a quantity of putrid fish. Measurements made at the declivities here give a height for the tidal wave of 12 metres (39 feet).

"As was to be expected, the tidal wave had also left its mark on other coasts of the German Protectorate, without having, however, caused any serious damage. In Hatzfeldt Hafen, on the coast of New Guinea, a noise like firing was heard on March 13, shortly after 6 a.m., from a north-north-easterly direction, and at 6.40 a.m. came an astonishingly high tidal wave from the north that rose 2 metres (6½ feet) above the highest flood-mark, and then receded with such violence that half the port was dry. The sea now rose and fell at intervals of three to four minutes, which lasted until 9 a.m.

"At 8 a.m. the height of the tidal wave stood at 7 to 8 metres (23 to 26 feet), so that the station was in imminent danger. In the course of the forenoon the movement gradually subsided, although the water still continued to rise and fall with steady intervals until 6 p.m., when it resumed its normal condition.

"In Kelana, the newly-established plantation near Cape King William, the phenomenon occurred from north-east at 6.30 a.m. The first wave forced itself 25 feet on the land the fourth, however, 35 feet; this was the greatest of the twenty waves observed, which came about every three minutes. The phenomenon was not observed here to be longer than an hour in occurring. No other circumstance of a striking nature was perceptible. The weather was calm and dull. On the morning of the 14th of March the whole coast to some distance was strewn with small pumice stones.

"From Matupi it was reported that from 8.15 until near 11 a.m. the sea receded at times from the island 12 to 15 feet below the lowest water mark, and then rose in several waves to the same height above high water mark. The phenomenon appeared chiefly on the south-east and north side of the island, the west side remaining untouched. The waves came partly from south, partly from west-north-west. The water appeared disturbed in its depths; it had a dark appearance, and carried discoloured foam. Neither earthquakes nor any subterranean rumblings were noticed. The weather was clear, with a gentle south-east breeze. On the south side of Gazelle Peninsula the phenomenon was also noticed by a ship ying at anchor.

"So far the report in the 'Notices of Kaiser Wilhelm's Land and Bismarck Archipelago.' Of the further movement of the tidal wave, nothing is as yet known, although it is not improbable that it spread further.

"In Sydney (Australia) and Arica (South America), extraordinary commotions of the sea were observed between the 14th and 17th of March, which may possibly have been in connection with this tidal wave. In Arica, as appeared in the *Mercurio* of Valparaiso of the 23rd of March, an immense wave was observed on the 14th of March towards 5 p.m., in the distance, which, increasing as it drew nearer, broke with great force near the pier. Three great waves followed quickly, one after another. Of the vessels busied in taking in cargo, several were shattered, and others capsized. The sea was for some time so agitated that the shipping of merchandize was attended with difficulty. On the island in front of the port the sea broke for a still longer period with great violence.

"According to the English journal NATURE (vol. xxxviii. p. 491), the tidal curves on the self-registering water-gauge in Sydney, showed on the 15th, 16th, and 17th of March, deviations from their customary form which may have been caused by the waves of an earthquake."

NOTES.

THE fifteenth general meeting of the Association for the Improvement of Geometrical Teaching was held on Saturday last at University College, London. After the reading of the Report of the Council, Mr. R. B. Hayward, who had been President for eleven years, resigned the presidency, and the post was conferred on Mr. G. M. Minchin, Professor of Applied Mathematics in the Royal Indian Engineering College at Cooper's Hill. In the place of Mr. Moulton, Q.C., Mr. Hayward was elected a Vice-President; while the other Vice-Presidents—the Rev. G. Richardson, Mr. R. Levett, and Mr. R. Tucker—retain their posts. In the course of his valedictory address, the retiring President remarked that, though they had not quite attained the expectations of some of their more ardent reformers, still they had met with a fair measure of success. Their influence was rather indirect than direct, and it must be expected that their advance would be, while steady, yet comparatively slow. The new President (Prof. Minchin) read a paper on "the vices of our scientific education."

IN the correspondence of Mrs. Austin, just published by her granddaughter, in the work entitled, "Three Generations of Englishwomen: Memoirs and Correspondence of Mrs. John Taylor, Mrs. Sarah Austin, and Lady Duff Gordon," there is an interesting and whimsical letter from Humboldt, dated Sans Souci, 1844. Mrs. Austin had written to him, suggesting a translation of his work, "Ansichten der Natur," into English. He jokes about the defects of his book from the translator's point of view, with its notes larger than the text, its "Teutonic sentimentality," and the impossibility of finding a title in English for it. He then proceeds:—"Alas! you have got someone in England whom you do not read, young Darwin, who went with the Expedition to the Straits of Magellan. He has succeeded far better than myself with the subject I took up. There are admirable descriptions of tropical nature in his journal, which you do not read because the author is a zoologist, which you imagine to be synonymous with 'bore.' Mr. Darwin has another merit—a very rare one in your country—he has praised me."

ON Tuesday, January 15, a new wing of the Leeds Mechanics' Institute, comprising the School of Science and Technology and Boys' Modern School, was opened by Sir James Kitson. The Leeds Mechanics' Institute was founded sixty-four years ago, and, like some other establishments of the same kind in various parts of the country, it has always recognized the need, not only from an intellectual but from an industrial point of view, of scientific education. Thanks, in part, to the wholesome influence of the Yorkshire College, Leeds, there has been lately in all the manufacturing centres of Yorkshire a remarkable growth of opinion as to the importance of this question; and the action taken by the Mechanics' Institute in adding to its buildings a set of rooms for scientific training must be regarded as a characteristic and eminently satisfactory sign of the times. The extension has involved a total cost of nearly £7000. The building is three stories high. The class rooms on the ground-floor will be used for the Boys' Modern School. On the first floor a series of rooms will be used jointly by the Boys' Modern School and the School of Science and Technology. On this floor there is a physics lecture theatre, with a sloping gallery capable of accommodating about fifty students. The metallurgical laboratory, the balance room, and the chemical laboratory occupy the second floor, which is reached by a wide stone staircase. In the former twenty-two students will be able to work at the same time.

EVERYONE who is in the habit of using Whitaker's Almanac is now familiar with the issue for 1889. It may not, however,

be too late to say that the work as a whole is more valuable than ever, and that especial credit is due to the editor for the excellent sections on subjects relating to astronomy. We may call his attention to the fact that for some mysterious reason the office of NATURE is not included in the list of newspaper offices in London.

THE prevention of smoke formed the subject of an interesting discussion at a recent meeting of the Institution of Engineers and Shipbuilders in Scotland. Mr. G. C. Thomson, who had read a paper on the subject, said, in summing up the debate, that he would like to add to his paper the effect of a day's fog in London. For the twenty-four hours ending Thursday morning, November 17, 1887, the gas sent out by the Gas light and Coke Company was 103,664,000 cubic feet, or 35,000,000 excess over the same day in 1886. The gas was sold at 3*s.* per 1000 feet, and was equal to £15,500, so that the value of the excess in money equalled £5250—a sum that would go a great way in putting many of the faulty furnaces in London into good working order, so that they would give no smoke. Mr. Thomson also called attention to the loss of health and life which a foggy day always entailed on a community.

AT the meeting of the Royal Geological Society of Ireland on January 9, Mr. Kinahan communicated a paper, a general supplement to his previous articles on the "Economic Geology of Ireland." In this the suggestion as to the pre-Cambrian age of some of the Irish rocks is of interest. The author pointed out that it is highly improbable that any Irish rocks are equivalents of the American Laurentians or Huronians; but in reference to the "Gap rocks" of the epoch between the Huronian and Primordial, called by Chamberlain the Agnotozoic epoch, he suggested that possibly rocks of this age might be found. He mentioned the Bray Head series, which he would provisionally call "Oldhamians," as rocks that seem to be evidently older than the Welsh Cambrians; and as the Welsh Cambrians by their fossils seem to be the equivalents of the American Primordials, any rocks older than the Welsh Cambrians ought to belong to strata of Agnotozoic age. The suggestion that the Oldhamians are older than the Welsh Cambrians was founded on the profound break between them and the Irish equivalents of the Llandriloes; while in Wales the Cambrians pass conformably upwards into the Welsh Llandriloes. The author then pointed out that if the Oldhamians are of pre-Cambrian age (Agnotozoic) it is probable the rocks of North-West Mayo (Mullet) are similarly Agnotozoic, while it is possible, if not probable, that Griffith's older rocks in Ulster (Donegal, Tyrone, and Antrim?) may also be Agnotozoic.

THE Quarterly Record of the Royal Botanic Society for April–June 1888 contains the report of a lecture, by Mr. G. J. Symons, on sunshine. The subject is discussed chiefly from an instrumental point of view, under the heads of thermometric solar radiation, sunshine-recorders, and sunlight-recorders. Mr. Symons points out that Newton, in the seventeenth century, compared the readings of two thermometers, one in the sun and the other in the shade. De Saussure, in 1774, was the first to make an apparatus for direct observations upon the heat of sunshine, and, in 1837, the subject was taken up by Sir John Herschel, M. Pouillet and others. Their researches led to the use of the black-bulb thermometer *in vacuo*, while bright and black-bulb thermometers were used by Arago in 1844. This class of instruments was further improved by the Rev. F. W. Stow, in 1869. The first direct sunshine-recorder was designed by the late Mr. J. F. Campbell, and erected by him in Whitehall, in December 1854; it consisted of a mahogany bowl, with a hollow sphere of glass, nearly filled with acidulated water, to form a lens. In December 1857, a solid glass sphere was substituted; the observations were discussed by Profs. Roscoe and Stewart (Proc. Roy. Soc., June 1875). Finally, towards the end of 1879, after various experiments at Greenwich and Kew Observatories, Prof. Stokes designed the card supporter which is now used by the Meteorological Office and other institutions. The observations have been discussed by Mr. Scott for the years 1880–85 (Quart. Journ. Roy. Met. Soc., July 1885). Mr. Blanford stated, some years ago, that this instrument would give better results than the thermometric method, which has now been practically discontinued in India. Of the photographic sunlight-recorders, the principal are those by Mr. J. P. Jordan and Prof. McLeod. Another pattern has been designed by Dr. Maurer, and illustrated in *La Nature* for May 19, 1888, in which it is stated that the paper can be left unchanged for twenty days. Mr. Symons concludes his interesting lecture by remarks upon the action of light upon vegetation.

REPORTS of earthquakes have lately been received from many different parts of the world. At Flekberö, in the Torrisdal, in South Norway, there was an earthquake on December 27, 1888. Shocks were felt at Herisau, Zug, Frauenfeld, and Zürich, and at Wyl (Aargau), on January 1, at 5 a.m. A severe shock occurred at Constance on January 7, at three minutes to 12 noon. It continued for two seconds, and seemed to move from west to east. It was also noticed in several parts of North Switzerland; and at Wattwyl the shock was so severe that the inhabitants rushed out of their houses in terror. At St. Gallen, the pictures, curtains, &c., swung about on the walls, and the woodwork creaked. According to a telegram sent through Reuter's agency from Smyrna on January 21, a disastrous earthquake occurred last Thursday at Sparta (?), in Asia Minor, in which 300 houses were destroyed. A shock was felt at Athens on January 22, as well as at Megara and Arachova. It was accompanied by heavy rains and a violent gale.

ON Friday morning last, a shock of earthquake was felt in the county of Midlothian. Of this earthquake we may have something to say on a future occasion.

REFERRING to the Calcutta earthquake of December 23 last, which is said to have been the most severe felt since 1885, the *Englishman* says that it took place at 10.50 p.m., and lasted for about a minute and a half. It was severe enough to try the stability of substantial houses. Sleepers were awakened by the loud rattling of doors and windows, and pemka frames and lamps swung about in a curious fashion. So far as could be judged, the direction of the wave was from east to west. The disturbance was wide-spread, but appears to have varied in intensity in different places.

THE papers on "Modern Views of Electricity," by Prof. Oliver Lodge, which have been appearing in NATURE, will soon be published as a volume of the "Nature Series."

A WORK on "The Principles of Inductive or Empirical Logic," by Dr. John Venn, is about to be issued by Messrs. Macmillan. It contains the substance of lectures delivered in Caius College for a number of years past. The general treatment of the subject is somewhat more in accord with that adopted by J. S. Mill than with that of the majority of recent English works on logic.

MESSRS. MACMILLAN AND CO. have in the press Part I. of "A Graduated Course of Natural Science for Elementary and Technical Schools and Colleges," by M. B. Loewy. The author's object is to place the teaching of natural science in schools upon an exclusively experimental basis, and to make it at the same time thoroughly methodical and systematic, the scholar being led from known and easily-understood facts to less-known and more difficult results. In this way, it is thought

instruction in science may be brought into close harmony with educational methods employed in other subjects of school teaching.

MESSRS. MACMILLAN AND CO. will also issue soon "Hydrostatics for Beginners," by F. W. Sanderson. The work is based on the author's experience in teaching physics to large classes of boys varying from 12 to 19, and in arranging and conducting each class in laboratory work.

A NEW compound, containing aluminium in a lower state of oxidation corresponding to ferrous iron, has been obtained by Prof. Hampe-Clausthal. It is a double fluoride of sodium and aluminium of the composition $2NaF . AlF_2$. In the earlier experiments which resulted in the production of this interesting substance, cryolite, the natural fluoride of sodium and aluminic aluminium, $6NaF . Al_2F_6$, was fused for several hours in a gas-carbon crucible along with a quantity of metallic aluminium. Air was rigidly excluded during the fusion, a current of hydrogen being led through the crucible by means of tubes inserted through the air-tight cover. The carbon crucible was protected from the direct flame of the furnace by means of an outer one o platinum. Under these circumstances the metal dissolved either wholly or in part, depending upon the amount present, in the melted cryolite; a little carbide of aluminium was at the same time formed and disseminated throughout the mass, especially aggregating near the surface, in minute particles, accompanied by small globules of the metal itself. After separation of these particles by various means as completely as possible, it was found that about half as much aluminium had entered into combination as that originally contained in the cryolite, indicating the probability of the course of the reaction being as follows: $6NaF . Al_2F_6 + Al = 3(2NaF . AlF_2)$. It was afterwards found, however, that by substituting a polished wrought-iron crucible for the carbon one, a product was obtained perfectly free from these particles, carbide of aluminium being no longer a possible product of the reaction, and the excess of metallic aluminium forming an alloy with the iron upon the walls of the crucible above the fused fluoride. Hydrogen was led through as before in order to exclude oxygen. 58 grammes of cryolite were fused for $5\frac{1}{2}$ hours with 29·5 grammes of the metal; at the end of the operation the homogeneous white substance formed at the bottom of the crucible on cooling was submitted to analysis, with results which entirely confirm the above supposition, the numbers corresponding to the formula $2NaF . AlF_2$. The new compound, which in outward appearance very much resembles cryolite, must therefore be considered as sodium aluminious fluoride. It may be remarked that all three elements were estimated, the sodium and aluminium in the ordinary manner, as chloride and hydrate, while the fluorine, which was found most difficult to determine satisfactorily and required prolonged treatment, was eventually weighed in the form of calcium fluoride. Now that an aluminious salt has at last been obtained, it is to be hoped that further attempts may follow, having for their object the formation of other compounds corresponding to the well-known salts of ferrous iron.

REFERRING to the discovery, last year, at Sandnaes, near Stavanger, in Norway, of enormous deposits of infusorial earth, at the time communicated by Lord Salisbury to the Royal Society, Prof. P. Waage, the well-known Norwegian chemist, is of opinion that this earth should be suitable for the preservation of food, after having been subjected to a process of intense heating, whereby all organic matter should be destroyed. Prof. Waage thinks that sterilized infusorial earth would be very much better as a means of preservation than boric acid, &c., now used in the preservation of fish.

LAST autumn, a family of the hazel-mouse, *Myoxus avellanarius*, was discovered in a wood at Slagelse, in Denmark. It is said that the animal had never before been found in that country. The mice had made their nest—ball-shaped in appearance—of grass and leaves between the branches of a tree, 6 feet above the ground. It had a circular entrance-hole at the side. Two of the animals are now hibernating in the possession of a farmer.

THE Museum of the Christiania University has just been enriched with a Runic stone, hitherto unknown, found in the Romsdal. The writing is in older Runic characters, and very clear, but parc of the stone is missing. It is believed to have been a memorial stone. Some years ago, a similar stone was found in the same locality, but the Runes are illegible.

THANKS to strict preservation, and to the fact that the inhabitants are realizing the value of the bird, the eider has greatly increased in number in Iceland during recent years. The people do all in their power to attract the bird to their property. Among these attractions are bells worked by the wind or by water, the hanging up of dress material of a glaring colour, and the keeping of brightly-coloured fowls. A Society has been formed for the granting of premiums for the killing of animals preying upon the eider, and, last year, 1155 such prizes were awarded.

THERE has been a very marked increase in the number of visitors to the South Kensington Museum during the last year, the numbers rising from 788,412 in 1887 to 897,225 in 1888. But this increase of 108,813 is quite put in the shade by that of 500,582 at the Bethnal Green Museum, which, in its total of 910,511 for the past year, has, as will be seen, distanced the parent institution. This great influx of visitors, more than double that of the previous year, was, no doubt, in great measure due to the exhibition there of Her Majesty's Jubilee presents after they had been shown to the West End at St. James's Palace. But some part of the increase must be attributed to the fine collection lent by the Hon. W. F. B. Massey Mainwaring. The increase in numbers at South Kensington was not confined to the main Museum, but extended to the separate collections—the Science Museum and the India Museum—which are in the galleries at the west side of Prince's Gate, and which are not open in the evening, as are, on three evenings in the week, the collections in the main building on the east of Prince's Gate, and the Bethnal Green Museum. The numbers visiting the Science collections increased from 177,465 in 1887 to 258,796 in 1888, notwithstanding the fact that the galleries have been severed by the new road cut across the Horticultural Gardens, while the visitors to the India Museum increased from 116,574 to 152,911. The numbers of visitors are taken in all cases by turnstiles.

WE have received several letters on "Hares Swimming." Mr. G. H. Kinahan writes that he believes the phenomenon to be not uncommon. "When I was stopping at Inver Lodge, Co. Galway," he says, "the keeper told me a hare had a nest with three young ones on a small island in the lake, and that it left them most of the day-time. I went with him to the island, which was about 30 yards in diameter, and about 100 yards from the shore, and after searching it most carefully, we could only find the three leverets. Sheep are animals that in general keep out of water, yet in Connemara I have seen them quite naturally swimming a river or even a lake." "E. H." writes:—"I was by the little River Arun below the old mill at Pulborough one day, when I saw a hare quietly cantering down the opposite field towards the river. A bank hid the actual crossing of the river from me; but when the hare emerged from the water into the field in which I was standing, I was amused to see the dog-like fashion in which it stood and shook off the moisture, scattering the spray far and wide, before resuming its leisurely

canter. The act had the air of being habitual." Mr. G. Plarr, writing from Tunbridge, tells us of a hare which he saw many years ago while he was walking along a mill-stream in Elsass. The hare was being chased by some boys in a meadow on the opposite side of the stream. It disappeared in the water, and emerged on the side on which Mr. Plarr was walking. Without stopping to shake the water away, it made off with great speed. The creature presented a strange appearance, its head seeming to be large beyond all proportion to its body. This was, of course, due to the fact that the head had been kept dry above water, while the rest of the body had been immersed.

MESSRS. DULAU AND CO. have sent us a catalogue of zoological and palæontological works, including works on Echinodermata, Vermes, and Crustacea.

IN the letter on "Alpine Haze," by Antoine d'Abbadie (p. 247, lines 13 and 17 from the top), *for* "earth-haze" *read* "earth-ashes," and *for* "Ventouk" *read* "Ventoux."

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mrs. Henderson; a Rhesus Monkey (*Macacus rhesus* ♂) from India, a Brown Capuchin (*Cebus fatuellus*) from Brazil, deposited; two White Ibises (*Eudocimus*, sp. inc.) from Central America, purchased; a Rufous-necked Wallaby (*Halmaturus ruficollis* ♂) from New South Wales, received in exchange.

OUR ASTRONOMICAL COLUMN.

DISCOVERY OF A NEW COMET.—A faint comet was discovered on January 14, at 18h. 4·7m., by Mr. W. R. Brooks, of Geneva, New York. Its position at the time of discovery was R.A. 18h. 4m. 0s., Decl. 21° 20′ S. The comet was moving rapidly towards the west.

MINOR PLANETS.—Herr Palisa at Vienna discovered a minor planet on January 4, which may possibly be Siwa, No. 140. Should it be a new planet, it will be No. 282, and Herr Palisa's sixty-ninth discovery. Three minor planets, all discovered by Herr Palisa, have recently been named. No. 278 has been called Paulina; No. 279, Thule; and No. 280, Philia.

THE OBSERVATORY OF TOKIO.—An Astronomical Observatory has just been instituted at Tokio, Japan, by the combination of the astronomical portions of the old Naval Observatory and of the Home Office, together with the Astronomical Observatory of the Imperial University. The site of the old Naval Observatory has been selected for the new institution, the meteorological portion of the former having been transferred to the Central Meteorological Observatory of the Home Office. The principal instruments of the new Observatory are a Repsold meridian instrument of $5\frac{1}{4}$ inches aperture; a transit-circle, by Merz and Repsold, of 5 inches aperture; and two equatorials, the one by Troughton, of 8 inches, and the other by Merz, of $6\frac{1}{2}$ inches aperture. The staff of the institution has not yet been fully organized, but Prof. H. Terao has been appointed Director, and has commenced regular observation. The approximate position of the Observatory is—longitude E. of Greenwich, 139° 44′ 30″ 3; N. latitude 35° 39′ 17″·5.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 JANUARY 27—FEBRUARY 2.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 27

Sun rises, 7h. 47m.; souths, 12h. 13m. 5·4s.; sets, 16h. 39m.: right asc. on meridian, 20h. 40·9m.; decl. 18° 19′ S. Sidereal Time at Sunset, 1h. 8m.

Moon (New on January 31, 9h.) rises, 3h. 54m.; souths, 8h. 22m.; sets, 12h. 43m.: right asc. on meridian, 16h. 48·9m.; decl. 19° 10′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	8 30	13 24	18 18	21 52·5	13 19 S.
Venus ...	9 18	15 7	20 56	23 35·8	2 57 S.
Mars ...	9 6	14 34	20 2	23 2·4	7 2 S.
Jupiter ...	5 29	9 24	13 19	17 51·9	23 5 S.
Saturn ...	17 24*	0 56	8 28	9 21·9	16 37 N.
Uranus...	23 33*	4 56	10 19	13 22·3	7 59 S.
Neptune..	11 39	19 22	3 5*	3 50·9	18 25 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Jan.	h.	
28	9	Jupiter in conjunction with and 1° 42′ south of the Moon.
30	14	Mercury at greatest elongation from the Sun 18° east.
Feb.		
1	15	Mercury in conjunction with and 4° 24′ north of the Moon.
2	22	Mercury at least distance from the Sun.
2	23	Mars in conjunction with and 3° 57′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Jan. 28,	20 31	*m*
			Feb. 2,	20 11	*m*
λ Tauri...	3 54·6	12 11 N.	Jan. 29,	0 11	*m*
			Feb. 1,	23 3	*m*
η Geminorum ...	6 8·2	22 32 N.	Jan. 30,		*m*
ζ Geminorum ...	6 57·5	20 44 N.	,, 28,	3 0	*m*
			Feb. 2,	3 0	*M*
R Canis Majoris...	7 14·5	16 11 N.	Jan. 28,	19 8	*m*
		and at intervals of		27 16	
U Monocerotis ...	7 25·5	9 33 S.	Feb. 2,		*m*
S Cancri	8 37·6	19 26 N.	,, 1,	20 7	*m*
T Vulpeculæ ...	20 46·8	27 50 N.	Jan. 29,	22 0	*m*
			,, 31,	0 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 28,	17 40	*m*
		and at intervals of		36 0	
W Cygni	21 31·9	44 53 N.	Feb. 2,		*M*
δ Cephei	22 25·0	57 51 N.	,, 2,	22 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near τ Leonis	169	4 N.	Very swift.
,, α Coronæ	235	26 N.	Very swift; streaks.

GEOGRAPHICAL NOTES.

THE International Geographical Congress will be held at Paris from August 5 to August 10 next. The Geographical Societies of London, Berlin, Leipzig, Manchester, Edinburgh, New York, Melbourne, Lisbon, Antwerp, and Milan, and many French Societies will be represented at the meeting.

M. EUGENE MARKOW sends to the French Geographical Society an interesting account of his recent ascent of Mount Ararat. He and his companion, after passing the night among the rocks at a height of 13,000 feet, began their ascent on August 13, at 5 a.m. Soon they passed a conical rock which rises on the south-east face of Ararat, and here M. Markow places the limit of perpetual snow on the mountain. From the base of the rock extends in an opposite direction a broad plain of *névé*, which reaches the summit at a slope of 35°. Underneath this *névé* was heard the sound of a stream formed of the melted ice. At the height of 14,800 feet, M. Markow found among the rocks a *Coccinella septempunctata* of a very bright red. At 15,500 feet, some flowers were found on a small sandy eminence. At 2 p.m. the party reached the foot of the sacred summit of the mountain. This summit presents a vast extent of snow, separated into two by a precipice commencing on the north-east side, and getting broader and deeper as it reaches the south-east. Part of the right summit is almost entirely free from snow, and is covered with small stones. The left summit, on the north-west, presents a plateau covered with snow, but having a small elevation in the middle. It is much larger than the right summit.

A pyramid is being raised on the summit of the mountain, which it is hoped will dissipate the superstition among the natives below that no one can ever reach the top.

CAPTAIN TRIVIER, a French naval officer, has left France to undertake an expedition across Africa. One of his chief objects will be the exploration of the Lualaba and its tributaries, and more especially the Lukuga, the outlet of Lake Tanganyika, about the real nature of which there has been so much controversy. Captain Trivier will follow the Lukuga to the lake, and make a careful report on its actual condition. Captain Hore, who has been more than ten years on the lake, has just returned home. He states that during the whole period of his stay on the lake it has steadily decreased in size; its level has fallen quite 15 feet, but the Lukuga still flows out with a rapid current. This it will continue to do until its muddy bed is worn down to the rock, when it will cease to be an outlet of Tanganyika. Captain Hore seems to think the lake will go on decreasing in size. Captain Trivier will cross to Ujiji and endeavour to make his way to the east coast at Bagamoyo.

THE first number is to hand of the *National Geographic Magazine*, the organ of the recently founded "National Geographic Society" of the United States. The work of the Society, of which all the leading officers of the U.S. Survey are members, promises to be of much higher scientific value than that of the American Geographical Society of New York. The Society has 200 members. Among the articles in this number are "Geographic Methods in Geologic Investigations," by Mr. W. M. Davis; "Classification of Geographic Terms by Genesis," by Mr. W. J. McGee; "The Survey of the Coast," by Mr. H. G. Ogden; and the "Survey and Map of Massachusetts," by Mr. Henry Gannett.

CAPTAIN WAHAB reports as follows regarding the survey work accomplished by himself and one sub-surveyor while with the Hazara field force:—"Up to the end of the Ahazai country we have a complete survey extending a good way west of the Indus, and a certain amount of reconnaissance work in the Chagarzai country up to about Judbai. North-east of the Black Mountains we have surveyed from Nandihar (the limit of this survey in the 1868 expedition), north to the range beyond Allahi, and west to the hills overlooking the Indus. We have fixed the course of the river up to say 15 miles north of Thakot, and I have sketched, on the ¼-inch scale, as much as possible of the country between the Indus and the Swat Valleys, what I could see from the Chel Mountain and the Ghorapher Pass. I have made three stations, and fixed a number of points in the lower ranges between the snowy peaks fixed by the Great Trigonometrical Survey and our frontier, which I hope may be useful on future occasions. While I was triangulating on the top of Chel, Imam Sharif went down the hill to Pokal, for the day, and got in most of the Allahi Valley. There is a gap in the survey of the Indus Valley from the bend west of Thakot down to Judbai, which cannot be seen without going into the Chagarzai country, but even if we do not go, I have got the course of the river practically fixed within about half a mile one way or the other."

ELECTRICAL NOTES.

F. KOHLRAUSCH has just made a fresh determination of the ohm. He makes it equal to 106·32 centimetres of mercury 1 square millimetre in section.

PROF. ROWLAND has made preparations to repeat his classical Berlin experiment by which he demonstrated the fact that a static electrical charge in motion acted like a current. He is going to use higher speeds and higher electrification, and it is therefore hoped that he will get accurate quantitative determinations.

AT Paisley, an electric discharge, which seriously damaged a chimney and its defective lightning-conductor, also killed a quantity of fish that were in a pond close by in which the conductor was earthed. When will people take the precaution to examine their lightning protectors?

DUBS (*Centralblatt für Electrotechnic*, 1888, No. 28) has shown that a strong blowpipe or oxyhydrogen flame from one carbon to another sets up an E.M.F. which would fully account for the opposing E.M.F. of the arc.

A NEW mode of regulating dynamos for constant current and constant potential has been devised by the Waterhouse Company in the United States by means of a *third brush* slightly in advance of the positive brush, and an external variable shunt circuit which can be adjusted automatically or by hand. The desire to evade patents has at least one merit—it exercises the faculty of invention and stimulates design. The *Electrical Engineer* of New York (December 1888) has an excellent paper by Mr. Caldwell on this third brush.

IT is sometimes asserted that the unit of work—the *erg*—is too small to be of any use, but Prof. Langley has shown that the perception of the colour crimson is produced by an expenditure of energy upon the retina, which can be represented by 10^{-13} horse-power, or 0·001 of an erg; while the sensation of green is due to 0·00000001 of an erg.

HELMHOLTZ has shown that if an invisible jet of steam be electrified or heated it becomes visible with bright tints of different colours according to the potential or the temperature.

DR. GORE, F.R.S., has submitted to the Royal Society a new instrument of research, which he thus describes:—"Take two small glass cups containing known volumes of distilled water. Form two voltaic cells of them by means of strips of stout wires of unamalgamated zinc cut from the same piece, and two small sheets of platinum, also cut from the same piece. Connect them together in series to a sufficiently sensitive galvanometer, so that the currents from the two cells oppose each other, and produce no visible deflection of the needles. This arrangement constitutes a 'voltaic balance,' and is extremely sensitive to change of chemical composition of the liquid in one of the vessels. Make an aqueous solution of known strength of the substance, and add it in sufficiently small quantities at a time to the water in one of the cups until the needle of the galvanometer visibly commences to move, and note the proportion of the substance and of water then contained in that vessel. As the amount of energy required to move the needle is the same in all cases, the different numbers thus obtained with different substances represent the relative amounts of voltaic energy of those substances. And as each substance and mixture of substances gives a different number, it is possible by this method to detect substances, to ascertain the degrees of strength or concentration of liquids, to ascertain whether a substance contains a soluble impurity, &c. The method also is in many cases an extremely sensitive one."

PROF. J. J. THOMSON (R.S., January 17, 1889) has examined the screening influence of conducting plates upon alternating currents of great frequency, and has deduced thereby the resistance of electrolytes and of graphite. He shows that the screening effect depends on the conductivity and thickness of the plate and upon the frequency of the alternations. The secondary induced currents are confined to the skin of the plate next to the primary, the thickness of this skin varying inversely with the conductivity of the plate and the frequency of the currents. Thus a thin plate of badly conducting material will be efficient with currents of great frequency, such as those of the rate 10^8 per second; while a thick plate of the best conducting material will not be sufficient to screen off currents of low frequency, such as those with a rate below 10^2 per second. Thus to measure the resistance of electrolytes it is necessary to have vibrating electrical systems such as those examined by Hertz, whose frequency is of the former class; and if two different plates produce the same screening effect, their thickness must be proportional to their specific resistances. He supports Maxwell's theory that the rate of propagation of electrostatic potential is practically infinite, a point called in question by Hertz; and he agrees with Hertz that the rate of propagation of electro-dynamic action is finite and measurable. He shows that the rate of propagation of an electro-magnetic disturbance through a metallic conductor and through the surrounding dielectric is the same, and this differs from one of Hertz's conclusions. But he also shows that this is not so when the conductor is a dilute electrolyte or a rarefied gas. In such a case there would be interferences and standing vibrations. Hence the striæ in so-called vacuum-tubes. He also concludes that the relative resistance of electrolytes is the same when the current is reversed a hundred million times a second as for steady currents.

SOMEONE in the United States has proposed to call static electricity "amberism." It is a good analogue to "galvanism" and to "magnetism." It would be well to introduce some term to relieve the word "electricity" from the dreadful abuse to which it is now subjected. The Board of Trade in their draft Provisional Orders are using it in three distinct and different

senses: (1) for electrical energy, which is measured in *watts*; (2) for electric currents, which are measured in *amperes*; and (3) for electrical quantity, which is measured in *coulombs*.

JOÉ has discovered that the resistance of cobalt in a magnetic field is *increased* in the direction of lines of force, and *diminished* in directions at right angles to them.

STAR NAMES AMONGST THE ANCIENT CHINESE.

IN two recent numbers of the *Chinese Review* (vol. xvi. Nos. 5-6) the well-known scholar, Dr. Joseph Edkins, writes on the subject of star naming amongst the ancient Chinese. He says that there are two great periods of star naming in ancient China, the first being about B.C. 2300, and the second during the Chow dynasty from B.C. 1120 to B.C. 220. The real beginning of Chinese astronomy is, in Dr. Edkins's opinion, to be found in the period preceding B.C. 2300, about which date, by command of the Emperor Yan, the observation of the meridian stars was made. Amongst primitive Chinese observers our Scorpion was a dragon, Aquarius a serpent or tortoise, Taurus a tiger, and Leo a bird. These figures were, however, larger than our zodiacal signs; for instance, the chief portion of Virgo, Leo, and Cancer would form the Red Bird. At that remote period we find that Chinese astronomers divided the heavens into four large sections, and twenty-eight small groups or constellations. The former, the large ones, are all animals, and are arranged from east to west, while the constellations are arranged from west to east. There were seven eastern constellations forming the Green Dragon—which comprised the stars in Libra, Scorpio, and Sagittarius; seven southern constellations, the Red Bird, or *Feng-hwang*—comprising Cancer, Leo, and Virgo; seven western constellations, the White Tiger—made up of Aries, Taurus, and Gemini; and the seven northern constellations, the Dark Warriors—or the Serpent or Tortoise. Each group, whether large or small, had its Chinese name. The Red Bird or Pheasant is the constellation of summer or the south; the Dragon, of spring or the east; the Tiger, of autumn or the west; and the Serpent or Tortoise, of winter or the north. Since the Great Bear points to Spica Virginis, the Chinese astronomers made the group led by Spica the group of spring. Another reason for thus making Spica the gate of the year is, perhaps, to be found in the fact that the Babylonians, from whom the Chinese probably got their astronomy, for a long time regarded Scorpio as the first of the signs. This is, of course, a mere guess, for we cannot, after this lapse of time, tell how much of the astronomical knowledge of the Chinese is derived from external sources. On the probable Babylonish origin of some of the astronomical knowledge of the Chinese, Dr. Edkins says:—"The contests of the early Buddhists with the worshippers of fire show that the Persian religion was propagated in India during and after the sixth century before Christ, and the eagerness with which the Hindus adopted the Greek astronomy after Alexander's invasion of India, as well as our knowledge of the fondness of the Buddhists for astrology, make it probable that Babylonian ideas on the stars were familiarly known in ancient India, during the period when they became popular in China. The resemblance of the cosmogony of the laws of Manu to that of the Babylonians seems to support strongly the correctness of the statement that Babylonian astrology was accepted at the same time in ancient India and in ancient China." With regard to the names of the four zodiacal signs, they are, as we have seen, those of animals, and it is peculiar that they are all Chinese animals but the Dragon, and it is not known that any species of dragon ever existed in China. In the naming of the constellations a wider field is included. Thus, the following are found: parts of the body, as heart, stomach, lips; buildings, a house, a wall, a well, a tower; articles of daily use, a peck measure, a net, a carriage; animals, K'wei K'ien (a humped boar leading a cow to sacrifice); adjectives and numerical groups, &c. From these names it appears that the origin of the appellations was popular rather than Imperial. In B.C. 1144, Wen Wang began to write the treatise called "Yi King." The adoption by Wen Wang of red as the Court colour of the Chow dynasty, and the fact that his son introduced five colours into the sacrifices, show that the Babylonish doctrine of the five colours and the five planets was known in China at that time. There are, however, variations in the colours. Thus, Mars is red in both China and Babylon; Jupiter, orange in Babylon, blue or purple in China; Venus, yellow in Babylon, white in China; Mercury, blue in Babylon, black in China; Saturn, black in Babylon, yellow in China. The "Yi King" shows that the stars were divided into four groups from the earliest times, for the Dragon and the Tortoise lie at the root of all the divination of that work; and the Tiger and Red Bird are respectively assigned to the west and the south. Shortly after the date of "Yi King" we find the following points mentioned: the cycle of twelve years, dependent on a revolution of Jupiter; the twelve hours into which the horizon is divided by the pointing of the Bear; the cycle of ten days; the cycle of twenty-eight constellations; the four seasons; the sun, moon, and planets. Astrology was, of course, implicitly believed in; in fact, the end and aim of all ancient Chinese astronomy was astrology. The conjunction of the sun and moon controlled the good and bad luck of the Empire, and particular stars foretold the fortunes of the various portions of the Empire, for each province had its presiding star. During the Chow dynasty—that is, after B.C. 1120—many constellations are named. Thus the fifth Emperor ordered a group of stars in Cepheus to be called Tsau-fu, after his favourite charioteer. Wang-liang was also a charioteer about B.C. 470, and his name was given to a number of stars in Cassiopeia. The virtues of a duke of the Tsi kingdom who died in B.C. 488 were so great that a star was called after him. Unlike the old names, all of which seemed to denote a popular origin, those named during the Chow dynasty show their Imperial origin. Thus several stars in Leo were styled Wu-ti-tso—that is, "throne of the five emperors." During the second century before the Christian era, Chinese astronomers pointed out the five emperors. The chief ruler of Heaven is the ancient pole, the star Tai-yi, 22° from our present pole. The seven stars of the Great Bear are the Government—rulers of the sun, moon, and five planets. The palace of the heavenly emperor is bounded by the oval formed of the fifteen stars of Draco, amongst which is Tai-yi. At the back of the bear is the group Wen Ch'ang Kung, "the palace of literature brilliantly spread abroad," the favourite object of the adoration of the *literati*. The abode of the eastern emperor is in Scorpio. The group containing Antares is Ming-t'ang, the council-hall of the emperor, where he give laws to his subjects. The adjoining stars are the sons of the emperor. The palace of the emperor is Arcturus, and the two large stars in Centaur to the south of Sagittarius form the south gate of his dominions. In Cancer and Leo lies the residence of the southern emperor. One group is the palace of the sun, moon, and planets, and surrounding this group is a guard of twelve feudal barons who keep the throne of the five emperors. Between Procyon and Regulus, and between the ecliptic and equator, there is a group in Hydra called the willow-branch, which rules over planets, and forms the beak of the Red Bird. The constellations of the Seven Stars adjoin this, and form the neck of the Red Bird: its crop is the kitchen of the palace; Hydra forms the bird's wings; the constellation Yi is the imperial hotel where visitors at the palace are accommodated; the constellation Corvus finishes the shape of the Red Bird, and is the last in the zodiac. The seven western constellations—that is, those made up of Aries, Taurus, and Gemini—are "the lake of fulness," "the five reservoirs of heaven," "the home of the five emperors." Hyades is "the announcer of invasion on the border." Later on—that is, probably about the second century—the stars are grouped into three principal sections, the first section containing the circum-polar stars, the second stars in Leo and Virgo, the third twenty-two stars in Serpens, Hercules, and Ophiuchus, the latter being said to be feudal rulers paying homage to the Emperor. The whole history, in fact, of Chinese astronomy is full of this comparison of the state of the kingdoms on the earth with the heavenly bodies. Thus, under the Tsin dynasty, the pole star is the abode of the supreme ruler. "The circum-polar stars form his court. Their name as a whole is the 'purple subtle inclosure.' The stars selected to represent the emperors of the five colonies" (*i.e.* blue, red, yellow, white, and black) "were Denebola and four others in Leo. They are surrounded by twelve groups, which have received names of office and rank representing together the court of an earthly emperor. This inclosure is the court, especially, it is said, of the yellow emperor, whose essence is called Han-shu-nieu. The four remaining colours are near him. The blue emperor is Ling-wei-yang. The red or south emperor is Chi-piau-nu. The white emperor of the west is Pe-chan kü, 'the white beckoning mason's rule.' The north or black emperor is

Hie-kwang-ki, 'mark of combining light.' Besides this palace in Leo and Virgo, there is another, Tien-shi-yuen, 'inclosure of the heavenly market.' It is not far to the north-east of Scorpio. It is the serpent in our astronomy. Within the brilliant circle of the serpent is a star called 'court of the western heaven.' There is also a bright star, α Herculis, which is called 'emperor's throne.' The twenty-two stars in the Serpent are named after the States into which China was formerly divided."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Among the numerous lectures on physics and chemistry this term, we note those of Prof. Liveing, on spectroscopic chemistry; Prof. Dewar, on physical chemistry; Mr. Pattison Muir, on chemical affinity; Mr. Robinson, on agricultural chemistry; Mr. Heycock, on chemical philosophy; Prof. Thomson, on electricity and magnetism, and on the kinetic theory of gases; Mr. Shaw, on thermodynamics and radiation; and Mr. Wilberforce, on dynamo electric machines (continuous current generators and motors). Prof. Stuart lectures on theory of structures.

Prof. Foster continues his elementary course of physiology; Dr. Lea his chemical physiology; and Mr. Langley his advanced histology and physiology.

In zoology, Prof. Newton lectures on the geographical distribution of Vertebrates. Mr. Sedgwick and Mr. Darwin conduct the large class of elementary biology. Mr. Gadow's course is on the morphology of the Amniota (recent and extinct). Mr. Sedgwick, Mr. Harmer, and Mr. Weldon continue their classes on the Invertebrata.

Mr. Darwin lectures on the physiology of plants (advanced), Mr. Gardiner has a general elementary course, Mr. Vaizey lectures on the morphology and classification of Cryptogams, and Dr. Hicks on elementary botany.

The lectures on geology are divided thus: Prof. Hughes, geology of a district to be visited at Easter; Mr. Marr, principles, and geology and scenery; Mr. Harker, petrology; Mr. Roberts, palæontology; Mr. Seward, palæobotany.

The principal mathematical lectures are the following: Prof. Stokes, semi-convergent series involving powers of a complex variable; Prof. Cayley, analytical geometry; Prof. Adams, lunar theory; Mr. Pendlebury, projective geometry; Mr. Glazebrook, hydrodynamics (waves and sound); Mr. Hobson, spherical and cylindrical harmonics; Mr. Larmor, geometrical optics and electro-magnetism; Mr. Forsyth, modern algebra (binary forms); Dr. Ferrers, elliptic functions; Dr. Besant, analysis; Mr. H. M. Taylor, higher plane curves; Mr. Webb, dynamics (elasticity and viscosity); Mr. Stearn, hydrodynamics (multiply-connected velocity-potentials and vortices); Mr. Herman, hydrodynamics (viscous and gravitating fluids).

An examination will be held at Gonville and Caius College on March 15 for one Shuttleworth Scholarship, value £60 per annum for three years. Candidates must be medical students of the University of not less than eight terms standing. In the case of candidates not already scholars of the College, the examiners may recommend at the same time for a foundation scholarship. Further particulars may be obtained from the tutors.

SCIENTIFIC SERIALS.

American Journal of Science, January.—The history of a doctrine, by S. P. Langley. This is the address delivered last year to the American Association for the Advancement of Science, here published complete with the notes that have not hitherto appeared. Its object is to show the steady progress of scientific truth, as illustrated by the history of the undulatory and corpuscular theories of light from the time of Descartes, Boyle, and other precursors of Newton down to the present day, when the identity of radiant light and heat as forms of motion, or as different effects of radiant energy, has been finally established.—Description of the new mineral beryllonite, by Edward S. Dana and Horace L. Wells. This is a new phosphate of sodium and beryllium discovered in 1886 by Mr. Sumner Andrews near Stoneham, Maine, the same district that has already yielded fine specimens of phenacite, herderite, and other rare minerals. It occurs mostly as a crystal in a fragmentary state, of small size and seldom well formed, but remarkable for the number of planes they present, eight or more distinct planes being frequently presented in each zone on a single crystal. Twins are common, leading to many curious variations of form. The crystals are colourless, or slightly yellowish, and transparent, with specific gravity 2·845, and hardness 5·5.—The iron ores of the Penokee-Gogebic series of Michigan and Wisconsin, by C. R. Van Hise. The author's recent explorations of this region confirm Prof. Irving's conclusion that the original rock of the iron-bearing formation is a cherty iron carbonate from which the various phases of rock and the ore found in it have been produced by a complex series of alterations. The iron ore is a soft, red, somewhat hydrated hæmatite, more or less manganiferous, and mostly very friable.—A quartz-keratophyre from Pigeon Point and Irving's augite-syenites, by W. S. Bayley. The remarkable bright red rock of Pigeon Point, Minnesota, is here studied in its various phases, with the general result that the sections described by Irving as augite-syenites are partly identical with the typical red rock itself, and partly the same in all essentials as the formations which have been called its intermediate varieties. The space between the fresh olivine-gabbro and the typical quartz-keratophyre is occupied by a series of rocks exhibiting a gradual transition between the heavy dark basic rock and the light red keratophyre.—On the occurrence of hanksite in California, by Henry G. Hanks. This anhydrous sulphate of soda has hitherto been found in limited quantities amongst the various borax fields of California. But the author's researches tend to show that it exists in great abundance, and that it plays an important part in the metamorphoses that produce gay-lussite, thinolite, and perhaps borax.—Further papers on Mount Loa are contributed by James D. Dana and the Rev. E. P. Baker, bringing its history down to July 1888.—H. L. Wells and S. L. Penfield contribute notes on the new mineral sperrylite.

American Journal of Mathematics, vol. xi. No. 2 (Baltimore, January 1889).—The number opens with an instalment of a memoir entitled "Remarque au sujet du théorème d'Euclide sur l'infinité du nombre des nombres premiers," by J. Perott (pp. 99–138). A footnote supplies bibliographical information as to previous memoirs on the same subject.—Next, Prof. Cayley writes on "The Theory of Groups" (pp. 139–57), a subject he has pretty largely written upon before, and to which his attention has been recalled by the section, in Mr. Kempe's Philosophical Transactions memoir "On the Theory of Mathematical Form," entitled "Groups containing from One to Twelve Units." The paper is largely illustrated by what the author styles "colour groups."—Mr. A. E. H. Love discusses "Vortex Motion in certain Triangles" (pp. 158–71), by a method explained by Dr. Routh in a paper in vol. xii. of the London Mathematical Society's Proceedings.—Another hydrodynamical paper follows, by Mr. Basset, "On the Steady Motion of an Annular Mass of Rotating Liquid" (pp. 172–81), wherein he follows up previous work in the line of Poincaré's and Prof. G. H. Darwin's recent investigations of the figures of equilibrium of rotating masses of liquid. The case considered is for an approximately circular cross-section and for rotation under the influence of its own attraction about an axis through its centre of inertia, which is perpendicular to the plane of its central line.—A paper, by Sophus Lie, "Die begriffe Gruppe und invariante" (pp. 182–86), is reprinted from the *Berichte der k. Sächs Gesellschaft der Wissenschaften*, August 1887.—A short note, by E. Picard, "Sur les formes quadratiques binaires à indéterminées conjuguées et les fonctions fuchsiennes" (pp. 187–94), closes the number. The method employed is that used by Poincaré in his memoir on fuchsian functions (*Journal de Jordan*, 1887).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Meteorological Society, January 16.—Dr. W. Marcet, F.R.S., President, in the chair.—The Report of the Council showed that a large amount of work had been done during the past year, and that considerable progress had been made in the investigation of one of the most interesting and hitherto neglected branches of meteorology, viz. thunderstorms. Forty-nine new Fellows were elected last year, the total number on the books now being 525.—After the Report had been

adopted, the President delivered an address on "Fogs," which he illustrated by a number of interesting lantern slides. Fogs and clouds are one and the same thing. A cloud is a fog when entered into, and a fog seen from a distance, suspended in the air, becomes a cloud. After describing the various kinds of fog—*e.g.* river, sea, Newfoundland, radiation, town, &c., fogs—Dr. Marcet referred to London fogs. Dr. Tyndall has accounted for them by assuming each particle of condensed vapour to be covered by coal-smoke. These fogs usually accompany a high barometer, and are frequently dry in their character. It is a well-known fact that cold air on the tops of hills, being heavier than the air below, slides down the slopes, so that the lower parts of the hill-sides are actually colder than the plains at some distance from the hills. Now, London, in the Thames Valley, is surrounded by hills—to the north, Highgate, Hampstead, and Harrow; in a westerly direction, Putney and Wimbledon; and in a more southerly direction, Clapham and Sydenham. The air is colder on these hills than in London with its millions of inhabitants, its coal-fires and factories, hence it is heavier, and will have a great tendency to slide down the hills towards the town and the river. Should the air in town be on the point of saturation, and the cold air from above saturated with vapour, it is obvious that the increased cold from above will produce a precipitation of moisture, and it will come to pass that a fog is produced. If the hill-tops be not only colder than the air below, but enveloped in a fog, it stands to reason that the fog below will be all the denser, and especially in the neighbourhood of water, such as the River Thames and the ornamental waters in the parks.—The following gentlemen were elected the officers and Council for the ensuing year:—President: Dr. Wm. Marcet, F.R.S. Vice-Presidents: Francis Campbell Bayard, Henry Francis Blanford, F.R.S., William Ellis, Richard Inwards. Treasurer: Henry Perigal. Trustees: Hon. Francis Albert Rollo Russell, Stephen William Silver. Secretaries: George James Symons, F.R.S., Dr. John William Tripe. Foreign Secretary: Robert Henry Scott, F.R.S. Council: Edmund Douglas Archibald, William Morris Beaufort, Arthur Brewin, George Chatterton, William Henry Dines, Frederic Bernard Edmonds, Charles Harding. Baldwin Latham, Capt. John Pearse Maclear, R.N., Edward Mawley, Henry Southall, Dr. Charles Theodore Williams.

Zoological Society, January 15.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of December 18 8, and called attention to the young Chimpanzee purchased of Mr. Cross, of Liverpool, on December 6 (see NATURE of January 10, p. 254).—A letter was read from Heer F. E. Blaauw, of Amsterdam, containing an account of the development of the horns of the White-tailed Gnu as observed in specimens bred in his menagerie.—Prof. Newton exhibited a specimen of *Pennula millsi*, Dole, brought from the Sandwich Islands by Mr. S. B. Wilson, remarking that it seemed to be identical specifically with *Rallus obscurus*, Gmelin, a species which has not been lately recognized.—Prof. Bell made some remarks on the question of the food of *Bipalium*.—Canon Tristram made some remarks on a specimen of *Emberiza cioides*, a Bunting of Siberia, of which a specimen was believed to have been obtained in this country at Flamborough in October 1887.—Prof. F. Jeffrey Bell read a note on the Echinoderm fauna of the Bay of Bengal.—Mr. F. E. Beddard and Mr. Frederick Treves gave an account of the anatomy of the Sumatran Rhinoceros as observed in two specimens of this animal that had lately died in the Society's Gardens. The muscular anatomy of the limbs of this Rhinoceros was especially treated of.—Prof. Newton read a paper on the breeding of the Seriema (*Cariama cristata*) in the Society's Gardens.

Entomological Society, January 16.—Fifty-sixth Anniversary Meeting.—Dr. D. Sharp, President, in the chair.—An abstract of the Treasurer's accounts, showing a balance in the Society's favour, was read by Mr. Osbert Salvin, F.R.S., one of the auditors; and Mr. H. Goss read the Report of the Council. It was announced that the following gentlemen had been elected as officers and Council for 1889:—President: The Right Hon. Lord Walsingham, F.R.S. Treasurer: Mr. Edward Saunders. Secretaries: Mr. Herbert Goss and the Rev. Canon Fowler. Librarian: Mr. Ferdinand Grut. And as other members of Council, Mr. Henry W. Bates, F.R.S., Capt. H. J. Elwes, Mr. William H. B. Fletcher, Mr. F. DuCane Godman, F.R.S., Prof. Raphael Meldola, F.R.S., Dr. P. B. Mason, Mr. Osbert Salvin, F.R.S., and Dr. D. Sharp.—Dr. Sharp, the outgoing President, then delivered an address, for which a vote of thanks to him was moved by Capt. Elwes, seconded by Mr. Salvin, and carried. A vote of thanks to the Treasurer, Secretaries, and Librarian was moved by Mr. J. W. Dunning, seconded by Lord Walsingham, and carried. Mr. Saunders, Mr. Goss, and Mr. Grut severally replied.

PARIS.

Academy of Sciences, January 7.—M. Janssen in the chair.—On the Lindstedt series, by M. H. Poincaré. Although these series are not convergent in the strict sense of the word, they are often very useful in astronomical calculations owing to the rapidity with which the terms decrease. Here M. Lindstedt's method is presented from a fresh standpoint, and brought into connection with the principles expounded in Jacobi's "Vorlesungen über Dynamik."—On the reactions between chromic acid and oxygenated water, by M. Berthelot. This peculiar reaction has acquired a fresh interest since M. Ad. Carnot's recent discovery of an ingenious method for effecting the quantitative analysis of chromic acid by means of oxygenated water, and reciprocally, with simultaneous reduction of both compounds. The analogy of the reaction with that of permanganic acid has induced M. Berthelot to repeat the experiments, with the result that this, like other reactions of oxygenated water, may now be interpreted by the laws of thermo-chemistry.—On an hydraulic machine constructed in England, by M. Anatole de Caligny. The reference is to Mr. Pearsall's apparatus, in which M. de Caligny's open tube is replaced by a chamber allowing the air to escape freely.—Observations made on the summit of Mont Ventoux on the calorific intensity of solar radiation, by MM. A. Crova and Houdaille. The object of these observations has been to ascertain whether, at an altitude of 1900 metres, solar radiation undergoes daily variations analogous to those recorded at Montpellier, and whether from the curves registered at the higher elevation a value may be deduced for the solar constant more exact than those obtained from the calculation of the curves traced at sea-level. It results from these researches that at the altitude of 1900 metres the solar constant may acquire a value very close to the 3 calories obtained by Mr. Langley from his observations on Mount Whitney. The polarization of the blue light of the sky was also studied by means of M. Cornu's photopolarimeter, and its spectrum analysis made with M. Crova's spectrophotometer modified for the purpose. The polarization appears in general to increase with the solar constant, thus furnishing useful data for determining the degree of calorific transparency in the atmosphere.—On the true and mixed butylic ethers, by M. E. Reboul. Theory anticipates ten of these compounds; but two only are known, the normal diprimary butylic ether of Lieben and Rossi, and Kessel's disecondary. Williamson's general method having mostly failed, or yielded only doubtful results, M. Reboul has attempted by the process here described to complete the whole series. He has so far obtained five, not yet described, which with the two already known leave three to be still procured under other conditions.—On M. Hirn's new work, entitled "Constitution de l'Espace céleste," by M. Faye. This important work deals with the question of an ether or subtle medium filling all space, as postulated by physicists to explain the phenomena of light, heat, and electricity, but the presence of which astronomers have failed to detect as a resisting medium in the movements of the heavenly bodies. With a view to putting this seeming contradiction in a clear light, M. Hirn has worked out some delicate calculations which have led to several remarkable and at times wholly unexpected conclusions. Thus in estimating the density of a new medium capable by its resistance of causing a secular acceleration of half a second in the mean velocity of the moon, he finds that such a density would correspond with a kilogramme of matter uniformly diffused throughout a space of about 390,000 square miles. This is a density one million times rarer than that of the air reduced to one millionth of its normal density in Mr. Crookes's ingenious apparatus.—On the perturbations of the planet Hestia (46), according to the theory of M. Gyldén, by M. Brendel. The application of M. Gyldén's theory of perturbations to this planet shows that it is subject to some very considerable disturbances, its mean motion being about three times that of Jupiter.—On a process by which diurnal nutation may be demonstrated, and its constants determined, by M. Folie. This extremely simple process consists in observing at intervals of six hours two stars distant not more than 3′ from the Pole.—On the quantitative analysis of organic nitrogen by the Kjeldahl method, by M. L. L'Hote. This new method is here

subjected to a thorough test, and is found to yield results greatly inferior in accuracy to those obtained from the old process by means of sodaic lime.—On the early and late varieties of beetroot, by MM. C. Viollette and F. Desprez. The early maturing plant, although yielding the largest proportion of sugar, is in other respects subject to many drawbacks preventing its general adoption by growers. A series of experiments here described show that a late variety may be obtained, which, while free from these disadvantages, yields an abundant supply of sugar.—M. Hermite has been unanimously elected Vice-President, and MM. Becquerel and Fremy members of the Central Administrative Committee, for the current year. In the present number is also published the list of the members of the Academy on January 1, 1889. The following are the English and American corresponding members:—*Geometry:* James Joseph Sylvester, George Salmon. *Astronomy:* John Russell Hind, J. C. Adams, Arthur Cayley, Joseph Norman Lockyer, William Huggins, Simon Newcomb, Asaph Hall, Warren De La Rue, Benjamin Althorp Gould, Samuel Langley. *Geography and Navigation:* Admiral G. H. Richards. *General Physics:* James Prescott Joule, George Gabriel Stokes. *Chemistry:* Edward Frankland, Alexander William Williamson. *Mineralogy:* James Hall, Joseph Prestwich. *Botany:* Joseph Dalton Hooker, Maxwell Tylden Masters. *Rural Economy:* Sir John Bennet Lawes, Joseph Henry Gilbert. *Anatomy and Zoology:* James Dwight Dana, Thomas Henry Huxley. *Medicine and Surgery:* Sir James Paget. Foreign Associates: Sir Richard Owen, Sir George Biddell Airy, Sir William Thomson.

January 14.—M. Des Cloizeaux in the chair.—On the solar statistics of the year 1888, by M. R. Wolf. From the various solar and magnetic observations made at the Observatories of Zurich and Milan, M. Wolf has by his well-known method deduced and tabulated for last year the mean monthly values for the relative number r, for the variations in declination v, and for the increase Δr and Δv that these quantities have received since the corresponding epochs of the year 1887. It results from these tables that both the relative number and the magnetic variation have continued to diminish, and that it is probable the minimum has now been nearly reached. It also appears that the slight anomalies recorded during the previous year have disappeared, and that the parallelism between these two series has been almost completely re-established.—Mode of diffusion of the voltaic currents in the human organism, by M. L. Danion. From the series of experiments here described it appears that, excepting the skin and bones, the various tissues and substances constituting the organism have practically the same electric conductibility. The skin is in general highly resisting, while the conductibility of the bones, which alone affect the diffusion of the current, is perceptibly less than two-fifths of that of the other hypodermic tissues. Under like conditions the diffused intrapolar and extra-polar intensities have the same value. Contrary to the universal opinion, the choice and combination of electrodes of various dimensions does not perceptibly modify the effects of hypodermic electrization. The experiments made on animals and on man confirm those carried out on homogeneous liquid masses, while at the same time showing the extreme diffusion of the voltaic currents, hence the deductions drawn from the latter order of experiments are applicable to the electrization of the animal organism.—Observations of Faye's comet, made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Trépied, Rambaud, and Sy. These observations cover the period from December 28 to January 5.—On the influence of the shock on the permanent magnetism of nickel, by M. G. Berson. These experiments form a supplement to those lately made by the author with a bar of steel. The various phenomena are in both cases strictly analogous, tending to show that with a field of feeble intensity a bar of either metal may be permanently magnetized, provided the shocks be given while the bar is within the field. The vibrations of the apparatus furnished with permanent magnets should also be carefully avoided, as they tend rapidly to diminish the force of the magnetic momentum.—On the oxidation and scouring of tin, by M. Léo Vignon. In a previous communication (*Comptes rendus*, November 5, 1888) the author showed that crystallized tin, deposited by the action of zinc and of the chemically neutral solutions of the stannous or stannic chlorides, is capable of high oxidation, and also when heated in contact with the air presents the curious property of combining with oxygen without melting, but burning like tinder (*amadou*). His further experiments with this partially oxidized tin have disclosed several facts, which explain the phenomena already described, and at the same time supply the elements of the theory on which depend the common industrial operations known as tinning and tin-soldering. In general it may be concluded that tin is capable of considerable oxidation in a dry or moist atmosphere, a conclusion which agrees with the comparative data already obtained on the heats of formation of the metallic oxides.—On ergosterine, a new immediate principle of the ergot (spur) of rye, by M. C. Tanret. The ergot of rye contains a crystallized substance, which closely resembles, and may readily be confounded with, cholesterine. But the careful study made by M. Tanret of this fungus shows that it differs in its composition both from animal cholesterine and its isomerous vegetable substances. This new principle is accordingly here described and analyzed under the name of ergosterine. Its composition may be represented by the formula—

$$C_{52}H_{40}O_2, H_2O_2.$$

It crystallizes in alcohol in the form of little pearly pellets, and in ether in that of sharp needles, and is quite insoluble in water. Like cholesterine, it is a monatomic alcohol, as appears from the analysis of its formic, acetic, and butyric ethers.—Papers are contributed by M. Hugo Gyldén, on the elementary terms in the co-ordinates of a planet; by M. Maquenne, on the heptine of a perseite; by MM. Ed. Heckel and Fr. Schlagdenhauffen, on the chemical constitution and industrial value of the gutta yielded by *Bassia latifolia*; and by M. Hueppe, on the virulence of cholera parasites.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Annuaire de l'Académie de Belgique, 1889 (Bruxelles).—Longmans' School Arithmetic with Answers: F. E. Marshall and J. W. Welsford (Longmans).—Biologia Centrali-Americana (Botany): W. B. Hemsley and Sir J. D. Hooker.—Hulfstabellen zur Mikroscopischen Mineralbestimmung in Gesteinen: H. Rosenbusch (Stuttgart, Koch).—Les Minéraux des Roches: A. M. Lévy and A. Lacroix (Paris, Baudry).—A Course of Easy Arithmetical Examples for Beginners: J. G. Bradshaw (Macmillan).—Solutions of the Examples in a Treatise on Algebra: C. Smith (Macmillan).—Calendar and General Directory of the Department of Science and Art for the Year 1889 (Eyre and Spottiswoode).—A Monograph of the British Uredineæ and Ustilagineæ: C. B. Plowright (Kegan Paul).—Graphics, or the Art of Calculation by Drawing Lines, Part 1: R. H. Smith (Longmans).

CONTENTS.

THURSDAY, JANUARY 31, 1889.

MIND IN MAN AND BRUTE.

Mental Evolution in Man: Origin of Human Faculty. By G. J. Romanes, M.A., LL.D., F.R.S. (London: Kegan Paul, Trench, and Co., 1888.)

THE subject with which Mr. Romanes deals in this volume is one which presents great, if not insuperable, difficulties. Whether or not there be a difference in kind—that is, in origin—between the mind of man and the mind of the brute, it is only in terms of the former that the latter can be interpreted. We can only reach minds other than our own by an ejective process of inference. Fully admitting that the evidence is amply sufficient to justify us in inferring the existence of mental processes in our dumb companions, the fact remains that there are enormous difficulties in getting at the nature of these mental processes. Our mental life is carried on in a rare atmosphere of self-conscious conceptual thought. And before we can put ourselves ejectively into the place of the brute, we have to divest ourselves of our conceptual habiliments; nay, more, we have—if current views be correct—to strip off the inner garment of our self-consciousness. Hence, some thinkers are driven to the extremity of agnosticism in this matter, and hold with Prof. Max Müller that, "according to the strict rules of positive philosophy, we have no right to assert or deny anything with reference to the so-called mind of animals." This, no doubt, is going too far. But, seeing that mind in the animal world and in very young children has to be interpreted not only by, but also in terms of, human consciousness, it behoves the investigator to at least express his opinions with becoming modesty. I cannot say that Mr. Romanes's modesty is obtrusive. There is, indeed, a tone of "cocksureness" ill befitting the subject in hand, and painfully marring the dignity of a work the ability and earnestness of which are conspicuous.

The problem which Mr. Romanes has set himself to solve in this volume is the genesis of self-consciousness and conceptual thought. He therefore begins by analyzing and classifying *ideas.* "Psychologists," he says, "are agreed that what they call particular ideas, or ideas of particular objects, are of the nature of mental images, or memories of such objects—as when the sound of a friend's voice brings before my mind the idea of that particular man. Psychologists are further agreed that what they term general ideas arise out of an assemblage of particular ideas, as when, from my repeated observation of numerous individual men, I form the idea of man, or of an abstract being who comprises the resemblances between all these individual men, without regard to their individual differences. Hence, particular ideas answer to percepts, while general ideas answer to concepts." This twofold classification, thus broadly and somewhat unsatisfactorily stated, Mr. Romanes deems inadequate. Defining *idea* as a generic term to signify indifferently any product of imagination, from the memory of a sensuous impression up to the result of the most abstract generalization, he classifies as under.

A "simple idea," "particular idea," or "concrete idea," is the mere memory of a particular sensuous perception.

A "compound idea," "complex idea," or "mixed idea," is the combination of simple ideas into that kind of composite which is possible without the aid of language.

A "general idea," "abstract idea," "concept," or "notion," is that kind of composite idea which is rendered possible by the aid of language, or by the process of naming abstractions as abstractions.

With regard to these, he says that the first division has to do only with what are termed percepts, while the last has to do with what are termed concepts. And there being no word to meet the middle division, he coins the term *recept* (= generic idea), which appears to him exactly to meet the requirements of the case, because in *receiving* such ideas the mind is passive, whereas in conceiving abstract ideas the mind is active.

I do not regard this classification as very satisfactory, and I doubt whether it will find much favour among modern psychologists. Regarding, as I do, every percept as a synthesis effected by the mind at the bidding of a sense-impression, I am not prepared to regard the mind as passive in what Mr. Romanes calls "reception." And I think that a subdivision of percepts into particular and generic would have been sufficient to meet all the requirements of Mr. Romanes's argument. As it is, he narrows down perception to a very limited province; for he admits that the ideation of infants is from the first generic. Throughout the whole of this chapter on *ideas*, Mr. Romanes seems to ride the "sensitive plate" analogy too hard. His percepts are photographs of particular objects; his recepts are composite photographs, like Mr. Galton's picture of the average blackguard. But this is to lose sight of the activity of mind, which, automatic though it be, is none the less real. In the great body of percepts and recepts there is far more give than take in the mental operation involved.

Turning to one of the examples of what, I presume, Mr. Romanes regards as a recept, he says: "All the higher animals have general ideas of 'good-for-eating' and 'not-good-for-eating,' *quite apart from any particular objects of which either of these qualities happens to be characteristic.*" I very much question whether any animals have the power of isolating qualities, implied in the words I have italicized. Nor should I call the idea of such an isolated quality either a percept or a recept. A dog may, by an automatic action of the mind, build into his percepts or recepts the element of niceness or nastiness as part of the object constructed by mental synthesis. But this is a very different thing from having a general (or generic) idea of niceness or nastiness apart from the object. Such an idea is the result of analysis, and the hanging of the isolated results of analysis on separate name-pegs.

Much of Mr. Romanes's work is necessarily devoted to language, and here, although he does not profess to speak as an expert, will be found much that will repay careful perusal. He introduces the term "denominative" for a sign consciously bestowed as such with a full conceptual appreciation of its office and purpose as a name. He considers that a parrot may use *bow-wow* as a denotative sign for a particular dog, and then extend it to other dogs, thus using it as a connotative sign. No parrot, however, could employ a word in its truly denominative sense. This is a conceptual, as opposed to a merely re-

ceptual, act. Natural or conventional signs may thus be built, by the animal, into the structure of its percepts or recepts. They cannot be consciously given for the purposes of conceptual thought. Mr. Romanes, at this stage of his inquiry, extends his classification, marking off four stages of ideation.

(1) *Lower recepts*, comprising the mental life of all the lower animals, and so including such powers of receptual connotation as a child when first emerging from infancy shares with a parrot.

(2) *Higher recepts*, or *pre-concepts*, comprising all the extensive tract of ideation that belongs to a child between the time when its powers of receptual connotation first surpass those of a parrot up to the age at which connotation as merely connotative begins to become denominative.

(3) *Lower concepts*, comprising the province of conceptual ideation when this first emerges from the higher receptual, up to the point where denominative connotation has to do, not merely with the naming of recepts, but also with that of associated concepts.

(4) *Higher concepts*, comprising all the further excellencies of human thought.

With this apparatus of terms, Mr. Romanes is prepared to enter upon the question of self-consciousness, which, he says, consists in paying that same kind of attention to internal or psychical processes as is habitually paid to external or physical processes, or in bringing to bear upon subjective phenomena the same powers of perception (*sic*) as are brought to bear upon the objective. I question whether Mr. Romanes's opponents will be quite satisfied with this definition. Starting with it, however, Mr. Romanes contends that, "given the protoplasm of the sign-making faculty so far organized as to have reached the denotative stage; and given also the protoplasm of judgment so far organized as to have reached the stage of stating a truth without the mind being yet sufficiently developed to be conscious of itself as an object of thought, and therefore not able to state to itself a truth as true; by a confluence of these two protoplasmic elements an act of fertilization is performed, such that the subsequent processes of mental organization proceed apace, and soon reach the stage of differentiation between subject and object." In working out this contention he makes use of the fact that a large number of the recepts of the brute have reference, not to objects of sense, or even to muscular sensations, but to the mental states of other animals. We wish he had given us more information on this head—not as to the fact, which can hardly be questioned, but as to the mode of origin of this ejective element, and as to how far such an element modifies the perceptual or receptual nature of the mental product or synthesis into which it enters. The ejective element is so purely inferential, and would seem to be reached by so indirect a process through the mental states of the animal itself, that further information on Mr. Romanes's view of the matter would have been welcome. Proceeding, however, from this basis, he takes it to be a matter of general observation that animals habitually and accurately interpret the mental states of other animals, while they well know that other animals are similarly able to interpret theirs—as is best proved by their practising the arts of cunning, concealment, hypocrisy, &c. Thus the truth is "gradually borne in upon the mind of an animal that it is a separate individuality; and this, though it is conceded that the animal is never able, even in the most shadowy manner, to think about itself as such. In this way there arises a sort of 'outward self-consciousness,' which differs from true or inward self-consciousness only in the absence of any attention being directed upon the inward mental states as such." Turning then from the animal to the child, Mr. Romanes contends that at an early age he only possesses outward or receptual self-consciousness. As yet he has paid no *attention* to his own mental states further than to feel that he feels them: he speaks to and of himself in the third person or by his proper name. "The change of a child's phraseology from speaking of self as an object to speaking of self as a subject does not take place—or but rarely so—till the third year. When it has taken place, we have definite evidence of true self-consciousness, though still in a rudimentary stage." These in brief—and it is difficult to condense the argument into a small compass—are the steps by which Mr. Romanes ascends to rudimentary self-consciousness as it first dawns upon the child-mind. I find myself unable either to accept or to criticize Mr. Romanes's account of the genesis of self-consciousness. I have read and re-read it, but find myself incapable of thinking myself into the gradually ascending stages. I fail adequately to imagine the mental condition of the dog or the very young child in which outward or receptual, and eventually inward or conceptual, self-consciousness is being evolved. I believe with Mr. Romanes that the evolution has taken place; but I fail to realize the *how*. This is, of course, not his fault: he gives me the steps of the process; he cannot give me the capacity to conceive them. In any case, Mr. Romanes claims to have shown—and how far the claim is justifiable the reader must determine for himself—"that, in whatever way we regard the distinctively human faculty of conceptual predication, it is proved to be but a higher development of that faculty of receptual communication, the ascending degrees of which admit of being traced through the brute creation up to the level which they attain in a child during the first part of its second year,—after which they continue to advance uninterruptedly through the still higher receptual life of the child, until, by further though not less imperceptible growth, they pass into the incipiently conceptual life of the human mind."

I have left myself no space to deal with the purely philological part of the work. I may note, however, that, in speaking of the roots into which language may be analyzed, Mr. Romanes contends that they can only be regarded as original or primary in the sense that they are the ultimate results of analysis, *i.e.* that they are not original in the sense of representing the ideation of really primitive man; and, again, that they for the most part stand for named recepts or lower concepts, and in a comparatively small degree for higher concepts or the results of conceptual analysis. In both these contentions I conceive he is right. Speaking as a layman of the work of one who freely admits that he is not an expert, the philological analysis seems to me extremely well done.

In conclusion, I would congratulate Mr. Romanes on this his latest volume, which undoubtedly contains much excellent and painstaking work. If I am not altogether satisfied with his psychology; if I am unable to agree

with him that "the phases of development which have gradually led up to conceptual thought admit of being as clearly traced as those which have led to any other product, whether of life or of mind"; if I even go further, and confess my belief that mental evolution never will be and never can be independently established, though it may be accepted as a corollary from organic evolution by those who are content to remain naturalists;—this does not lead me to welcome any the less cordially the valuable researches of Mr. Romanes in a very interesting but exceedingly difficult field of investigation.

C. LLOYD MORGAN.

THE MICROSCOPICAL STUDY OF MINERALS IN ROCKS.

Microscopical Physiography of the Rock-making Minerals: an Aid to the Microscopical Study of Rocks. By H. Rosenbusch. Translated and Abridged for use in Schools and Colleges by Joseph P. Iddings. Illustrated by 121 Woodcuts and 26 Plates of Photomicrographs. (New York: John Wiley and Sons. London: Macmillan and Co., 1888.)

Hülfstabellen zur mikroskopischen Mineralbestimmung in Gesteinen. Zusammengestellt von H. Rosenbusch. (Stuttgart: E. Koch, 1888.)

Les Minéraux des Roches. Par A. Michel-Lévy et Alf. Lacroix. (Paris: Libraire Polytechnique, Baudry et Cie. 1888.)

IT has been well said that this is an age of books. With equal propriety it might be characterized as an age of text-books. Indeed, so rapidly do these aids to learning increase and multiply, that it seems almost incredible that any rational proportion can still exist between demand and supply. In works on petrography, the year just passed away has been exceptionally fruitful. From the ponderous tome, replete with detail and illustrated by magnificent plates, down to the superficial shilling primer, we have before us a remarkable series of volumes in which rocks and their constituent minerals are treated from all the most recent points of view. It was in the early months of 1888 that Prof. Rosenbusch's invaluable "Mikroskopische Physiographie," was completed by the publication of the second part of Volume II., and the same year has also witnessed the appearance of Mr. J. P. Iddings's translation of the first volume, on the "Mikroskopische Physiographie der petrographisch wichtigen Mineralien." The usefulness of this book as a work of reference cannot be over-rated, as will be acknowledged by all those who have had occasion to use it. Unfortunately, it has hitherto been sealed to those who possessed no knowledge of German, and Mr. Iddings has earned the thanks of English-speaking petrographers in undertaking its translation, a task involving no inconsiderable amount of patient labour.

In the English edition the book has been slightly abridged, but in doing this the translator has "endeavoured to retain all that appeared essential to a fair general comprehension of the subject, omitting what seemed to be refinements beyond the need of the average student, and for which the advanced student is referred to the original work. Thus most of the historical portions have been omitted, as well as the elaborate treatment of the optic anomalies of certain minerals, and many notes on European localities; while a number of notes on American occurrences have been inserted." We cannot help thinking, however, that it would have been an advantage to retain the alphabetical index to petrographical literature, which, on account of its completeness, forms one of the most valuable features of Prof. Rosenbusch's work. Reference to original sources should be encouraged to the utmost in a students' book.

Mr. Iddings has added some useful information on allanite, or orthite, as some prefer to name it. According to his researches, carried out in conjunction with Mr. Whitman Cross, the occurrence of this mineral as an accessory constituent of rocks is much more wide-spread than is usually supposed. These authors have shown it to be widely distributed through a great variety of rocks in the United States, it having been found in gneiss, granite, granite-porphyry, quartz-porphyry, diorite-porphyrite, andesite, dacite, and rhyolite. It is possible that it has sometimes escaped recognition, owing to its resemblance in colour and pleochroism, to some varieties of hornblende and biotite. It may, however, be distinguished from the former by its higher double refraction, and from the latter by its larger optic axial angle.

The translator must be congratulated on the very successful way in which he has rendered the original into clear and concise English, while keeping strictly within the author's meaning; indeed, were any fault to be found with Mr. Iddings's style, it would perhaps be that he has a tendency to translate too literally. But this is erring on the safe side. Occasionally, however, he goes to the other extreme, the translation becoming so free as to be inaccurate, as, for instance, when "*crystallinische Schiefer*" is rendered "Archæan formation," so that phenomena of dynamic metamorphism (such as the marginal crushing of crystals, or "cataclastic structure"), that are common to the crystalline schists of whatever age, are thereby limited, inferentially of course, to rocks of Archæan age (pp. 251, 277, 279, 284, 307, 309, 310). The word *Sphärokrystal* has been rendered *spherulite*, on the ground that the latter has become well established in English literature. This is true; but the word spherulite is equally well known in Germany. On p. 392 of the "Physiographie der massigen Gesteine," Prof. Rosenbusch carefully distinguishes between the meanings he attaches to *spherocrystal* and *spherulite* respectively; the former being applied by him to spherular bodies consisting of radially-aggregated fibres of a single mineral (*e.g.* chlorite), the latter to those imperfectly individualized fibrous bodies so common in vitreous rocks.

In one or two instances only has Mr. Iddings been led astray by the peculiarities of the German idiom. The most serious of these slips occurs on p. 286, where we read that, "in the so-called pseudomorphs of cassiterite after orthoclase from Huel Coates, tourmaline and quartz, besides cassiterite, *form a principal part of the muscovite.*" The meaning of the original, which is, it is true, somewhat ambiguously expressed, is, that besides cassiterite, tourmaline, and quartz, muscovite forms one of the principal constituents of the pseudomorphs after orthoclase from Huel Coates.[1]

[1] "Auch bei der sog. Pseudomorphose von Cassiterit nach Orthoklas von Huel Coates im Kirchspiel St. Agnes, Cornwall, bildet neben Cassiterit, Turmalin und Quarz der Muscovit einen Hauptgemengtheil."

Typographical errors are rather numerous; but these, with few exceptions, are corrected in the page of errata inserted after the title-page.

Prof. Rosenbusch's "Tables for the Microscopic Determination of Minerals in Rocks" is one of the most useful compilations which have appeared for some time. It has doubtless been suggested by, and to some extent modelled on, Dr. E. Hussak's "Anleitung zur Bestimmung des gesteinsbildenden Mineralien," which, in spite of numerous inaccuracies, was found to be in such demand among students that soon after its publication it was translated into English, errors and all. Tables of this kind are sure to be favourably received by the practical worker, on account of the saving in time and labour resulting from their use, a single glance being sufficient to obtain all the requisite information about a given mineral. The book consists of nine two-page tables, in which we find recorded data concerning the cleavage, form, crystallographic development, colour, pleochroism, indices of refraction, optic orientation, dispersion, crystal-system, specific gravity, behaviour with reagents, and chemical composition of all the minerals occurring in rocks, whether as essential or accessory constituents. Many will be surprised to hear that these amount to as many as 190. There can be no hesitation in predicting a wide circulation for this very serviceable little book. An English edition is being prepared, we understand, by Messrs. Swan Sonnenschein and Co.

"Les Minéraux des Roches," by Messrs. A. Michel-Lévy and A. Lacroix, is a more elaborate work, dealing with the same subject. This book, as we are informed in the preface, is complementary to the "Minéralogie Micrographique," published in 1878 by the first-named author in collaboration with M. Fouqué. It is divided into two parts: (1) the application of optical and chemical methods to the microscopical study of minerals, by A. Michel-Lévy alone; (2) a descriptive summary of the principal minerals occurring in rocks, by both authors.

The first part contains a mathematical exposition of the optical properties of minerals, which, though tending occasionally to abstruseness, is characterized by a true French elegance of treatment, the excellent use made of curves in representing graphically the variation of extinction-angles being especially worthy of commendation. On p. 54 we have a description of an instrument for the measurement of double refraction, which has been constructed for the author by M. Nachet, the celebrated Parisian microscope-maker. The *comparateur*, as it is named by its inventor, consists of a special eye-piece, to which is attached an arrangement of prisms and lenses, by which the tint and luminous intensity of a given mineral can be compared with those of a movable quartz wedge. A series of measurements of the double refraction of the principal rock-forming minerals, made by the authors with this instrument, follow on p. 66, together with the colours presented by plates of 0·01, 0·02, and 0·03 mm. respectively. At the end of the book there is also a magnificent plate, representing Newton's scale of colours up to the fifth order. By an ingenious contrivance, the colours given by the different minerals are shown for thicknesses ranging between 0 and 0·06 mm.

In the second part of the book the minerals are arranged in accordance with a scheme which is partly alphabetical, partly systematic—*i.e.* the members of a natural group (*e.g.* the felspars, micas, amphiboles, and pyroxenes) have not been separated. This method is, perhaps, not quite satisfactory, as the advantage of an alphabetical arrangement is in great part lost, while at the same time the systematic classification is necessarily incomplete. But reference to the minerals is much facilitated by an excellent index.

This part of the book contains a number of new determinations made by the authors, who have "endeavoured," and let us add with success, "to make it as complete as is consistent with the present state of the science." It concludes with a synoptical table, in which the optical data are again briefly recapitulated.

F. H. H.

SEWAGE TREATMENT, PURIFICATION, AND UTILIZATION.

Sewage Treatment, Purification, and Utilization. By J. W. Slater, F.E.S. (London: Whittaker and Co., 1888.)

THIS is the work of an advocate of chemical precipitation processes for the treatment of sewage. It cannot claim to be an impartial review of the "sewage question," for the bias of the author is exhibited in nearly every page. The author is evidently of opinion that his case is likely to derive support from what is known as *argumentum ad hominem*, or abuse of the other side (those who hold different views from his own). The Rivers Pollution Commissioners come in for a considerable share of these attentions, as the following passages will show:—"Such a man was by nature qualified for a Royal Commissioner, who can never admit that either himself or any of his predecessors can have been mistaken" (p. 165). The spoiling of an effluent from a chemical precipitation process by fermentation of the deposited sediment is spoken of as a result "to gladden the heart, and for once justify the representations of ex-Royal Rivers Pollution Commissioners" (p. 172). "Mr. Bailey-Denton—who seems to be to Prof. Frankland what Ali was to Mahomed and Mr. Grant Allen to Charles Darwin" (p. 79). The Royal Commissioners on Metropolitan Sewage Discharge are alluded to in the following terms (p. 86): "That Commission refused or neglected—and in such a case these two terms are nearly equivalent—to examine fully and fairly into the merits of precipitation. . . . It would not, or at least did not, visit Aylesbury. It was satisfied to condemn precipitation on the faith of the archaic reports of the Royal Rivers Pollution Commissioners, reports which, if true at the date when written—and this is a fairly strong concession—are demonstrably false if applied, *e.g.*, to the process now in operation at Aylesbury."

It will perhaps raise a smile amongst *connoisseurs* in sewage matters to learn that managers of sewage works require protection from the (dishonourable) tricks of critical visitors. The author, at any rate, believes their innocence is likely to be imposed upon. He writes (p. 168):—"A few hints may here be given about sampling effluents for analysis or for preservation. I write here mainly for the guidance of officials left in charge of

sewage works, that they may know what tricks may be attempted. It is commonly said 'that any stick is good enough to beat a dog with,' and on a similar principle, any unfair, dishonourable stratagem is held legitimate to bring discredit on a process for the chemical treatment of sewage." "Another necessary precaution relates to sampling. If any visitor asks for a sample, it should be given him solely on the condition that he takes and seals up at the same time a check sample, to be left at the works for analysis by some independent chemist. Without this precaution he may, for instance, add to his sample a little urine or blood, or a culture solution, and still represent it as a normal sample" (p. 170). These passages serve to show the opinion in which the author holds his brother experts.

It is very plain that the author rejects *in toto* all experience but what is favourable to his own views. The following passage is a specimen of the assertions constantly made throughout the book:—"Just in proportion as a sewage is purified by precipitation, its manurial value for irrigation will decrease down to that of plain water" (p. 70). This statement, which implies that complete purification is attainable by precipitation processes, is at total variance with the truth, and has been disproved over and over again; but it is a sufficient index of the kind of treatment the subject throughout receives.

On the subject of sewage manures the author quotes, as representative of their average composition and value, the results of Dr. Tidy's analysis of the sewage manure from the "A.B.C." process at Aylesbury. Can the author not be aware that the Aylesbury manure has become one of the wonders of the scientific world? How is it that the "A.B.C." process can convert a nearly worthless substance—sewage sludge—into a rich and paying manure? Can the author tell us?

OUR BOOK SHELF.

The International Annual of Anthony's Photographic Bulletin. Edited by W. Jerome Harrison, F.G.S., and A. H. Elliot, Ph.D. (London: E. and H. T. Anthony and Co., 1888.)

IN the pages of this volume we have a valuable collection of articles written by men many of whom are eminent in the art and science of photography.

At the present day, considering the great number working, either as amateurs or professionals, at this subject, and that that number is still on the increase, and also the numerous books that have been published, one would think it hard for another work to differ a great deal from those preceding it. The editors, in their preface, tell their readers to "partake of and enjoy the feast that they have set before them; and, if the viands tickle their palates, praise rather the maker of that particular dish than those who have spread the table."

The present work consists of a popular account of nearly every branch of the subject, including stellar photography.

The volume is beautifully illustrated by specimens of various methods of printing, engraving, &c. The appendix contains a list of Photographic Societies of the British Isles, colonies, and of America, with numerous tables.

The editors have produced a volume which will not only be appreciated by members of the photographic world, but will be interesting to any ordinary reader.

Instruction in Photography. By Captain W. de W. Abney, C.B., R.E., F.R.S. Eighth Edition. (London: Piper and Carter, 1888.)

ALL that is needed for photographers, whether amateur or professional, will be found in this well-known book, which has been thoroughly revised and brought up to date. By the addition of new matter the volume has been slightly increased, and an alteration of some importance consists in the introduction of both the French and English measures for the various formulæ used throughout.

The work is profusely illustrated, and the least we can say is that no studio ought to be without it.

Lessons in Elementary Physics. By Balfour Stewart, M.A., LL.D., F.R.S. New and Enlarged Edition. (London: Macmillan and Co., 1888.)

THE present edition of this well-known book was prepared only in part by the late author. The task has been completed by Mr. W. W. Haldane Gee, B.Sc., Lecturer of the Victoria University. No material alterations have been made, except in the chapters relating to electricity and magnetism, which have been rearranged, and expanded by the addition of new matter and figures.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Supposed Fossils from the Southern Highlands.

IN the article in your last number upon my paper lately read before the Royal Society of Edinburgh, respecting the discovery of some supposed annelid tubes in the quartzites of Loch Fyne, some observations are made on the interest attaching to the question as bearing on the geological horizon of the metamorphic series which constitute the great bulk of the South-Western and Central Highlands. With reference to this question, however, I am anxious to disclaim attaching any great importance to the discovery of annelids, because I do not admit that any real doubt exists as to the Silurian horizon to which our rocks were referred by Murchison. In this country, particularly, the evidence seems to me complete whether fossils be, or be not, found in them; because in this country we have complete stratigraphical evidence from the occurrence of a small local coal-basin in the district of Kintyre. The backbone of that long peninsula consists of the same metamorphic rocks as the rest of the country. Along its shores it is fringed by cakes of Old Red Sandstones which have resisted denudation, or have survived destructive dislocations. At the southern end of the peninsula these sandstones have survived in considerable masses—including beds of conglomerate, of freestone, and of limestones. All these beds rest unconformably on the metamorphic schists and limestones, whilst they are, again, also to be seen dipping and passing under the coal-measures and Carboniferous limestones in their usual and natural order. There can be no doubt of the order of succession in this case, and it establishes the position of our metamorphosed slates and quartzites as rocks which belong to the horizon below the Old Red.

Of course, the discovery of fossils would put an end to the new surmise that they are not sedimentary deposits at all, but intrusive rocks, simulating true bedding in consequence of "foliation," or some other process of mineralization. But this surmise is so wild that I feel no doubt about its early disappearance.

In this, I believe Dr. Geikie entirely agrees; and he has consequently expressed a confident expectation that fossils will be found in our schists and quartzites some day.

I may add that since my return from Edinburgh a correspon-

dent has sent to me an unmistakable specimen of annelid piping from a rock at Killin. Now that we are all on the scent, I have no doubt that my Loch Fyne worms will be found at the head of a long procession. ARGYLL.

Inveraray, January 28.

Mr. Howorth on the Variation of Colour in Birds.

PERMIT me to point out that the fact cited by Mr. Howorth (*antea*, p. 294), of the similarity between the birds of Western Europe and those of Japan, which, according to him, illustrates "from an unexpected quarter" the views expressed in his work "The Mammoth and the Flood," is by no means so novel as the general reader might, from his communication, suppose. On the contrary, it has been before the world for more than half a century, and is, or ought to be, familiar to every well-informed ornithologist, since it was in 1835 that Temminck gave ("Manuel d'Ornithologie," Part 3, Introd. pp. l.–liii.) a list of 114 species of birds, which he said were common to the Japanese Empire and to Europe. It has subsequently been shown that this number was exaggerated; but, as observed in 1857 by Mr. Sclater, in his classical paper on the geographical distribution of birds, "there can be no question as to the general strong resemblance of the Japanese avifauna to that of Europe" (Journ. Linn. Soc., Zoology, ii. p. 134).

That the birds of Siberia differ in appearance from those of Japan and of Western Europe has also been abundantly shown by the long succession of illustrious explorers and naturalists from whom we derive all but an infinitesimally small portion of our knowledge of Siberian and Central Asiatic zoology; but Mr. Howorth does not seem to be aware that another hypothesis has usually been brought forward to account for that difference, as well as for the similarity of the Japanese avifauna and our own. In regard to the latter, this hypothesis has been propounded to my certain knowledge for more than five-and-twenty years (*Ibis*, 1863, p. 189); and, whether true or false, has not, so far as I am aware, been refuted. It is that the resemblance is an effect due to "the prevalence in both localities of an insular, as opposed to a continental climate," for it has been observed over and over again in various parts of the world (and not only in birds) that island forms possess a darker coloration than their continental representatives (*cf.* Gould, Proceedings of the Zoological Society, 1855, p. 78), while the fact that neither British nor Japanese birds exhibit the brilliant hues of their continental kinsfolk is notorious.

Another remark upon Mr. Howorth's communication I would offer. His theory, of which he has "little doubt," that "the willow grouse of the Continent is an altered form, and that our red grouse is the parent," is not new, but was definitely stated some eight or nine years ago in the "Encyclopædia Britannica," from the article "Grouse" in which I venture to quote a passage showing that there is a side of the question which he does not seem to have considered.

"A very interesting subject for discussion would be whether *Lagopus scoticus* or *L. albus* has varied most from the common stock of both. We can here but briefly indicate the more salient points that might arise. Looking to the fact that the former is the only species of the genus which does not assume white clothing in winter, an evolutionist might at first deem the variation greatest in its case; but then it must be borne in mind that the species of *Lagopus* which turn white differ in that respect from all other groups of the family *Tetraonidæ*. Furthermore, it must be remembered that every species of *Lagopus* (even *L. leucurus*, the whitest of all) has its first set of *remiges* coloured brown. These are dropped when the bird is about half-grown, and in all the species but *L. scoticus* white *remiges* are then produced. If, therefore, as is generally held, the successive phases assumed by any animal in the course of its progress to maturity indicate the phases through which the species has passed, there may have been a time when all the species of *Lagopus* wore a brown livery even when adult, and the white dress donned in winter has been imposed upon the wearers by causes that can be easily suggested, for it has been freely admitted by naturalists of all schools that the white plumage of the birds of this group protects them from danger during the snows of a protracted winter. On the other hand, it is not at all inconceivable that the Red Grouse, instead of perpetuating directly the more ancient properties of an original *Lagopus* that underwent no great seasonal change of plumage, may derive its ancestry from the widely-ranging Willow-Grouse, which, in an epoch comparatively recent (in the geological sense), may have stocked Britain, and left descendants that, under conditions in which the assumption of a white garb would be almost fatal to the preservation of the species, have reverted (though doubtless with some modifications) to a comparative immutability essentially the same as that of the primal *Lagopus*."

In conclusion, let me recommend those interested in the local variation of colouring in the plumage of birds to study Gloger's "Das Abändern der Vögel durch Einfluss des Klima's" (Breslau, 1833), a treatise which, though naturally out of date in many respects, contains much that ornithologists would be the better for not overlooking. ALFRED NEWTON.

Magdalene College, Cambridge, January 26.

Constitution of the Chlorides of Aluminium and the Allied Metals.

IN NATURE of December 27 (pp. 198–200), Dr. Young has directed the attention of your readers to the formulæ of the so-called hexachlorides, R_2Cl_6, and bodies of similar constitution. As this question possesses a great theoretical interest, I may be allowed to say a few words about it.

The constitution of the chlorides R_2Cl_6 was regarded by Friedel as analogous to that of carbon hexachloride, $Cl_3{\equiv}C{-}C{\equiv}Cl_3$, and the metals themselves were said to be tetravalent. Mendelejeff, in his classical paper on the periodic law—a gem of chemical literature—was the first to point out that it is not necessary to regard aluminium in Al_2Cl_6 as tetravalent, but that a compound of this formula represents only a molecular combination—a polymeric state—of $AlCl_3$. This view was very little regarded by chemists. It was, ten years later (Proc. Vienna Acad., 1882), extended by the author of the present lines, to other bodies of similar constitution, and it was shown that several chlorides give a vapour consisting partly or entirely of polymeric modifications of the simple chloride. So we have the molecules $Sn_2Cl_4{-}SnCl_2$, $Fe_2Cl_4{-}FeCl_2$ ($N_2O_4{-}NO_2$), and it was supposed that other substances of similar constitution would split up into simpler molecules, if they could bear higher temperatures, especially Al_2Cl_6, aluminium belonging to a natural group of *triad* elements, whose other members, viz. B and In, give chlorides of the simple and molecular formulæ, BCl_3 and $InCl_3$. As further instances of the above case may be quoted to-day, $H_2F_2{-}HF$, $Be_2Cl_4{-}BeCl_2$, $Ga_2Cl_6{-}GaCl_3$.

It may, however, be useful to those readers of NATURE who cannot follow completely the current chemical literature, to know that the values of vapour-densities obtained by Dumas's method are not strictly comparable with those obtained by V. Meyer's method; for, as has been shown lately by Victor Meyer and his pupils, especially Biltz, the vapour density of dissociable substances is found smaller by V. Meyer's apparatus than by that of Dumas at one and the same temperature. So Dumas's method shows, for the above chlorides, the existence of double molecules at a temperature at which that of V. Meyer indicates single molecules. It was already pointed out by Dr. Young that the single molecules, $GaCl_2$, $InCl_2$, $InCl_3$, and $CrCl_3$, were found by V. Meyer's method only, and there is no doubt that some of them may be found double by Dumas's method.

The result of the vapour-density determinations of aluminium chloride and the chlorides of the allied metals, carefully collected by Dr. Young in NATURE, is of great theoretical importance, for, as the chlorides RCl_3 exist as single molecules in the gaseous state, the respective metals are undoubtedly *trivalent* in these compounds.

But the question arises, what is the valency of these metals when their chlorides have the double molecule R_2Cl_6?

It is impossible to regard these compounds as analogous to carbon-hexachloride and the said metals as tetravalent, for that compound, when heated to a higher temperature, does *not* split up into two molecules, CCl_3, as the chlorides R_2Cl_6 do. Moreover, it must be admitted that the theory of valency which was developed by the study of organic compounds (*carbon* derivatives) does not strictly hold good in the case of the remaining elements. It is impossible to state the number of "bonds" by which the chlorides RCl_3 are kept together in double molecules, R_2Cl_6. This number is certainly not *one* ($Cl_3R{-}RCl_3$); just as two molecules of $SnCl_2$ in Sn_2Cl_4 cannot be regarded as being united by one bond, $Cl_2Sn'''{-}Sn'''Cl_2$, for no one will assume that tin is trivalent in this compound. If we admit any bonds at all between the two Sn atoms, we must admit two; $Cl_2Sn{=}$

$SnCl_2$. Again, silica (as has been shown by Mendelejeff long before Henry) certainly has a more complicated molecular formula than SiO_2, say $nSiO_2$, otherwise it would be a gas, analogous to CO_2, for $SiCl_4$ boils at 57° and CCl_4 at 76°. But would it be reasonable to say that the SiO_2 molecules are held together by the fifth valency of Si?

The only satisfactory answer to the above question is that in molecules R_2Cl_6 the two RCl_3 groups are held together by their own—let us say residual—affinity.

I think that the study of the question touched in the above lines will greatly add to the development of our theoretical ideas in chemistry, and deserves the careful attention of chemists.

B. BRAUNER.

Bohemian University, Prague, January 8.

IN my letter, referred to by Dr. Brauner, I considered only the molecular formulæ of the chlorides, as indicated by the experimental results.

Dr. Brauner has now, in his interesting letter, enlarged the field of discussion by introducing the question of the constitutional formulæ of these compounds, involving the vexed question of the valency of the elements. Dr. Brauner points out the importance of the proof of the trivalency of these metals in their chlorides when in the gaseous state at high temperatures, and he then discusses the valency of the metals when their chlorides have the formula R_2Cl_6.

Although I am inclined to believe that the views expressed by Dr. Brauner may eventually prove to be correct, yet I cannot help thinking that we are not yet in a position to speak with certainty on one or two points. Dr. Brauner states that the number of "bonds" by which the atoms of the metal in the compounds R_2Cl_6 are united is certainly not *one*, and again, that Sn_2Cl_4 cannot be expressed by the formula $Cl_2Sn'''—Sn'''Cl_2$. But is this quite certain? Indium, the metal next to tin in the seventh horizontal series, is mono-, di-, and tri-valent in the compounds InCl, $InCl_2$, $InCl_3$; is it, then, quite unwarrantable to assume that tin is di-, tri-, and tetra-valent in the compounds $SnCl_2$, Sn_2Cl_4, $SnCl_4$?

As regards silica, $nSiO_2$, the properties of the compound seem to indicate that n is a large number, and here, unless we arrange the Si atoms in a ring, like the carbon atoms in benzene, we must probably look on the compound as "molecular."

It is difficult to find experimental evidence which bears directly on the question; perhaps such results as those obtained by Dr. Ramsay and myself on the dynamical and statical methods of measuring vapour-pressures may throw some light on the matter. We find that the two methods give identical results with ammonium chloride, nitrogen peroxide, and acetic acid, just as they do with all stable solids and liquids, but very different results with all the other dissociating substances examined. This appears to indicate some difference in the molecular arrangement of the two groups of compounds, but it is quite uncertain whether the difference is one between "molecular" and "atomic" compounds, or between compounds formed in a very simple manner, such as $2O_2N = O_2N—NO_2$, and those in which there is a breaking down of a stable molecule, as in the case of chloral hydrate, $Cl_3C—C\begin{smallmatrix}\diagup H\\ \diagdown O\end{smallmatrix} + H.OH = Cl_3C—CH\begin{smallmatrix}\diagup OH\\ \diagdown OH\end{smallmatrix}$.

I am at present engaged in a study of the vapour pressures of halogen compounds by both methods, and it is just possible that some further light may be thrown on this question by the results of the investigation.

SYDNEY YOUNG.

University College, Bristol, January 15.

Remarkable Rime and Mist.

YOU have a letter in NATURE of January 17 (p. 270), signed "Annie Ley," which induces me to add the following:—We had here on January 6 an extraordinary rime formed at a temperature varying between 21°·5 and 25°·7 (1° warmer on the grass than at 4 feet), the air being almost calm. This rime increased in thickness and in length with the height above the ground. The length measured of the deposit on a birch-tree at 5 feet was ⅝ of an inch; at 10 feet, 1 inch; at 15 feet, 1¼ inch; and at 25 feet, 1½ inch. The hoar was nearly horizontal, pointing downwards at an angle of 15°. That deposited on the grass, however, was perpendicular, rising with a thin stem and having a large funnel-shaped head. Suddenly, at 10 a.m. of the 7th (next morning), the whole of the rime (still frozen) fell to the ground, and under the birch-tree of 30 feet in height and 18 feet across (sparse of branches, and none for 10 feet), the fallen rime covered the ground to the depth of rather more than 2 inches, and this, when melted, yielded 0·550 of an inch of water (or 3⅝ inches of rime to 1 of water). The rime on the grass when carefully collected and melted yielded 0·033 of an inch. There was a dense mist whilst the rime was being deposited.

From this elevation (530 feet) we frequently look over dense mists that cover the water of the Bristol Channel and see the hills of Somerset, Devon, Monmouth, and Glamorgan. On the 19th instant, with a hoar frost, over the Bristol Channel was a dense dark mist apparently extending 100 feet into the air. This mist, at 8.30 a.m., rapidly changed on its upper surface to cirri clouds, and then to transparent vapour, and in an hour the whole mist by this process had disappeared. Elsewhere the sky was cloudless. These mists of the Bristol Channel change on their upper surface sometimes to cirro-strati, sometimes to cumuli, and twice they have been known to change to thunder-clouds during the last two years. The change to cirri has only been seen once.

E. J. LOWE.

Shirenewton Hall, near Chepstow, January 22.

Cercyonis alope and *nephele*.

IN his review (NATURE, December 27, 1888, p. 193) of my work on butterflies now in course of publication (in which I receive a far more generous treatment than I am accustomed to), Captin Elwes thinks me illogical in holding to the probable specific distinction of *Cercyonis alope* and *nephele*, and at the same time the specific unity of the Eastern American forms of *Cyaniris pseudargiolus*, *lucia*, *violacea*, and *neglecta*; and suggests as to the former that climatic differences in the regions they respectively occupy may have brought about the distinctions noted.

On general grounds, it seems in the highest degree probable that climatic differences have had much to do with the origin of the different forms in both cases, be they species formed or forming. But surely Captain Elwes is confusing the judgment when he fails to make a distinction between the successive seasonal forms of a digoneutic butterfly, as in the case of *Cyaniris* and de Nicéville's Indian species to which he alludes, and the synchronous variation of a monogoneutic species, like those (or that) of *Cercyonis*.

SAMUEL H. SCUDDER.

Cambridge, U.S.A., January 10.

MODERN VIEWS OF ELECTRICITY.[1]

PART IV.—RADIATION.

XIII.

Possible Accounts of the Faraday and Hall Effects.

THE account I have given of the magnetic rotation of the plane of polarization has made it depend on the phenomenon of hysterĕsis, in a way which may be thus summarized. The value of μ for increasing magnetization is different from that for decreasing magnetization; an electric displacement such as occurs in every half-swing of a light-vibration is resolvable into two opposite circular components, one of which increases, while the other decreases, any magnetization already existing in the direction of the ray: the value of μ affects the speed of transmission of light; hence the two circular components will not proceed at the same pace, and the direction of vibration will infinitesimally rotate. The same thing is repeated at every half-swing, the elemental rotations being all in the same sense, and so the ultimate rotation of the plane of polarization in transparent bodies is accounted for.

[1] Continued from p. 13.

The Hall effect observed in conductors follows at once; for the rotation of a displacement is equivalent to combining it with a small perpendicular displacement; and it is this perpendicular or transverse E.M.F. exerted by a magnetic field which Hall discovered. At the same time, there are one or two facts which militate against this view of the Hall effect, chief among which is the singular behaviour of nickel, which rotates light one way and electric displacement the other way. For some time it was possible to hope for a way out of this through the usual convenient avenue of "impurity"; but now that both experiments have been performed on the same identical piece of metal, still with opposite results, this exit is closed. In this unsettled state, so far as I know, the connection between the rotation of light and the Hall effect at present stands.

It may be well here to repeat the caution, appended as a footnote to the last article, not to assume that this account of the magnetic rotation of light and the Hall effect is true. If true, however, it is convenient as linking the phenomena on to hysterēsis, and the direction of the effect in iron is correctly given—namely, a rotation against the magnetizing current.

Prof. Ewing has since pointed out, in a letter to me, that, attending more precisely to the instruction of his curves, we find the difference in μ for positive and negative magnetizing forces only lasts through a number of cycles for the time during which the final state has been approached, and does not persist after a steady state has been reached. This would make the magnetic rotation of light a function of time; and certain experiments by Villari on spinning a glass disk between the poles of a magnet, so that fresh and fresh portions of glass were continually exposed to the magnetic field, showed a marked falling off in the amount of rotation as soon as high speeds were obtained; thus proving, apparently, that a certain short time was necessary to set up the effect. This experiment, and other modifications of it, want repeating, however.

Prof. Ewing has subsequently expressed a doubt as to whether the kinematic resolution of a displacement into two equal opposite circular components is, under the circumstances, legitimate.

Prof. Fitzgerald has further pointed out that, although when attending to one element only the theory might possibly work, yet, as soon as one takes into account the whole wave-front, it breaks down; for all the main magnetic disturbance lies in the wave-front, as is well known, and the extra magnetic disturbance which I have postulated as a consequence of electrostatic displacement is annulled by interference of adjacent elements.

If I were quite sure that there were no vestige of truth in the suggestion I have made, I should, of course, withdraw it; but, as I do not feel perfectly sure either way, I leave it in a dilapidated condition for the present.

Another and apparently distinct account of the magnetic rotation can also be hinted at, which links the phenomenon on to the facts of thermo-electricity. It labours under worse disadvantages than the preceding, being more hazy.

Referring back to NATURE, vol. xxxvii. p. 12, we find that, to explain what is called the "Thomson effect" in metals, we were led to suppose a connection between one kind of electricity and some kinds of matter more intimate than between the other kind of electricity and the same matter. Thus, the atoms of iron were said to have a better grip of positive electricity than of negative; while copper, on the other hand, had a better grip of negative than of positive. All metals could be arranged in one or other of the two classes, with the exception of lead, which appears to grip both equally. It is the same phenomenon as was originally named by Sir W. Thomson, "the specific heat of electricity in a substance." Certain it is that vibrating atoms of iron push positive electricity from the more rapid to the less rapid places of vibration—that is, from hot to cold—and a whole class of the metals do the same; while another class, like copper, push it from cold to hot.

Permitting ourselves to picture this effect as a direct consequence of the Ohm's law relation between electricity and matter, combined with a special relationship between certain kinds of matter and one or other kind of electricity, a relationship which can exhibit itself in other ways also, we get a possible though rather hazy notion of a Faraday rotation in a magnetic field by supposing that the Amperian molecular currents in these substances consist not of precisely equal positive and negative currents, but of opposite currents slightly unequal; say, for instance, that the density of the positive constituent of the bound ether of a substance is slightly different from that of the negative constituent, so that on the whole the bound ether in a magnetized molecule is slowly rotating one way or the other, at a pace equal to the resultant rotation of its constituents. Suppose that in iron the positive Amperian electric current is the weaker of the two, then the ether, as a whole, will be rotating with the negative current, and accordingly an ethereal vibration entering such a medium will begin to screw itself round in a direction opposite to that of the magnetizing current. Whereas in copper or other such substance it would be rotated the other way.

According to this (admittedly indistinct) view, lead ought to show no rotatory effect at all; and of course, therefore, no Hall effect either. And the classes into which metals are divided by the sign of their Hall effect should coincide with the classes into which the sign of their Thomson effect throws them.

Hall finds that, of the metals he examined, iron, cobalt, and zinc fall into one class, while gold, silver, tin, copper, brass, platinum, nickel, aluminium, and magnesium fall into the other. Now, referring to the thermo-electric results of Prof. Tait, we find iron, cobalt, platinum, and magnesium with a negative sign to their Thomson-effect-coefficient, or with lines in the thermo-electric diagram sloping downwards; while gold, silver, tin, copper, aluminium, and zinc slope upwards, or have a positive sign to their "specific heat of electricity."

According to this, therefore, the discordant metals are zinc, platinum, and magnesium. The proper thing to say under these circumstances is that the metals used in the very different experiments were not pure. They certainly were not; but I do not feel able to conscientiously bolster up so inadequate a theory by help of this convenient fact.

In the *Philosophical Magazine* for May 1885, Mr. Hall gives some more measurements, showing that in bismuth the effect is enormous, and in the same direction as in copper, whereas in antimony it is also great, and in the same direction as in iron. All these things seem to point to some thermo-electric connection—whether it be of the sort I have vaguely tried to indicate, or some other.

Other Outstanding Problems.

Outstanding problems bristle all over the subject, and if I pick out any for special mention it will only be because I happen to have made some experiments in their direction myself, or otherwise have had my thoughts directed to them, and because they have not been so directly called attention to in the body of the articles.

Referring back to the end of Part II., "a current regarded as a moving charge," it is natural to ask, Is this motion to be absolute, or relative to the ether only, or must it be relative to the indicating magnetometer? In other words, if a charged body and a magnetic needle are flying through space together, as, for instance, by reason of the orbital motion of the earth, will the needle experience any deflecting couple?

It is one of many problems connected with the ether

and its motion near gross matter—problems which the experiments of Fizeau (showing that a variable part of ether was bound with matter and transmitted with it, while another constant portion was free and blew through it) began to throw light upon; aberration problems such as have been partially solved by the genius of Stokes; problems connected with the motion of ether near great masses of matter, like those which Michelson is so skilfully attacking experimentally: it is among these that we must probably relegate the question whether absolute or relative motion of electric charges is concerned in the production of magnetic field, and what absolute motion through the ether precisely means. It is doubtless a question capable of being attacked experimentally, but the experiments will be very difficult. I believe that Prof. Ayrton has attempted them.

Referring back to Parts I., II., and III., we find a number of questions regarding momentum left unsettled. Has an electric current any true momentum mechanically discoverable? Now, this question, before it can be answered in the negative, will have to be attacked under a great number of subdivisions. One may classify them thus. Two main heads: (1) When steady, Does a magnet behave in the least like a gyrostat? (2) When variable, Is there a slight mechanical kick on starting or stopping a current? With four or more subsidiary heads under each, viz. (*a*) in metallic conductors; (*b*) in electrolytes; (*c*) in gases; (*d*) in dielectrics.

Suppose the answer turns out negative in metals, it by no means follows that it should be negative in electrolytes too. In fact, as matter travels with the current in the case of electrolytic conduction, it is hardly possible that there is not some momentum, though it may be too small to observe—either a kick of the vessel as a whole at starting and stopping, or a continuous impact on an electrode receiving a deposit. The present writer has looked for these things, but after gradually eliminating a number of spurious effects the result has been so far negative. In a light quill vessel fixed to the end of a torsion arm, the main disturbance was due to variations of temperature which gradually introduced a minute air-bubble, and by kicking this backwards and forwards simulated the effects sought. In the case of the suspended electrode, convection currents in the electrolyte, caused by extra concentration or the reverse, seem determined to mask any possible effect.

One obvious though very troublesome source of disturbance in all cases is the direct effect of terrestrial magnetism on the circuit. To get over this, the writer not only made his circuits as nearly as possible of zero area, but also inclosed them in the iron case of a Thomson marine galvanometer, lent for the purpose by Dr. Muirhead.

In gases, the experiment of Mr. Crookes, where a stream of particles propels a mill inside a vacuum-tube—perhaps even the ancient experiment of the blast from a point—shows that momentum is by no means absent from an electric current through a gas.

To see if there are any momentum effects accompanying variation of electric displacement in dielectrics, the writer has suspended a mica-disk condenser at the end of a torsion arm, and arranged it so that it could be charged and discharged *in situ*. Many spurious effects, but no really trustworthy ones, were observed.

In the writer's opinion the subject is by no means thoroughly explored, and he only mentions his old attempts as a possible guide to future experimenters.

Then, again, there is the influence of light on conductivity. Annealed selenium, and perhaps a few other things, improve in conductivity enormously when illuminated. The cause of this is unknown at present, and whether it is a general property of matter, possessed by metals and other bodies to a slight degree, is uncertain; for the experiments of Börnstein with an affirmative result for the case of metals have been seriously criticized.

Even though metals show no effect, yet electrolytes might possibly do so, but the effect, if any, is small; and it is particularly difficult in their case to distinguish any direct radiation effect from the similar effect of mere absorbed radiation or heat.

The writer has found that a glass test-tube kept immersed in boiling water conducts distinctly better when the blinds of a room are raised than when they are lowered, though nothing but diffuse daylight falls upon it. But as the effect could have been produced by a rise in temperature of about the tenth of a degree, and as the absorption of diffuse daylight is competent to produce a rise of temperature as great as this in the glass of a thermometer-bulb even though immersed in boiling water, he feels constrained to regard the result, though very clear and distinct, as after all a negative one, and has accordingly not published it.

A few months ago I should have put in a prominent position among outstanding problems the production of electric radiation of moderate wave-length, and the performance with this radiation of all the ordinary optical experiments—reflection, refraction, interference, diffraction, polarization, magnetic rotation, and the like. But a great part of this has now been done, and so these things come to be now mentioned under a different heading.

Conclusion.

"Conclusion" is an absurd word to write at the present time, when the whole subject is astir with life, and when every month seems to bring out some fresh aspect, to develop more clearly some already glimpsed truth. The only proper conclusion to a book dealing with electricity at the present time is to herald the advent of the very latest discoveries, and to prepare the minds of readers for more.

Referring back to Part IV., vol. xxxviii. p. 418, we spoke confidently of a radiation being excited by electric oscillations, a radiation which travelled at the same rate as light, which is reflected and refracted according to the same laws, and which, in fact, is identical with the radiation able to affect our retina, except in the one matter of wave-length. Such a radiation has now been definitely obtained and examined by Dr. Hertz, of Karlsruhe, and in the last month of last year, Prof. von Helmholtz communicated to the Physical Society of Berlin an account of Dr. Hertz's latest researches. The step in advance which has enabled Dr. Hertz to do easily that which others have long wished to do, has been the invention of a suitable receiver. Light when it falls on a conductor excites first electric currents and then heat. The secondary minute effect was what we had thought of looking for, but Dr. Hertz has boldly taken the bull by the horns, looked for the direct electric effect, and found it manifesting itself in the beautifully simple form of microscopic sparks.

He takes a brass cylinder, some inch or two in diameter, and a foot or so long, divided into two halves with a small sparking interval between, and by connecting the halves to the terminals of a small coil, every spark of the coil causes the charge in the cylinder to surge to and fro about five hundred million times a second, and disturb the ether in a manner precisely equivalent to a diverging beam of plane-polarized light with waves about twice the length of the cylinder.

The radiation, so emitted, can be reflected by plane conducting surfaces, and it can be concentrated by metallic parabolic mirrors; the mirror ordinarily used being a large parabolic cylinder of sheet zinc, with the electric oscillator situate along its focal line. By this means the effect of the wave could be felt at a fair distance, the receiver consisting of a synchronized conductor with a microscopic spark-gap, across which the

secondary induced sparks were watched for. By using a second mirror like the first to catch the parallel rays and reconverge them to a focus, the effect could be appreciated at a distance of 20 yards. If the receiving mirror were rotated through a right angle, it lost its converging power on this particular light.

Apertures in a series of interposed screens proved that the radiations travelled in straight lines (roughly speaking, of course).

A gridiron of metallic wires is transparent to the waves when arranged with the length perpendicular to the electric oscillations, but it reflects them when rotated through a right angle, so that the oscillations take place along the conducting wires; thus representing a kind of analyzer proving the existence of polarized light. The receiver itself acted as analyzer, however, for if rotated much it failed to feel the disturbance.

Conducting sheets, even thin ones, were very opaque to the electrical radiation; but non-conducting obstacles, even such as dry wood, interrupt it very little, and Dr. Hertz remarks, "not without wonder," that the door separating the room containing the source of radiation from that containing the detecting receiver might be shut without intercepting the communication. The secondary sparks were still observed.

But the most crucial test yet applied is that of refraction. A great prism of pitch was made, its faces more than a yard square, and its refracting angle about 30°. This being interposed in the path of the electric rays, they were lost to the receiver until it was shifted considerably. Adjusting it till its sparks were again at a maximum, it was found that the rays had been bent by the pitch prism, when set symmetrically, some 22° out of their original course, and hence that the pitch had an index of refraction for these 2-foot waves about 1·7.

These are great experiments. As I write, they are but a month old, and they are manifestly only a beginning. They are very simple: I have repeated some of them already. They seem likely to settle many doubtful points. There has been a long-standing controversy in optics, nearly as old as the century, as to whether the direction of the vibrations was in, or was perpendicular to, the plane of polarization; in other words, whether it was the elasticity or the density of the ether which varied in dense media; or, in Maxwell's theory, whether it was the electro-magnetic or the electrostatic disturbance that coincided with that plane. This point has indeed by the exertion of extraordinary power been almost settled already, through the consideration of common optical experiments; but now that we are able electrically to produce radiation with a full knowledge of what we are doing, of its directions of vibration and all about it, the complete solution of this and of many another recondite optical problem may be expected during the next decade to drop simply and easily into our hands.

We have now a real undulatory theory of light, no longer based on analogy with sound, and its inception and early development are among the most tremendous of the many achievements of the latter half of the nineteenth century.

In 1865, Maxwell stated his theory of light. Before the close of 1888 it is utterly and completely verified. Its full development is only a question of time, and labour, and skill. The whole domain of Optics is now annexed to Electricity, which has thus become an imperial science.

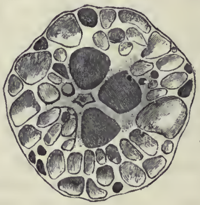
OLIVER J. LODGE.

A JAMAICA DRIFT-FRUIT.

IN 1884, I made a collection of drift-seeds and seed-vessels washed ashore on the spit of land inclosing Kingston Harbour, Jamaica, called the Palisadoes. The collection was sent to Kew, and it was utilized by Mr. Hemsley in the appendix to the "Botany of the *Challenger* Expedition," vol. i. pp. 277-304, when discussing the oceanic dispersal of plants. In the collection there was a fruit I had not seen before, and which was equally new to Mr. Hemsley and the other officers at Kew. There were several specimens of this fruit obtained, and these were afterwards placed in the Kew Museums amongst other drift-fruit, to wait until sufficient material was forthcoming to lead to their identification.

The general character of the drift-fruits collected at Jamaica points to the conclusion that they were brought by the Gulf Stream current from the mouths of the Amazon and Orinoco. Jamaica lies directly in this current, and for a long period fruits unknown in the island itself have been collected on its shores. The most striking of these is the fruit of *Manicaria saccifera*, a palm confined to Trinidad and the mainland of South America. Sloane ("Natural History of Jamaica," vol. ii. p. 186) noticed this fruit more than two hundred years


1.


2.


3.

Representation of a Jamaica drift-fruit (natural size). 1, external aspect; 2, cross-section; 3, longitudinal section.

ago, and states that "it is frequently cast up by the waves on the shores of this island, and it is one of those fruits thrown (also) on the north-west islands of Scotland by the currents and seas." None of these fruits appear to have germinated in Jamaica, for the palm, which is a most striking object, with entire leaves, could not fail to be noticed if it existed there. The fruits are known at Jamaica as *sea-cocoanuts*, while at Barbados they are called *sea-apples*. Other South American fruits found drifted at Jamaica were *Carapa guianensis*, *Dimorphandra mora*, and *Astrocaryum* sp. The collection of drift-fruits, although by no means a new subject, is one which has not received the attention it deserves. Since the publication of the "Botany of the *Challenger*," the active part played by the sea in the dispersion of plants has been more fully recognized. Mr. Hemsley has given a summary of results, in which many interesting facts have been brought to light, and doubtless observers in numerous parts of the world are now engaged in making observations.

But to return to the unknown drift-fruit. It is seldom that specimens remain at Kew for a long period without something occurring to call attention to them. It was so in this instance. In November 1887, Mrs. Hubbard sent Prof. Oliver a drift-fruit from the coast of Devonshire which was found to be exactly identical with the fruits collected at Jamaica. The specimen had the same water-worn appearance, and there was little doubt that it had travelled across the Atlantic in the drift of the Gulf stream.

This fruit, a representation of which is given opposite, possesses such distinctive characters that it can easily be recognized. It is evidently a drupaceous fruit, but the fibrous or fleshy outer layer (the sarcocarp) has either decayed or been worn away while drifting. What is now left is the woody indehiscent putamen or pericarp, externally marked by a mammillated surface corresponding, as shown in sections, with numerous cavities or resin-cysts existing in its walls. The fruit is normally five-celled, but two of these are suppressed, and only three remain. The seeds, as shown in Fig. 3, are solitary, and they possess abundant albumen. The presence of resin-cysts is a character of some value in seeking for the order or genus to which such a fruit could belong. In the Kew Museums, there are some fruits collected by G. Mann, on the Gaboon River, West Africa, which afford a clue. These are the fruits of *Aubrya gabonensis*, Baill, belonging to the natural order *Humiriaceæ* (Oliver, "Flora Trop. Afr.," vol. i. p. 275). They are smaller than the Jamaica drift-fruit, and are covered with a brown, fibrous or leathery epicarp 3 millimetres in thickness. The bony endocarp is, however, similarly developed and plentifully furnished with resin-cysts. The resemblance altogether is very close. The natural order *Humiriaceæ* is a very small one, and consists of genera entirely confined to Brazil and Guiana, with the exception of the single African genus already mentioned. There can be little doubt that the drift-fruit is derived from tropical America, and not from Africa. The American genera of *Humiriaceæ* are *Vantanea*, *Humiria*, and *Sacoglottis* ("Gen. Plant.," vol. i. p. 247). It is not necessary to give all the details, but I may say at once that Prof. Oliver is of opinion, and in this I agree, that the drift-fruit will doubtless be found to belong to a species of *Humiria*, and possibly to *H. balsamifera*, Benth. (Hook., *Kew Journ.*, vol. v. (1853), p. 102).

This species is said by Aublet (who figures the leaves and flowers only in "Plant. Guian.," p. 564, t. 225) to grow in all the forests of French Guiana, where it is known, from the colour of the wood, as *bois rouge*. Strange to say, the fruit of apparently so common a plant is unknown in European herbaria. Urban, who described it, with a figure, in "Flora Brasiliensis" (vol. xii. p. 439, t. 92), had not seen a mature fruit. The specific name is derived from the fact that the bark, when wounded, yields a reddish balsamic juice, possessing an odour like that of storax, and which after a time becomes hard and brittle. It is then used as a perfume. An ointment is also prepared from it.

For the present, therefore, we must leave the question open as regards the exact determination of this drift-fruit. There can be little doubt that it belongs to the genus *Humiria*, but until we obtain fruits of *H. balsamifera* we are unable to say whether it is that species or not.

D. MORRIS.

P.S.—Since the above was in type, I have gathered information which considerably increases the interest connected with this fruit. It is first figured and described (as a drift-fruit) by Clusius in his "Exoticorum libri decem," lib. 2, cap. 19. This work bears the date of 1605. The description adds nothing as to the origin of the fruit, except that it was received from Jacob Plateau. The figure given by Clusius, with the description, is reproduced by J. Bauhin, nearly eighty years afterwards, in "Historia Plantarum" (1680), tom. i., lib. 3, cap. cxi., Fig. 1. There is another figure, still from a water-worn and drifted specimen, given in "Petiveri Opera," tab. lxxi., Fig. 5 (1764), with the information that "it is a hard oval fruit with seed-holes [resin-cysts] round its surface. Cat. 605. Found on the shore of Jamaica." Finally, Mr. E. G. Baker, F.L.S., to whom I am indebted for the references to Bauhin and Petiver, has recognized the fruit in the Sloane Collection at the British Museum (Natural History), labelled "No. 1656." We are still, however, without information as to the origin of the fruit or the plant bearing it.

D. M.

HAZE.

I HAVE for some time given in my lectures an explanation of the common summer haze which appears to me to be very probable. I do not know whether it is new, but it has not been referred to in the discussion raised by Prof. Tyndall's letter on Alpine haze. Some time since I mentioned it to Prof. Lodge, and at his suggestion I send it to you, though its extension to other kinds of haze is somewhat speculative.

It is that haze is often due to local convection currents in the air, which render it optically heterogeneous. The light received from any object is, therefore, more or less irregularly refracted, and, through the motion of the currents, its path is continually varying. The outline of the object, instead of appearing fixed, has a tremulous motion, and so becomes ill-defined. At the same time, reflection occurs where there is refraction at the surfaces of separation of heterogeneous portions. Much of the light which, in a homogeneous medium, would come straight from the object, is thus lost for direct vision, and the contrast between neighbouring objects is lessened. The reflected light is diffused as a general glare. The combination of the quivering of outline, and the loss of direct light, with the superposition of the reflected light as a diffused glare, gives the appearance we call haze.

This explanation appears to me to accord well with the obvious facts of summer haze—the haze which is seen in the middle of a hot, cloudless, summer day. The lower layers of air, being heated by contact with the earth, rise in temperature till equilibrium is no longer possible, and convection begins, streams of the heated air rising, and streams of colder air falling to take its place. The variation of temperature and density gives optical heterogeneity. The existence of these streams is sometimes shown by the quivering of distant objects, looked at through the air close to the ground, but a telescope will often show the quivering of outline at higher levels, and when quite invisible to the naked eye. Accompanying this refraction, reflection must occur. We have a direct proof of its occurrence in the fact that the glare is greatest under the sun, where reflection occurs at angles approaching grazing incidence, for which it is a maximum; while it is least opposite the sun, where reflection occurs at angles approaching normal incidence, for which it is a minimum. The opening lines of the "Excursion" perfectly describe the resulting appearance:—

> "'Twas summer, and the sun had mounted high;
> Southward the landscape indistinctly glared
> Through a pale steam; but all the northern downs,
> In clearest air ascending, showed far off
> A surface dappled o'er with shadows flung
> From brooding clouds."

During the night the lower strata become colder than the upper ones, and the atmosphere passes into a state of stable equilibrium. We should therefore expect that, if the foregoing explanation is true, there would be

complete absence of haze, and it is well known that the air is peculiarly clear in early morning, when we get above the fog-level.

According to this account of heat haze, it stands in sharp contrast to fog, of which it is so often supposed to be a relative in reduced circumstances. While the one requires convection, the other usually occurs when the air is in stable equilibrium, the lowest strata being the coldest. In the fog, for example, which so frequently heralds or accompanies the break-up of a frost, the lower strata are still cold, while above the wind has changed, and the air comes up warm and vapour-laden. The vapour diffuses downwards into the lower, cold strata, and is there condensed, and it is possible that the more rapid diffusion of water vapour has something to do with the continuance of the fog, for it would diffuse downwards more rapidly than the rest of the air with which it has come.

There are other cases of haze which may, perhaps, be explicable by optical heterogeneity. In the dry east winds of spring we frequently have a haze when the air is far from saturation-point, and the clouds, if they exist, are at a high level. It appears possible that this haze is due to small convection currents of the cold air from above, the temperature falling too rapidly from below upwards for equilibrium.

Sometimes the heterogeneity may be due to water vapour. After rain, when the ground is still wet, the drying of exposed surfaces often shows that the air is not saturated, yet there is a haze or mist which is supposed to be thin fog, *i.e.* water-dust. Evaporation must be going on, and the air must certainly be unequally charged with vapour. With this inequality there must also be convection. I have never been able to detect, with certainty, quivering of outline either in this case or in the previous one of the east wind haze, though I have sometimes suspected its existence. Possibly someone who has used a high-power telescope for terrestrial objects might be able to give information on this point. But it is to be noted that the differences of density in both these cases are much smaller than in the case of summer haze, and the currents should therefore be on a smaller scale.

It seems worthy of inquiry whether the haze observed under cumulus clouds, referred to by the Rev. W. Clement Ley, may not also be due to water-vapour heterogeneity. The cumulus cloud indicates the existence of a large body of vapour-laden air extending no doubt below, as well as above, the condensation level. As this mass sweeps along, the lower part of it is retarded by the earth and by the lower strata, and more or less disturbance and mixture with the surrounding air will occur. There will therefore be heterogeneity. I do not know whether this would be at all sufficient to account for the haze observed, but the suggestion seems worth considering.

J. H. Poynting.

THE EARTHQUAKE AT EDINBURGH.

Though no earthquake of great destructive energy has passed across the area of the British Islands within historic times, the number of actual shocks which have been experienced and recorded amounts now to many hundreds. Scotland, being more generally mountainous than the other divisions of the United Kingdom, has hitherto enjoyed by far the largest share of these manifestations of terrestrial disturbance. Hardly a year passes without adding to the list. The latest addition is that which startled the inhabitants of Edinburgh, on Friday morning, the 18th instant, a few minutes before 7 o'clock, when a large part of the population was still in bed. It agreed in general character with the usual type of Scottish earthquakes, and might indeed be taken as an illustrative example of them—not strong enough to do damage to property or life, but yet quite sufficiently marked to arrest attention and to exhibit some of the more prominent features of seismic movements.

The earthquakes that visit Scotland so frequently are always singularly local in character. Now and then a more vigorous shaking may be felt across the breadth of the Highlands, but as a rule the tremor is confined to a comparatively circumscribed district. Again, not only are they local, but there is usually some centre or median belt of ground where the effects have been more distinctly perceptible than elsewhere. Some districts are specially liable to them. Thus, the tract around Comrie, in the south of Perthshire, has long been noted for the frequency of its earthquakes. Another area often similarly affected embraces the West Highlands up to Inverness. It is worthy of remark that this latter region was considerably disturbed by a series of shocks almost exactly a year before, viz. on February 2, 1888. If we look at the geological structure of these earthquake districts, we observe that they are traversed by some of the most important lines of fissure by which the crust of the earth within the area of Britain has been disrupted. Comrie stands on the line of a powerful fault which runs across Scotland from sea to sea, and which, by bringing the softer and less prominent sandstones of the low country against the harder and more precipitous schists of the hills, has been in some measure the origin of the line between the Lowlands and Highlands, and thus of the limitation of the Saxon and the Gael. Again, in the West Highlands, a profound dislocation has defined the line of the Great Glen from Inverness to the Linnhe Loch. The relation of the chief earthquake centres to these ancient lines of fracture in the terrestrial crust can hardly be accidental. The actual shock that starts the seismic wave is probably in these areas to be traced to some slipping of the rocks along these lines of rupture. Though the faults are of great geological antiquity, they doubtless remain lines of weakness, along which any changes due to the secular cooling and contraction of our planet may be expected to show themselves.

The area shaken by the earthquake of Friday, January 18, is not one which has been much subject to experiences of this kind, though a few shocks are spoken of as having been felt there in the past. But its geological structure shows how well fitted it is to become a theatre of disturbance of the usual feeble Scottish type. It extends from the edge of the Firth of Forth southwards into the interior across the site of Edinburgh, and thence along the chain of heights known as the Pentland Hills. These hills consist mainly of volcanic rocks belonging to the period of the Lower Old Red Sandstone. The beds of lava and sheets of tuff increase in thickness towards the north-east, until in the Braid Hills, which lie immediately along the southern outskirts of Edinburgh, they are replaced by the materials that fill up what seems to have been the chief vent from which they were discharged. These volcanic eruptions had long ceased when the Carboniferous rocks of the district began to be laid down upon their upturned and worn edges. During a prolonged period of deposition and depression they gradually sank and were buried under some thousands of feet of sedimentary strata. In later times the area was disrupted by a group of long parallel fractures running in a general north-east and south-west line. Vast denudation likewise took place. By degrees the deep cover of Carboniferous strata was worn away from the tops of the volcanic ridges which now once more appear at the surface as the chain of the Pentland Hills. Thus the site of the earthquake coincides with an ancient volcanic centre and with a belt of country which has been dislocated by lines of fault.

The area within which this earthquake was most perceptible is tolerably well defined by that of the geological features here described. It is rudely elliptical in shape, about fifteen or twenty miles along the longer, and from five to seven across the shorter axis. Outside these limits, the shock, though in some places distinctly felt, seems to have rapidly lost force, so that at Linlithgow, about ten miles away, it was hardly perceptible. The effects by which the passage of the seismic wave made itself sensible were of the usual kind. For the most part all that was noticed was a tremor or quivering movement of the ground or houses, accompanied with the rattling of any loose objects. I was myself awake, and about to rise, when I was aware of a peculiar grinding noise and the jangling of all the crockery and glasses on the wash-hand-stand. It seemed as if some heavy load had been discharged from a waggon on the ground immediately outside the house. But as the position of the house on the outskirts of the southern suburbs of the city made this impossible, I was at a loss to conjecture the cause of the disturbance. I thought of an earthquake, but found that no other inmate of the house had noticed anything peculiar. In the course of the day, however, accounts came from all parts of the district affected, which put the real nature of the event beyond any doubt. The undulation of the ground was experienced by many observers, and the general impression was that the wave moved from west to east, or from south-west to north-east. Some who were not yet astir felt first one end of the bed and then the other depressed and raised. To others who were already on foot the ground seemed to sink away from their feet and to rise upward again. Chairs appeared to slide forward and backward. Pictures, lamps, and other hanging objects were set swinging. In one recorded case, an eight-day clock, which had been standing at the time, and of which the pendulum swings in an east and west line, was made to go. On the other hand, the parish clock of Currie was stopped at the moment of shock. One boy is said to have been pitched out of bed, and was found immediately afterwards with a hammer in his hand ready to attack someone whom he supposed to be concealed under the bed. Numbers of witnesses spoke of the feeling of giddiness or nausea that so frequently accompanies earth-movements. Dogs barked or howled to mark their sympathy in the general alarm. No damage of any kind, however, seems to have been done, unless under that name be included the dislodgment of a portion of the plaster from the ceiling of a church at Portobello.

The most violent motion appears to have been felt along the north-west base of the Pentland Hills and thence in a north-easterly direction through the northern parts of Edinburgh. There can be little doubt that the source of the shock lay somewhere along the line of these hills. We may plausibly connect it with the failure of support, and consequent fall, of some part of the rocks along one of the main lines of fault that define the chain of the Pentlands.

The greater frequency of earthquakes in the winter half of the year has often been remarked upon. It is certainly during that season that they most generally occur in Scotland. With regard to the shock of last week, we find that a strong gale had been blowing for some time, and that the barometer had fallen four-tenths of an inch between Thursday and Friday. The position of the moon, according to some seismologists, conduces to the determination of those disturbances of the terrestrial crust that give rise to earthquakes. With regard to the disturbance of the 18th instant it is noticeable that full moon happened the previous morning, when there was a lunar eclipse. It is about the time of new moon and the first quarter that the chief earthquake-maxima are said to take place.

A. G.

DECOMPOSITION OF NICKEL AND COBALT.

CHEMISTS have recently heard with great interest that Dr. Krüss, of Munich, a well known and trusted worker, has succeeded in "decomposing" nickel and cobalt. Judging from the accounts which thus far have reached us, it would appear that his discovery consists in finding that cobalt and nickel contain about 3 per cent. of a new element which is common to both as ordinarily prepared. By the removal of this hitherto unrecognized "impurity," the properties of the cobalt and nickel salts are slightly modified as to colour, &c. It is to be expected that the discovery will serve to explain the discrepancies which are to be noted among the various determinations of the atomic weight of nickel and cobalt. We give the following details from a short note upon the subject in the *Chemiker Zeitung* of January 27:—

Dr. Krüss, in conjunction with Herr Schmidt, read a paper at the meeting of the Munich Chemical Society of January 11, upon the results of their attempted redetermination of the atomic weights of these two metals. Clemens Zimmermann, as the outcome of a determination made some years ago, found the atomic weight of cobalt 58·74, and that of nickel 58·56. The method followed by Krüss and Schmidt was a revival of the older process of Winkler, and consisted in determining the relative equivalents of gold and nickel, and of gold and cobalt. Such a method ought now to be very much more trustworthy, on account of the recent exceptionally accurate determinations of the atomic weight of gold made simultaneously by Dr. Krüss himself, and by Prof. Thorpe and Mr. Laurie in this country. The process simply consisted in precipitating the gold from a neutral solution of gold chloride or sodium gold chloride by weighed quantities of metallic nickel or cobalt, chlorides of the two latter metals passing into solution. The results, however, were not what were expected, and differed among themselves to a remarkable extent. It was found impossible to precipitate the equivalent in gold of the nickel or cobalt employed, owing to the inverse effects of polarization, small quantities of metallic nickel or cobalt being reprecipitated, and thus mixed with the finely divided gold. In order to determine the amount of metal which had been dissolved, the precipitate was weighed, dissolved in aqua regia, and the gold reprecipitated by sulphurous acid. The weight of gold thus obtained, deducted from the weight of the whole precipitate, of course gave that of the admixed nickel or cobalt. Even after making this correction, the method afforded such surprisingly untrustworthy values that it became necessary to seek for the disturbing cause. On washing the reprecipitated gold, it was noticed that the red colour of the filtrate due to chloride of cobalt gradually paled, and eventually took a greenish tint. On evaporating a quantity to dryness and igniting in a platinum dish, the small residue obtained was found to dissolve in warm concentrated hydrochloric acid forming a beautiful green solution. On cooling, the liquid again became almost colourless, and gave a white precipitate of hydrate on addition of ammonia, which on ignition gave a white oxide; and a brownish-black precipitate of sulphide with ammonium sulphide. A number of other reactions also show that the compounds of this metal, which was itself obtained as a black powder by reduction of the oxide with charcoal, are not to be identified with those of any known element. The most interesting fact, however, is that the washings from the gold obtained in the nickel determinations gave precisely the same indications; the same residue on ignition, a green solution on treatment with warm hydrochloric acid, and the same hydrate and oxide. Several other methods were also described in the memoir, which we hope shortly to see published, by which this same common ingredient could be extracted from both nickel and cobalt. It is very interesting that Dr. Krüss has also obtained a green double chloride of the

new metal and red cobalt chloride, which possesses all the properties of green nickel chloride, which has thus been decomposed into a red and a colourless salt.

NOTES.

THE Royal Society has recently entirely recast the regulations under which the Government fund of £4000 for the promotion of scientific research is administered.

MR. COMMON'S 5-foot telescope is now completed, and photographs of the moon and nebulæ have already been taken to test the figure of the silver-on-glass speculum. We hear the results are quite satisfactory. Congress is to be asked to vote 250,000 dollars for the purchase of a refractor of the same dimensions for the Washington Observatory.

MR. ISAAC ROBERTS, working on the lines laid down by Mr. Common, has recently, by exposures of over four hours, obtained most important additions to our knowledge of the nebulæ of Orion, Andromeda, and the Pleiades.

WE believe that the Royal Astronomical Society has this year awarded its medal to M. Lœwy, of the Paris Observatory.

PROF. CORFIELD has been elected an Honorary Member, and Dr. Louis Parkes a Foreign Associate, of the Société Française d'Hygiène.

LAST week's number of the *Electrician* contains a notice of the work of Sir William Thomson, accompanied by an admirable steel engraving.

THE new Laboratory at the Normal School of Science, built for the accommodation of the students in the practical courses in physics and astronomical physics, is now finished. It accommodates about eighty students.

THE Medals and Funds to be given at the anniversary meeting of the Geological Society on February 15 have been awarded by the Council as follows: the Wollaston Medal to Prof. T. G. Bonney, D.Sc., F.R.S.; the Murchison Medal to Prof. James Geikie, F.R.S.; the Lyell Medal to Prof. W. Boyd Dawkins, F.R.S.; and the Bigsby Medal to Mr. J. J. Harris Teall; the balance of the Wollaston Fund to Mr. A. Smith Woodward, of the British Museum; that of the Murchison Fund to Mr. Grenville A. J. Cole, of the Science Schools, South Kensington; and that of the Lyell Fund to M. L. Dollo, of the Royal Museum at Brussels.

AT the last meeting of the Mineralogical Society, there was described a new mineral species, an oxychloride of lead, to which the name of "daviesite" was assigned. This mineral was found as very minute crystals in a specimen from Sierra Gorda in Bolivia. The crystals are very rich in faces, and belong to the ortho-rhombic system; their parametral ratios are $a : b : c = 1·2594 : 1 : 0·6018$; they are elongated in the direction of the vertical axis, parallel to which there are faces of the prism (110) and the pinacoid (100), and they are terminated sometimes by a simple basal plane (001), sometimes by the domes (011), (031), (101), (301), (501), and the pyramids (211), (121), (221), (521). The optic axes are visible when the basal plane is normal to the axis of the convergent polarized light.

THE Scientific Department of the Scotch Fishery Board, in view of the approaching great spawning period of the marine food-fishes, have begun a series of systematic investigations at some of the more important fishing-grounds lying off the east coast of Scotland. From a report presented to the Fishery Board by Prof. Ewart, it appears that one of the Board's naturalists (Mr. T. Scott) has recently made some interesting observations on board the large steam-trawler *Southesk*, of Montrose, as to the spawning of the plaice (*Pleuronectes platessa*) at Smith Bank, a well-known ground in 20 fathoms of water, lying off the Caithness coast, where operations were carried on for several days. A great variety of fish were obtained; but, except a few gurnards, only the plaice, which were present in great numbers, were spawning. Specimens both quite ripe and partly spent were captured by the trawl, while the tow-net revealed the presence of countless multitudes of floating eggs on the surface—in all stages of development—as many as 10,000 being obtained in one haul. These were shown to be almost entirely the ova of plaice, the remainder being the ova of the gurnard. Such an opportunity of witnessing the spawning of a shoal of flatfish on so gigantic a scale, and of proving so clearly the relationship between the spawning fish and the pelagic ova, does not often occur.

A BOTANICAL STATION was established early last year at St. Lucia on the most unpretentious footing. A Committee appointed to supervise the work of the Curator, Mr. John Gray, reports that, "considering the difficulties incidental to the starting of such an undertaking, the peculiar nature of the ground, and the limited funds available for the purpose, the work thus accomplished is satisfactory." The Committee says that the most encouraging feature of Mr. Gray's report is the general appreciation shown by the agriculturists of the district in the success of the Station, as shown by their increasing disposition to seek advice from the Curator, and to try and obtain seeds from him. Already the demand for cocoa, coffee, and nutmeg plants is so great that additional land will have to be acquired for the extension of the Garden.

LORD WOLSELEY, who is not often caught tripping in making hasty statements, writes as follows in the current number of the *Fortnightly Review*:—"The battles of the future will be very different from even those of 1870. . . . One remarkable change will be the absence of nearly all that terrific noise which the discharge of five or six hundred field guns, and the roar of musketry caused in all great battles. . . . The sound of cannon will be slight, and will no longer indicate to distant troops where their comrades are engaged, or the point on which they should consequently march. Our sentries and advanced posts can no longer alarm the main body upon the approach of the enemy by the discharge of their rifles. The camp or bivouac will no longer be disturbed at night by the spluttering fire of picquets in contact with the enemy. Different arrangements for giving the alarm upon the approach of hostile columns will have to be resorted to. The main column on the march cannot in future be warned, by the shots of flanking parties, of the enemy's proximity, and a battle might possibly be raging within a few miles of it without that fact becoming at once apparent." Will some competent member of the "Scientific Corps" kindly explain, or are they all in civil employ?

WE make the following extract from a letter addressed by Mr. A. W. Tuer to a contemporary:—"The melodious hum of skating was perhaps never heard to greater advantage than through the crisp air of a bitterly cold morning little more than a fortnight ago—the first Sunday in the year. Almost as soon as Kensington Gardens were entered, one became conscious of a clearly-defined musical sound coming from the direction of the Round Pond—G as nearly as I could judge, but corrected to G sharp, when, half an hour later, I got to a piano. I had wished to compare the notes—probably lower—given forth by other and larger sheets of ice, but procrastination strangled an opportunity which perhaps others will take when it again offers. Comparing a sheet of ice to a taut string, and the countless skates to the hairs of a bow—scientifically, a poor comparison enough—the sound might be expected to have been like that produced by the scraping of a fiddle, but it exactly resembled the whistle of a distant locomotive."

Prof. J. H. Gore, of the Columbian University, has in preparation a bibliography of geodesy. During two trips to Europe he has collected about seven thousand tit'es, having examined nearly every large library except that at St. Petersburg. He begins with the first effort to ascertain the shape of the earth by triangulation in the seventeenth century. The work will be published soon by the Coast Survey. Prof. Gore is trying to make his service complete by personal application for data, he having written to all astronomers and other mathematicians in the world whose addresses he could obtain.

The American Society of Naturalists (*Science* reports) held in Baltimore, on December 27 and 28, one of its largest and most successful meetings. Methods of instructing large classes in botany were presented by Profs. Goodale and Wilson, and in geology by Profs. Niles and Williams. The Society fully approved the excellent work of its committee on education, in paving the way for better instruction in the natural sciences in all grades of schools, especially the lower ones. Mr. J. E. Wolff showed a photographic method of class illustration, and Prof. W. M. Davis explained a most interesting series of paper models, illustrating the development of certain topographic forms and their relation to base-levels of erosion. The Society is composed largely of teachers, and desires to so arrange its meeting next year that the members may be able to attend the meetings of specialists held about the same time.

We referred recently to the investigations in the Torres Straits of Prof. A. C. Haddon, of the Royal College of Science, Dublin. The *Ceylon Observer* now publishes extracts from some later letters from Prof. Haddon, from which it appears that he was anxious to spend some time in Ceylon, to work out the life-history of the Ceylon turtle and to add to his collections; but it is unlikely that he will be able to do more than call at Colombo on his way home. Writing in November from Thursday Island, Prof. Haddon says:—"I have now been out nearly three months, and have had a very pleasant time, and have seen and learnt much. I find that the anthropology is an untouched field, absolutely so as regards the manners and customs of the past, and I have taken some trouble and a great deal of interest in collecting what information I can in the short time at my disposal. In most of the islands the people are dying off; the younger men know nothing of the life of their forefathers, and there are but few old men left. In a few years' time it will be too late, so I have deemed it desirable to turn some of my attention to that subject, although I had no intention of so doing when I started. Experts at home must judge whether my information is of value. I have had a little peep at New Guinea for a few hours—Mowat—to this end. In April and May I am going to the Louisiades, Sudest and other islands of the south-east end, and also to Port Moresby and Motu-Motu. Mr. Chalmers strongly urged me to give an extra month to that end of New Guinea, promising to take me about, an opportunity which money could not purchase, and which falls to the lot of very few. So I shall conclude my stay in this part of the world with a good look at New Guinea under the best of guidance. It has been a great grief to me that I could not manage Ceylon as well, but I feel I have done quite right in not yielding to the strong temptation."

Experiments have recently been made (says *Science*) showing in what order a fatigued eye recovers the power of perceiving different colours. The important factor is what colour has been used to induce fatigue. If the eye has been fatigued by long exposure to red, the sensitiveness for green is the first to reappear, then for blue, then yellow, and finally red. After a "blue-fatigue," the order is yellow, red, green, blue; after a "green-fatigue," the order of recovery is red, blue yellow, green; after "yellow-fatigue," it is red, blue, green, yellow. The eye recovers last the perception of the colour by which the fatigue has been induced, and first recovers the sensitiveness for the complementary colour. The fatigue is in the retina, for it is an independent phenomenon in the two eyes. The point of finest vision, the fovea, requires a longer time to recover from colour-fatigue than the less sensitive lateral portions of the retina. The physiological process is considered to be related to the visual purple of the rods and cones. On the sense of taste, the same journal states that in the case of a patient whose entire tongue, including the large circumvallate taste-papillæ at the root of the tongue, had been removed, it was found that some power of taste remained. The sensations of sweet, bitter, and sour could be obtained by applying appropriate substances to the back of the pharynx or the stump of the tongue, though if applied to the tongue the taste was apparent only during swallowing. The taste of salt was not perceived. Though these results are not fully in harmony with previous experiments, they are helpful in localizing the tasting-powers of various portions of the mouth cavity.

The Indian papers report that a severe earthquake, lasting about four seconds, followed by slight momentary shocks at intervals, occurred at Quettah on the morning of December 28. The shocks were felt at intervals till December 31. No injury, beyond the destruction of a few native shops and bazaars, is reported.

Turkestan has again been the scene of earthquakes. On November 28 last, at 11.40 a.m., a shock of earthquake, much stronger than any lately experienced, was felt at Tashkent. After a feeble earth tremor, which lasted for four or five seconds, there was a violent shock; the houses cracked, windows rattled, and the inhabitants rushed into the street. The wave of the earthquake came from the east, and at Khojent some houses were damaged.

Prof. Mushketoff has made a gift to the Russian Geographical Society of a very interesting album of 175 photographs, showing the effects of the last great earthquake at Vyernyi. It illustrates with perfect accuracy, the damage done in the houses, as also several geological changes due to the earthquake.

A member of the Astrakhan Scientific Society has been taking photographs of fishermen at work at the mouth of the Volga, and of the inplements used by them. An album of 200 photographs gives a complete representation of the present state of these important fisheries, and a copy is to be deposited at the St. Petersburg Academy of Sciences.

In December last, a Chinese scientific expedition, under the learned functionary Miao, arrived at Irkutsk.

At a recent meeting of the Geological Society of Stockholm, Dr. N. O. Holst exhibited the forehead and part of the leg of the skeleton of a bison found in a bog near Vadstena. The discovery was made by a farmer as far back as 1865, but it has only recently been proved that the parts are those of a bison. Only two similar discoveries have been made in Sweden, viz. in the province of Scania. Baron de Geer maintained that recent careful researches disproved the theory held by some that a sound had in prehistoric times separated Scania from the rest of Sweden, and thus prevented the immigration of the bison thither.

In a recent British Consular Report on the Agriculture of the Department of the Maritime Alps, Consul Harris says that the reckless destruction of the forests had already considerably altered the climate and other conditions of that region in the first half of this century, and had caused a large portion of the soil, or the "flesh of the mountains," as Elisée Reclus calls it, to disappear from the summits, which are now only the most barren slopes. When the snows of winter melt off the higher mountains, devastating floods are very common, and the population has, within the past twenty years, decreased very

much on account of these floods. It has been calculated that in one year alone the amount of soil carried down by the two rivers, the Loup and the Paillon, was sufficient to have covered the whole department to a depth of 6 centimetres. The Consul adds that, though something has been done to encourage replanting, a more serious effort than has yet been made is needed to reafforest the country.

FROM a recent British Consular Report on the trade of Maranham, it appears that a large number of india-rubber producing trees were discovered last year in the district of Pinheiro. The principal traders, who were suffering severely from the depression in the sugar and cotton industries, formed themselves into a company to work up the new discovery, but up to the present the trees have produced little.

THE *Ceylon Observer*, commenting on the destruction wrought by the scaly insect on the cocoa-nut palm in the West Indies, says that it is most remarkable that in Ceylon the palm has been absolutely free from this and other such pests. This fact perhaps is due to the absence of those long-continued droughts which have so grievously affected Jamaica and its neighbouring islands. While the palm, however, has flourished so well in Ceylon, the coffee-plant is almost extinct in the island, a few isolated fields in each district being all that have survived the ravages of the green scale insect. Frequently the finest coffee-trees, with fresh and vigorous-looking bushes, and with stems as thick as a man's thigh, are so completely under the influence of the pest that no crop is produced. Experiments with soap, lime, kerosene oil, &c., have up to the present produced no satisfactory results. Tea seems to flourish where the coffee-plant dies, and even where the tea plantations are attacked by fungoid or insect pests, the plants can be pruned down till not a leaf is left; or, if the attack is a very severe one, the tea-garden can be burnt to the ground without suffering any permanent injury, for the roots and stems in a very few months again display their luxuriance as richly as before.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, presented by Mr. Lishman; a Serval (*Felis serval*) from Malindi, East Africa, presented by Mr. H. C. Hunter; a Common Fox (*Canis vulpes* ♂), British, presented by Mr. E. Baldwin Cashel; a Common Fox (*Canis vulpes* ♀), British, presented by Lieutenant H. F. Sparrow, "The Buffs"; a Chough (*Pyrrhocorax graculus*) from Ireland, presented by Mr. A. Mudge; a Gold Pheasant (*Thaumalea picta* ♂), a Silver Pheasant (*Euplocamus nycthemerus* ♂) from China, presented by Mrs. Theodore Lloyd; a Sharp-nosed Crocodile (*Crocodilus acutus*) from the West Indies, received in exchange.

OUR ASTRONOMICAL COLUMN.

ROUSDON OBSERVATORY, LYME REGIS.—The observations at Mr. Peek's private observatory have been carried on during the past year. 163 nights were available for observations, as compared with 165 in 1887. As last year, the attention of the observers has been chiefly directed to transit-observations for time, and to observations of variables of long period. The object of the observations of variables is to determine the exact dates of maxima and minima, and, as far as possible, the light-curve of each star. The list of stars under examination is gradually being revised, circumpolar stars being substituted for those withdrawn, in order that uninterrupted observations may be made throughout the year. Owing to the lengths of the periods of the stars taken, the complete observations are not yet ready for publication. The importance of these light-curves cannot be over-estimated, as they will undoubtedly throw much light on the origin of the variability.

A sidereal clock, by Grubb, has been added to the equipment of the observatory.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 FEBRUARY 3–9.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 3

Sun rises, 7h. 36m.; souths, 12h. 14m. 7·4s.: sets, 16h. 52m.: right asc. on meridian, 21h. 9·6m.; decl. 16° 22′ S. Sidereal Time at Sunset, 1h. 48m.

Moon (at First Quarter February 7, 21h.) rises, 9h. 25m.; souths, 14h. 58m.; sets, 20h. 43m.: right asc. on meridian, 23h. 54·3m.; decl. 5° 49′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	8 3	13 19	18 35	22 14·9	9 24 S.
Venus.....	9 0	15 7	21 14	0 3·3	0 40 N.
Mars......	8 47	14 26	20 5	23 22·3	4 51 S.
Jupiter....	5 8	9 3	12 58	17 57·8	23 6 S.
Saturn....	16 53*	0 26	7 59	9 19·7	16 48 N.
Uranus...	23 5*	4 28	9 51	13 22·1	7 58 S.
Neptune..	11 11	18 54	2 37*	3 50·8	18 25 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb	h.	
3	20	Venus in conjunction with and 5° 37′ north of the Moon.
5	12	Saturn in opposition to the Sun, southing at midnight.
5	12	Mercury stationary.
8	16	Neptune stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Feb. 7,	19 50	*m*
Algol	3 1·0	40 32 N.	„ 8,	4 10	*m*
λ Tauri	3 54·6	12 11 N.	„ 5,	21 55	*m*
R Canis Majoris	7 14·5	16 11 N.	„ 6,	21 16	*m*
			and at intervals of	27 16	
W Virginis	13 20·3	2 48 S.	Feb. 9,	5 0	*m*
S Virginis	13 27·2	6 38 S.	„ 9,		*M*
U Coronæ	15 13·7	32 3 N.	„ 7,	22 52	*m*
R Draconis	16 32·4	66 59 N.	„ 5,		*m*
T Vulpeculæ	20 46·8	27 50 N.	„ 7,	19 0	*m*
			„ 8,	21 0	*M*
Y Cygni	20 47·6	34 14 N.	„ 3,	17 40	*m*
			and at intervals of	36 0	

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near η Aurigæ	74	42 N.	
„ λ Draconis	168	71 N.	
„ θ Draconis	240	61 N.	February 6.

GEOGRAPHICAL NOTES.

THE paper read at Monday's meeting of the Royal Geographical Society was on the Gran Chaco of the Argentine Republic and its rivers, by Captain John Page, of the Argentine Navy. The Gran Chaco, Captain Page said, is a vast central tract of country lying between the southern tropic and 29° S. lat., bounded on the north by Brazil and Bolivia, on the south by the Argentine province of Santa Fé, on the east by the Paraná and Paraguay Rivers, and on the west by Santiago del Estero and Salta. It contains about 180,000 square miles, or considerably more than the superficies of Great Britain and Ireland. About one-third part of this vast area belongs to Paraguay. The Gran Chaco has been called, particularly in allusion to the low-lying Paraguay section, the *oceano firme*, or solid ocean. This section and the central section of the Argentine rise from the Paraguay River towards Bolivia almost imperceptibly, having numerous and very extensive marshes and jungle, which are drained by many small streams likely to become, as the country progresses, important local waterways. The monotonous level of these sections is relieved by various prominent points of great beauty along the Paraguay River. Both are well wooded, although the predominating woodland

feature is the great and almost interminable palm forests, which, singularly enough, in the Chaco are a sure indication of marshy lands subject to inundation, although in the province of Entre Rios, and other parts of the world, they are the exact contrary. On the northern and eastern borders of the River Bermejo the Central Chaco rises sensibly, as if to form a barrier to the waters of that river in their easterly progress. The Chaco Austral of the Argentine is the most favoured in natural riches of the three great sections. Its surface rises gradually from the Paraná River, and is intersected by several small streams, which are even now useful as a means of water-carriage to the many colonists settled along their courses; after rising thus up to the parallel of 25° 40′ S., the ground dips towards the valley of the San Francisco, sending its waters with those of that river to the Bermejo, sometimes in untimely floods. This depression extends across the Central into the Paraguayan Chaco, taking in the sections of the two rivers that are subject to yearly overflows between long. 61° and 62° W. of Greenwich, thus making a point of analogy between the two. The Austral is favoured with extensive primæval forests, notably that on the north-western border extending into Salta and covering a superficies of many hundreds of miles, quite unexplored, and sometimes designated by the name of "impenetrable." The principal water-courses of these territories are the Pilcomayo and Bermejo, which are undoubtedly destined to become highways of commerce. The waters of these rivers differ in colour, those of the Pilcomayo being dark and sometimes brownish, and those of the Bermejo red, as its name indicates; both are long, narrow, and tortuous, as are most of the interior rivers of the La Plata system; both run in a general south-east direction, preserving a remarkable parallelism throughout their entire course, running distant from each other as nearly as possible 180 miles. Neither of these streams receives tributaries of any kind over the greater part of their course, and their waters are consequently subjected to a great and constant drain from evaporation, in a climate whose average temperature is 80° F., as well as from absorption by the deep alluvial covering overlying the compact argillaceous bed, which is a geological characteristic of the whole Chaco subsoil. The impermeability of this bed probably arrests the effect of absorption, and in a great measure accounts for the non-diminution of the wealth of waters delivered into the Paraguay; such a geological formation may also account for the saline properties of the waters found in the Chaco, wherever wells have been made. The density of the Bermejo water is greater than that of the Pilcomayo; the amount of sediment it brings down is enormous, and it is deposited with such extraordinary rapidity that it cannot but be considered a peculiarly strong feature of the mechanical work of this river, by which its geological formations are rapidly made, and, indeed, unmade as well; this swift precipitation of its detritus, which it replaces by an increasing abrasion of the banks, may be caused, to some extent, by the quantity of salt contained in its water. This constant precipitation goes on in the Bermejo, even when at its height, and when in the exercise of its greatest carrying power, with a speed quite equal to the square of its normal current; a fact which would seem to say that its currents are swifter on the surface than over its bed. Captain Page has seen this river eat away an entire point of land, and, by way of compensation, deposit, just a turning below, an amount of detritus sufficient to form a similar promontory, which, in one season of low water, became covered with a thick and luxuriant growth of red willow. The Pilcomayo—the Piscumayu, as it is called in the Quichua tongue, signifying Bird River—is to a great extent unknown. The section that is *quite* unknown, and that is surrounded by a certain mythical halo which it will be a geographical triumph to dispel, is that comprised between long. 61° and 62° W., and the parallels of 22° and 23° S.; the river at this point was said, by theorists who forgot to account for its reappearance immediately below, to disappear altogether. Captain Page then gave an historical sketch of the various expeditions which have explored the Gran Chaco, concluding with an account of an expedition in a steamer up the River Bermejo, which he himself led, amidst many dangers from banks, and snags, and wrecks, as well as from the widespread flood that suddenly overtook him.

WITH the first number for 1889 a useful modification has been introduced into *Petermann's Mitteilungen.* For the last few years, in addition to the classified list of geographical publications each month, there has been a separately paged supplement containing critical analyses of the more important works. These were often carried to such length that many of the notices were from six to twelve months behind date. Now, the two lists are to be amalgamated, the notices are to be greatly reduced in length, and thus it is hoped that new works in the various departments of geography will be promptly made known to the readers of the *Mitteilungen.* In the first number of this year is an important paper on valleys of erosion, by Dr. V. Hilber. The paper consists mainly of an analysis of the nine theories that have been advanced to account for the formation of such valleys. The author himself favours the regression theory, according to which valleys have mostly been formed by retrogression, through the erosion of a river from its mouth backwards.

CAPTAIN VAN GÈLE, the explorer of the Mobangi-Wellé, was to leave Antwerp on the 29th for the Congo, to undertake a special mission. He is accompanied by Lieut. Le Marinel, Lieut. De Rechter, and M. Ferd. Meunier, as naturalist.

M. MAUREL, who has explored French Guiana, recently described the results of his observations and investigations to the Geographical Society of Toulouse. From the orographical point of view, he stated, French Guiana comprises four zones, rising in stages to the Tumac Humac Mountains. The first consists of a broad band of alluvial country. The second zone is hilly, covered throughout by a series of hillocks and bluffs, not exceeding 650 feet in height, and frequently separated by shallow valleys. The third zone M. Maurel describes as mountainous, with an irregular surface, abrupt slopes, and deep valleys. The Tumac-Humac chain constitutes the fourth zone, and it rises by a series of gradations to a height of about 4000 feet. M. Maurel has collected a number of flint objects, which he believes belonged to a pre-historic race that must have inhabited the country before the alluvial period. He accounts for the present formation of Guiana by two long-separated volcanic outbursts, acted on subsequently by a large river, which he believes gave origin to the deposits of the first zone.

MR. J. Y. BUCHANAN, PROF. A. H. KEANE, AND MR. J. T. WILLS, are candidates for the Lectureship in Geography at Cambridge, vacant by the resignation of Dr. Guillemard.

THE Russian Geographical Society has just brought out, as an appendix to the nineteenth volume of its Memoirs, an atlas containing all the measurements made by A. Kaulbars in the delta of the Amu-daria. These measurements, which will be invaluable to those who may hereafter study the changes going on in the delta of the great Central Asian river, could not be embodied in M. Kaulbars's capital work "The Old Beds of the Amu," published by the Society in 1887. Now they are given partly in the atlas (on the scale of 1 : 1,500,000), and in full in the text which accompanies it.

THE PRESENT STATE OF SEISMOLOGY IN ITALY.[1]

THIS group of papers affords the reader a very fair means of forming a mental estimate of what Italy has been doing to study her earthquakes during the last year or eighteen months.

Signor E. Brassart, for some years the Mechanical Constructor of the Central Office of Meteorology and Geodynamics, has produced a seismoscope in which a small slug perched on a thin column was overturned by the earthquake, and fell into an umbrella-like balance-pan surrounding the peg. In this way the direction of the shock was supposed to be indicated by the

[1] "Sismoscopi o Avvisatori Sismici," Ermanno Brassart. "I Sismometri Presentemente in Uso nel Giappone," esaminati e descritti da Ermanno Brassart; con proposta di un Sismometro di Nuovo Modello. "Il Sismometrograph a Tre Componenti con Una Sola Massa Stazionaria," Nota di Ermanno Brassart. "Sulla Sistemazione delle Osservazioni Geodinamiche Regolari," del Prof. Giulio Grablovitz. "Relazione della Sottocommissione Incaricata di Studiare alcune Proposte per l'Ordinamento del Servizio Geodinamico nell' Italia Meridionale e nelle Isole," del Prof. T. Taramelli. "Relazione alla R. Sottocommissione Geodinamica sulla Distribuzione delle Aree Sismiche nell' Italia Superiore e Media," del Prof. T. Taramelli. "Il Terremoto nel Vallo Cosentino del 3 Dicembre, 1887," Studio del Dott. Giovanni Agamennone.—All these papers are published in the *Annali dell' Ufficio di Meteorologia e di Geodinamica*, vol. viii. Parte 4, Anno 1886. (Rome, 1888.)
"Alcuni Risultati di uno Studio sul Terremoto Ligure del 23 Febbraio, 1887," Nota di T. Taramelli e G. Mercalli, *Rendiconti d. R. Accad. dei Lincei*, vol. iv. fasc. 1. (Roma, 1888.)

fold in which it fell, and its weight disturbed the equilibrium of the balance, which by making contact gave notice of the shock. Many of these are at present in use in Italy. A recent improvement is to suppress the balance, the contact is made by the slug being caught between the supporting style and surrounding umbrella-shaped grooves. The author of the paper experimented on earthquake alarms for vertical shocks, and found that, with three wire spirals, two supporting different weights and one without, the last gave less repeated oscillatory movements after the shock, returning more quickly to rest; and he therefore suggests this as an improvement on the old spring and bob seismoscope.

The next point discussed is whether it is preferable to stop or start a clock at the moment of an earthquake, and is in favour of starting one, as the few minutes between the shock and the arrival of the observer could afford little time-error, whereas there is no means of correcting the time-error of a clock stopped by the shock.

Following an observation of Prof. J. Milne on the facility with which light objects, such as pens, pencils, &c., when propped up nearly vertically, fall by the slightest movement, the author has constructed a seismoscope. A long thin bar, standing in a concavity, is supported by a small prop that can be regulated so as to put the bar almost in a vertical position, which is surrounded by an isolated ring of brass. On the occurrence of a shock, the bar falls and completes a circuit with the brass ring so as to ring a bell, start a clock, &c. Experiments made by the inventor and Prof. Tacchini showed the extreme sensibility of this seismoscope compared with others placed on a marble wall-bracket upon which small weights were allowed to fall. Drawings and descriptions are given of a method of using this seismoscope to start a clock, the pendulum of which is retained ready to vibrate by a short catch. The ring surrounding the falling bar is supported on a jointed base and the falling bar displacing it, and acting on a system of jointed levers the catch falls out of the way of the clock pendulum.

Next, Signor Brassart alludes to the different ideas current about the best method of connecting seismic apparatus to the ground, and shows the necessity of a series of experiments to determine this by using stakes, walls, &c., and employing seismographical instruments of different types.

In the next memoir by the same author, a description and drawings of the principal apparatus used in seismological research in Japan is given, together with a criticism of each. He concludes the review of these instruments by pointing out that it is only those in which a pendulum is employed, together with those possessing a rolling base, either spherical or cylindrical, that do not require adjustment, beyond rendering the base horizontal. His own choice would be a vertical pendulum, because not only does it not require adjustment and is constantly ready to indicate the shock, but is always in the same condition; and then adds, "In other arrangements it is possible greater stasis or neutrality of the mass is obtained, but notwithstanding that, for the above-mentioned reasons I should show a propensity for pendulums." This is a preference I fear few other seismologists would share with the author, since he undoubtedly alludes to vertical forms. The floating seismograph of Gray is favourably mentioned, but troubles from evaporation of the liquid, &c., pointed out. This is similar to the opinion expressed by the writer of this article, some years since, in a paper on seismographs published in NATURE.

Signor Brassart then proceeds to suggest an instrument consisting of an annular pendulum suspended by a silken cord 1 metre long, attached at its upper end in the middle of another horizontal cord made taut by side screws in the frame. In the middle of the bob, near the centre of gravity, is a pin which slides in two slots placed at right angles to each other. These slots are cut in the ends of levers of the first order, one being bent at right angles so as to bring the opposite ends of the levers side by side, to which are attached writing needles. These are made to trace on a smoked glass plate held on a truck, which by a seismoscope is liberated and drawn along at a uniform and known rate by a clock. This proposition was soon carried out with some modifications, and at the end of the paper is an appendix describing it. The changes of design consist of the suspending silk cord being attached to a stage supported on three vertical cork cylinders, the object of which is not very evident, except, perhaps, to absorb a part of the vertical component. This latter is registered by a Gray's compensated bob attached to the same frame, and by means of levers made to trace side by side with those of the two horizontal component styles on the same plate, or, as the author suggests, in some suitable cases upon an endless band of paper. This seismograph has many defects which it will be more convenient to discuss later on. This paper is dated December 1887.

It has doubtless been the ambition of most inventors of seismic instruments to be able to register the three components of an earth-movement employing but one steady-point or mass. Anyone who makes such an attempt, however imperfect be his result, deserves much credit for trying such a herculean feat in mechanical construction.

Signor E. Brassart's later production is described in February of this year, and, in so far as the registration of the horizontal components goes, is quite similar to that described in December 1887. Inserted in the suspending cord is a spiral spring inclosed in a tube, which consequently also allows the bob movement up and down. The peg or rod then slides in the slots of the horizontal component levers, is prolonged downwards a suitable distance, and terminates in a ball, which is engaged in a short tube or cup at the end of a bar. A gimbal is a mechanical arrangement by which a bar supported in its centre can rotate around any theoretical axis in the horizon of the gimbal, whilst any motion along a line normal to the horizon or plane of the gimbal would be resisted; or, if the gimbal be suspended to a lever, this will be moved in proportion to the vertical movement applied to the gimbal arrangement. (The writer of this article believes the gimbal was first applied by himself in seismological instruments, but he is open to correction.)

Signor Brassart takes advantage of this, and makes the sphere at the end of the peg from the pendulum, form a ball-and-socket joint with the tube or cup bar of the gimbal arrangement which is supported horizontally at the end of a lever. Now all horizontal excursions of the peg, joint, and gimbal bar, are quite free, but vertical excursions will disturb the whole gimbal system with its lever, the motion of which by suitable mechanical arrangements is traced by the side of the horizonal component styles.

It will be convenient now to examine the favourable points and disadvantages of these instruments. In the first place, we have the well-known defects of the vertical pendulum for registering the horizontal components of an earthquake. Then, the author's method of the peg sliding in the two slots of levers introduces an amount of friction, even where the finest workmanship and materials are adopted, as to modify the results to an important extent. In regard to the gimbal arrangement, that answers perfectly during vertical without horizontal movement; but the moment that lateral displacement takes place, the bar of the gimbal, and the pendulum length, form two sides of a triangle instead of a straight line. On the principle that two sides of a triangle are always greater than the third, the more the horizontal components are, the less would the vertical component be registered. And even there is a point in which the motion of the pendulum bob would simply resolve itself in a greater tilt of the gimbal bar.

The next study by Prof. G. Grablovitz is a kind of programme of seismological observations to be made in Italy. The paper shows a clear conception of the known or probable relationship of different seismic phenomena, and apparently the best methods to adopt for their study. One most favourable point about the author's writing is a total absence of that long-winded style, charlatanism, and seismic magic, which has characterized the writings of many Italian seismologists up to quite recently. Very little, however, is new: similar suggestions may be found scattered through the writings of many other seismologists, and part, at least, were thrown out by the writer of this article in many of his papers relating to Ischia. It is, in fact, in organizing the new observatory on that island by Prof. G. Grablovitz that has led that gentleman to the considerations contained in his paper. From observations made with the mercury cup in the temporary Ischian Geodynamic Observatory, passing carriages and numbers of people could be detected at a distance of 100 metres; therefore the author considers this as the minimum distance of isolation such an observatory should possess.

The next is a report of a sub-committee, drawn up by Prof. T. Taramelli, dated December 1886. It consists of a review of the principal earthquakes that have affected different regions of Southern Italy, and the means then in existence for their study. This was preceded by another report referring to Northern and Central Italy. The author opens his report by discussing the views of different writers as to the causes of earthquakes, with-

out adding in any way to those views. Chapter ii. is a review of the earthquake parts of Italy referred to in the report, together with an attempt to separate the country into seismic provinces, a work already in part carried out by Mercalli, and where observatories are wanting to suggest the establishment of them. Appended to this memoir is a map of Italy coloured in nine tints, showing the seismic intensity in different regions. If the reader compare this with a geological map of the country, he will be struck with the relationship of earth-movements and the rocks. We notice intense foci near Sienna, north of Florence, and the alluvial flats of the mouths of the Po, another in Umbria, and a very intense one between Aquila and Solmona, where the Apennines reach their greatest elevation. There are the two volcanic centres of Naples and Melfi, with the intervening of Benevento and Ariano, a district in which comparatively recent rocks are much metamorphosed, whilst to the south-west of Potenza we observe a large but only moderately intense focal area. The toe of the Italian boot looks very dark with the two intense centres of Cosenza and Aspromonte, together with the Messina district in Sicily. In these we have granite, gneiss, and other crystalline rocks, covered chiefly by more or less friable Tertiary deposits, much eroded into many narrow deep valleys, and sharp crumbling intervening ridges. One cannot but be struck by the striking relationship between the distribution of seismic areas and the alluvial flats, the volcanic regions and more active points of mountain-building in the geologically-speaking young country of Italy. In fact, this map constitutes the most important part of the report.

The last two memoirs are descriptive in character, dealing with the two recent destructive earthquakes. Of these two, the first, by Dr. J. Agamennone, refers to the earthquake which occurred on December 3, 1887, in the Cosenza Valley. Besides the topographical, a geological map is given, with a description from which we learn that this strip of mountainous land lying between the Gulfs of Taranto and of Policastro is composed principally of granite and allied rocks with gneisses and schists. The depression constituting the valley is filled by clays, sand, marls, and other more or less incoherent rocks of Tertiary age, which have been eroded by innumerable side valleys into pinnacles, and tongues of land with more or less perpendicular sides very liable to slips. Bisignano, which suffered most severely, is situated on some of these pinnacles or almost isolated tongues of molasse. Although the town has been destroyed by nine or ten earthquakes, many of these had their foci at some considerable distance. This last earthquake occurred during a barometric maximum corresponding to a marked centre of high pressure that crossed Europe from December 1-3, and three days after full moon. The author could find no relationship between increase or decrease of microseismic movements in Italy and this earthquake. As there were probably three shocks, the first being comparatively feeble, the people had already escaped from their houses when the second arrived, so that, comparatively to the ruin, few lives were lost. These are the results of the statistics: houses down or threatened ruin, over 900; deaths, 22; wounded, 60; and damage done to the value of 1,000,000 lire.

It appears that here stone must be brought from some distance, and building materials are very dear, hence bad construction and ruin. This but too well recalls the appalling scenes that passed under the eyes of the writer of this article after the earthquakes of Ischia [1] which might have been far less destructive, had it not been for the horribly bad walls, &c.

From the exceedingly incorrect time kept and the few approximately accurate records, the calculations afford for the *velocity of propagation* from 650 to 3300 metres per second.

The author shows that the epicentre was near the station of Mount Grassano, and that the greatest damage was on the loose unsupported friable rocks resting inclined against the sloping faces of the older compact rocks—conditions of effect identical with what occurred in the great Calabrian earthquake of 1783, and noticed by Mallet in 1857. The intensity of this earthquake the author calculates, on somewhat flimsy grounds, at eleven times less than the Andalusian, and four times less than that studied by Mallet in Italy in 1857. The sound seems to have been communicated to much greater distances through the compacter and more elastic rocks than through the loose detritic deposits. What, however, is strange, is the manner in which the author talks of one shock being subsultory and another, in the same locality, as undulatory, as if they were two entirely different kinds of earthquakes.

In fine, we pass to the consideration of the Ligurian earthquake of February 23, 1887 (described by Prof. Taramelli and Mercalli), which drove so many people away from the towns of the Riviera. The first eight pages are devoted to the geology of the district, which may be said to be composed of a range of compact fairly elastic rocks close to the shore-line. The valleys that incise the cliff along the coast extend not only to the sea line, but some distance beneath the water, whilst near the coast they are partly filled by platforms of late Tertiary, and very incoherent deposits. It is on these small somewhat triangular planes that most of the towns are met with. The region is one of the most disturbed by earthquakes of Upper Italy, no less than twelve, more or less destructive, occurring since the thirteenth century.

The authors sent out 1100 circulars to different localities, and from the answers were able to obtain much information. It appears that, as on former occasions, slight shocks were felt over the whole district, and preceded the great one, no less than four occurring during the previous night to February 23, 1887. The area affected extended southwards to Rome, Mount Ferru (Sardinia), east to Pordenone, west to Perpignan, and north to Lyons and Basle. The mesoseismic area was crescentic in shape, 100 miles long between Mentone and Albissola. The form was due to the focus being beneath the sea, and to the region occupied by the elastic crystalline rocks. The effects of geological structure in affecting the limitation of isoseismal areas was beautifully illustrated in the Ischian earthquakes described by the reviewer. It is not customary to call the region above mentioned the mesoseismal area, which should be limited to that space around the epicentre and above the focal area: a better definition would be that of almost total destruction. The zone of severe injury extended to the Langhe of Piedmont and Astignano. The limits of severe shaking reach Turin and the lower Canovese, whilst the earthquake was strongly felt as far as Como, Arona, Parma, Leghorn, Marseilles, and nearly all of Corsica.

The main shock seems to have had two maxima, and to have lasted about 30 seconds. The authors calculated the *velocity of projection* at 9·4 m. at Oniglia, 3·53 m. at Faggia, and 4·7 m. per second at Nice. As in other cases of localities not epicentral, the rumble preceded the shock, the more, the farther the observer was from the focus.

Azimuths point to the epicentre being 15 miles from the beach, midway between Oniglia and San Remo, which is confirmed by the isoseismals being concentric to a point 20 kilometres south of Porto Maurizio. The shock appears to have started at about 6.19 a.m. The velocity of propagation was calculated at 1452 m. per second westward, and 584 m. per second towards Genoa: this the authors consider to be due to a main and secondary focus, whilst the depth obtained from emergence angles appears to be 18 kilometres. Little disturbance of the sea occurred, but it is said to have remained at a lower level for some days at Loano and Porto Maurizio. Dead fish of deep-sea character were found along the coast some days after. No meteorological phenomena of importance were noted, but strong telluric currents were set up at the moment of the earthquake. After nine minutes, another destructive shock occurred, another at 8.53 (M.T. Rome), and in the ruined area there occurred another twenty-two slight shocks, and others continued to occur with diminished force to March 11, so that at Savona fifty were perceptible in all.

The first three earthquakes killed 640 persons, and wounded as many, and the damage in the provinces of Porto Maurizio, Albenga, and Savona, was valued at 21,500,000 lire.

The greatest damage occurred on thin layers of incoherent rocks superposed on the more elastic and crystalline ones, at sharp boundary lines of different rocks, and unfavourable topographical positions. The effects were augmented by bad and unscientific construction of the houses, or badly repaired buildings, that have suffered in former shocks.

In this article, already of inordinate length, fair justice has hardly been done to the works reviewed, but the reader will see that Italy is awaking to her duty towards humanity and science in organizing the study of her seismic phenomena, and shaking off that conservatism and isolation which ruled her in this department up to the year after the great Ischian earthquake.

H. J. JOHNSTON-LAVIS.

[1] H. J. J.-L., "Monograph of the Earthquakes of Ischia: a Memoir dealing with the Seismic Disturbances in that Island from Remotest Times, with Special Observations on those of 1881 and 1883." (Naples: Furcheim, 1885.)

FRIENDLY SOCIETIES AND THEIR FUNDS.[1]

M. DE LAFITTE is the Vice-President of a Friendly Society numbering some 150 members and of a somewhat advanced type. He has evidently discharged his duty as Vice-President with exemplary devotion to the work, and has extended his studies and observations over the wider area covered by the French Friendly Societies generally. From a Report of the Minister of the Interior, it appears that the number of these Societies on December 31, 1885, was just upon 8000, that the average number of members in each was 136, and that, consequently, the total number of members whose names were inscribed in their rolls at that date was more than a million.

The volume before us is professedly an elementary treatise on Friendly Society finance. The author, in the opening chapter, selects, as the typical Society, one which possesses no other resources than the contributions of its members, and which, in return, undertakes to provide three distinct benefits: viz. (1) an allowance during sickness, (2) an old age pension to commence at a specified age, and (3) a sum of money to be paid to the relatives, on the decease of a member, to cover funeral expenses. He deals, throughout the book, with the various conditions under which contributions are levied and benefits conferred; but as a rule he assumes that new members are admitted at the age of 16, that the contributions cease at age 70 (or thereabout), when members become pensioners, and that sickness allowances, so far as money allowances go, are, at the same time, also discontinued.

The author, at the outset, urges the desirability of apportioning the total contribution of each member into three parts, corresponding to the three kinds of benefits assured to the member; and of keeping an exact account of the fund accumulated, from year to year, out of each part, *i.e.* of keeping an exact account of the resources available, at any moment, for each kind of benefit.

He points out, what is indeed obvious enough, that the share of contribution allotted to pensions must accumulate for many years; in fact, till the age for commencing pensions is attained by the members of the Society. He points out that, of the share of contribution allotted to meet sickness claims, a fraction of it only is required in the earlier years of life, when periods of sickness are less frequent and less prolonged, the remainder of it forming a provision against the heavier disbursements of later years, when the periods of sickness have become more frequent, more protracted, and more costly. Also, he points out that with the share of contribution intended to secure funeral moneys, there is likewise a gradual accumulation going on from the earlier years of life, when deaths are fewer in number, to the later years of life, when they are doubly and trebly as numerous. In respect of all three kinds of benefits, then, each member of the Society, during his years of active membership, has an increasing interest in the adequate accumulation of the funds of the brotherhood. These funds are trust funds, a collection of deposits confided to the Society by its members, and for which it remains accountable till the time arrives for restoring them, in the shape of aid to the members in sickness, or old age, or to the relatives upon the occurrence of decease. The preceding considerations show that every Society has not only accumulating funds fed by unused portions of the contributions received year by year, but also, and on the other hand, growing liabilities, and it has therefore to take care that the liabilities do not grow faster than the funds accumulate.

Every Society renders accounts year by year, but they are of the usual form, and that is not enough: at the same time that it exhibits its resources, it should know how to calculate with exactness its actuarial liability under current contracts with its members. By actuarial liability we mean the obligation of the Society to gradually accumulate a fund out of which future benefits may be paid as they mature. By inserting the amount of such actuarial liability in its balance-sheet, the Society completes its statement of liabilities and assets, and shows conclusively whether its accumulated funds are more or less than they should be. Such a balance-sheet, if the liabilities and assets are shown separately for each of the three kinds of transactions—sickness allowances, pensions, and death payments—is a perfect balance-sheet. It should be made out every year, and carefully studied by those responsible for the management of the Society.

The author's chief object in writing the present treatise is to give tables of actuarial liability, and to enunciate, prove, and illustrate rules by which such liability may be calculated. He proposes to explain to Friendly Societies how two or three of their constituted officials, in the course of an afternoon, may determine the liability of the Society with precision, and without possessing any preliminary knowledge other than that of the elementary rules of arithmetic, such as are taught at the primary schools to children of between ten and thirteen years of age.

Considering the labour usually expended on actuarial valuations, and the special knowledge and skill usually regarded as indispensable in such investigations, these are brave words. Impressed, as we are, with the immense amount of good that would accrue from the accomplishment of the author's design, we wish him whatever success is possible. There is nothing so much needed for the stability of Assurance Companies as well as Benefit Societies as a fuller acquaintance on the part of the public with the nature of actuarial liability. If such knowledge were general, we should hear little more of the insolvency of such associations, and see little more of the sad spectacle of a group of dependent and helpless persons finding in sickness or old age that the savings of long years of self-denial have been sacrificed.

In the calculations of Friendly Societies the average mortality and sickness found to have been experienced under more or less similar conditions must form the basis of all reasoning, and the effect of compound interest, combined with mortality and sickness, must, in some form or another, be introduced as further necessary preliminary information. The author lays before us, as giving such necessary preliminary information, several tables. In addition to two tables of mortality, viz. the table of the Caisse des Dépôts et Consignations, and the table of the English Institute of Actuaries, known as the H^M (healthy males) table, and a sickness table, showing the average number of days of sickness per head at each year of age, we have two tables of a somewhat unusual kind for the present purpose. These tables embody new rates of the Caisse Nationale des Retraites pour la Vieillesse, of whom pensions are bought, and show, for any present age, the amount of deferred annuity, to commence at age 50, 55, 60, or 65, corresponding, in the first table, to a payment, cash down, of 100 francs, and, in the second table, to a payment of 10 francs a year until the pension commences. A table of the present values of temporary annuities, terminable at any one of the four ages first mentioned, completes the group of fundamental tables.

The fact that both the above-named tables of mortality are frequently used in one and the same solution is, as the author appears to be conscious, open to criticism; and so is the fact that contributions are sometimes assumed to be payable at the beginning of the year and sometimes at the end of it. In a particular case, instanced by the author himself as showing that the consequent error is insignificant, the error amounts to 4 per cent. of the true figure, and this is hardly to be regarded as an insignificant difference. With respect to the tables themselves, they display appreciably different rates of mortality, and if one is found to be suitable for a particular Society, the other can hardly be suitable also. The H^M is a table of which English actuaries are justly proud; but a record of the mortality experienced by the middle and upper classes of this country is hardly to be accepted as a measure of the mortality to be expected amongst the artisans and mechanics and the mechanical and agricultural labourers of France. It might not, possibly, be inapplicable to some of the rural districts of that country, but if so, it would probably not be applicable to the districts of the large towns and the cities. Strictly speaking, no one table of mortality is suitable in all the varying conditions of locality and occupation to be found within the boundaries of French territory; and herein lies an obvious objection to the application of stereotyped results, such as those prepared for general use by the author, to the circumstances of all the Friendly Societies of an entire country, indiscriminately.

In selecting a sickness table on which to base the calculations of sickness liability, the author very properly observes that sickness tables, when compared one with another, often present discordant results. This is explained to be due to the limited number of observations or individual experiences on which the tables are based, and partly to the difficulty of saying exactly when a period of sickness begins or ends—of drawing, with precision, the line of demarcation between positive sickness and mere indisposition; besides, chronic and incurable maladies have been

[1] "Essai d'une Théorie Rationnelle des Sociétés de Secours Mutuels," par Prosper de Lafitte. (Paris: Gauthier-Villars et Fils, 1888.)

dealt with differently in the tables compiled for publication. Of the tables at the author's command, he has chosen that given by M. Hubbard as being, in his judgment, the best representative of the circumstances with which he is immediately concerned.

We cannot, at this point, do better than compare M. Hubbard's figures with those we are familiar with in England, viz. those of the Manchester Unity of Oddfellows and the Foresters. We give, then, the average number of days of sickness per annum at each of the undermentioned ages, as shown by the several tables specified. In the cases of the Oddfellows and the Foresters, the total average sickness in the year is shown in two parts—sickness within six months of its commencement, and sickness counting only beyond this limit. The rules of French Societies generally do not continue aid in money beyond the term of six consecutive months, a large proportion of them not beyond three.

Age.	M. Hubbard	Manchester Unity of Oddfellows: first six months and subsequently.		Foresters: first six months and subsequently.	
20	4·4	4·6	0·3	5·6	0·3
30	5·6	5·2	0·9	5·4	0·9
40	6·0	6·4	1·8	6·9	2·0
50	7·1	9·1	4·8	9·1	4·5
60	11·7	15·0	13·2	13·8	13·1
70	17·1	25·8	44·8	24·0	48·6

A glance at the above table affords sufficient evidence that the author is right in his assertion that sickness tables present very discordant results; and we should be somewhat failing in our duty if we did not utter a word of caution as to the use of M. Hubbard's tables by unskilled persons, more especially so as the author is inclined to advocate the continued payment of the sickness allowance in prolonged sickness rather than its curtailment. The experience of the Oddfellows and the Foresters may not supply a suitable basis for the calculations of *les Sociétés de Secours mutuels*; but this experience is extensive, has been carefully tabulated by competent actuaries, and is, at least, worthy the attention of anyone proclaiming, as our author does, the narrowness of existing and available data.

We have not the space to follow the author in detail through the numerous rules and explanations given by him in connection with the calculation of actuarial liabilities and kindred matters. We must content ourselves with an examination of one only, and that a simple one. We propose to consider the method recommended for the calculation of the actuarial liability attaching to the receipt of contributions on account of pensions. As an example, the author takes the case of a member of 47 years of age, who has contributed 10 francs a year since the age of 16, and who is looking forward to a pension of 294 francs 11 centimes to commence at the age of 65. He then proceeds on the basis of the Caisse des Retraites tables, substantially, as follows:—

According to the second of the two tables, an annual contribution of 10 francs, the first payment being made at age 47, secures a pension, to commence at age 65, of 41 francs 35 centimes a year. Whilst, for the same annual contribution, the first payment being made at age 16, the pension is one of 294 francs 11 centimes a year. The equivalent at age 47 of the earlier contributions (from age 16 to age 47) is, therefore, a pension amounting to the difference of the two preceding ones, 252 francs 76 centimes a year. But, according to the first of the two tables, the cost, at age 47, of a pension of 34 francs 53 centimes a year is 100 francs. It follows that the cost of a pension of 1 franc is $\frac{100}{34·53}$ francs, and of 252 francs 76 centimes is $\frac{100 \times 252·76}{34·53}$, *i.e.* 732 francs.

We do not think the tables on which this calculation is based are more convenient in form than those in general use, or that they lend themselves more readily to a simple presentation of the theory of the subject; and we do not think the method of arranging the calculation is any improvement upon that commonly adopted. Annuity tables intended as aids to arithmetical computation are usually constructed to the unit of annuity, and not to the unit, or ten, or hundred, of contribution or purchase money. A common form of annuity table gives, for each year of life, the present value of a temporary annuity, payable to one of certain ages, say to age 65, accompanied by the present value of a deferred annuity to commence immediately thereupon, the two present values added together giving the present value of a life annuity to run during the whole of the annuitant's life. The ordinary calculation, which may be called the method of present values, then proceeds to estimate the liability of the Society to provide the future benefit, on the one hand, and the claim against the member to provide future contributions on the other:—

Present value, at age 47, of a pension of 294 francs 11 centimes a year, after age 65, is the tabular number (2·90) × 294·11	853 francs
Present value of contribution of 10 francs, payable from age 47 to age 65, is the tabular number (12·10) × 10	121 francs
Leaving the actuarial net liability as before ...	732 francs

The principle of present values which distinguishes the usual method of calculating actuarial liabilities, is quite as easy for the general public to understand as the inverted method of using tables, proposed by M. de Lafitte; indeed, easier, because of the uniformity with which it is applicable to all cases of annuity, assurance, or sickness contracts. M. de Lafitte himself makes reference to it in explaining the calculation of sickness liabilities. We are of opinion, however, that there is a still simpler way of explaining to the general public the calculation of actuarial liabilities than that depending on the principle of present values. The latter looks to the future, the former to the past. The latter is a prospective method of procedure, the former is sometimes described as a retrospective method. It consists in imagining a number of persons, equal to the number in the table of mortality or sickness, to have actual existence, and to live and die as indicated in the table of mortality, and to fall ill as indicated in the table of sickness, and for the periods mentioned therein. By supposing contributions to be received year by year from all who live to pay them, and the various benefits to be paid out as they accrue due, the amount of money remaining in hand, as thus shown, is the sum that a Society of the tabular number of members should have in hand, and therefore the amount also of the Society's actuarial liability as required. For any other number of members than that suggested by the table, the amount of the actuarial liability would be proportional.

We have confined our observations, in the main, to the subject of actuarial liability, the use of tables of such liabilities, the mortality and sickness experiences on which they are based, and the principles on which they are explained, because the chief object of the book is to deal with this subject and to popularize it. There is, however, a chapter on the due, or fine, or fee, to be charged new members who join at other than the age for which the rates of contribution are arranged; another chapter on raising the contributions of existing members to provide increased benefits or to meet a deficiency; another on the mission and the proper sphere of Friendly Societies; and others beside, all affording excellent reading, and on which, had space permitted, we should like to have said a few words; but we must conclude this notice. The author manifests an enthusiasm in the work he has undertaken, has expended a great deal of time and thought upon it, and evidently has the welfare of Friendly Societies at heart. If one may judge of literary composition in a language which is not one's own tongue, we should like to express the opinion that the author's writing is clear and attractive. In his efforts to promote and encourage a wider knowledge of the somewhat abstruse subject of actuarial liabilities or valuation reserves, he has our very best wishes.

WATERSPOUTS IN THE HUGHLI.[1]

ON Tuesday, the 4th instant, a fine waterspout was projected from the level vapour-plane of a silvery-edged towering cumulus cloud—or, as our American cousins would term it, a "thunder-head"—over the western side of Kulpee anchorage, and near the village of Jiggerkolly, which, by the aid of a good telescope, showed well the downrush on the inside of the tube, and its counterpart the whirling uprush on its outside, twisting and coiling round and round against the watch-hands (face upwards).

[1] Reprinted from the *Englishman* of September 13, 1888.

The day had been close, hot, and breezeless, so far as the surface was concerned, and the cloud in question formed a grateful screen to the afternoon's sun, which was shining brightly north and south of our position; and continued to do so until old Sol hid his face for the night in lower clouds later on. At about 5 p.m., a well-formed waterspout was observed trailing away north-eastward from the cloud's northern verge; then about 40° or 30° high. Near the dense cloud from which it grew its mass was dark and opaque; but from half way down its length to below where it terminated in an unfinished turmoil of jags and rags, it was semi-transparent, the clouds beyond in the background being distinctly seen through it; and there was a light-coloured tube within its gauzy mass which at times was very pronounced and conspicuous.

There seemed to be no commotion on the river or the shore, over which its lower part hung at a height of five or six hundred feet, from the time when it was first observed until it began to wane and draw itself upwards. It was then, when it had shrunk and shortened, that the inside downrush was seen to advantage, and the simultaneous upward whirl around the dense remains of the tube, which, so far as I was able to make out from the motions of the cloudlets, I cannot do better than liken to the turning inside out of a coat sleeve or of a stocking, only the end of the tube was always ragged; and here, where this reversing process was taking place, there was great commotion in the air currents, more especially after the tube had withdrawn itself up to its opaque head.

I had a good telescope, observed these phenomena very carefully, and was on the alert for optical illusions; and, as the upper part of the spout remained intact for a long time after the gossamer-like lower continuation had melted into invisible vapour, I had an opportunity of studying it well.

It was very remarkable to see two or three common kites hovering high up in the vapour plane round and round the dense tube, every now and then becoming hidden behind it as they performed their gyrations, and in the same direction as the ragged fragments of rack or scud which were rushing round and upwards towards the dense head of the spout, until they were actually lost to sight in the very cloud itself.

The "thunder-headed" cloud, from the lower part of which the spout issued, gradually melted away, and grew less gaudy as it dwindled, without, as is usual under similar circumstances, pouring forth a torrent of rain. But there was a double rainbow projected on the clouds banking up over to the eastward, and evidently rain was falling in the space between them and the sun for some time after the last remains of the spout had disappeared, and all was again tranquil aloft.

The following evening, during a heavy thunderstorm and severe squall from the north-east, some twenty miles further down the river, a flash of lightning revealed the existence of a grand waterspout close by. This thunderstorm was remarkable, as it stretched south and north from the pilot station to far north of our position off Kedgeree, and struck both places at nearly the same hour, 7 p.m. Possibly all these phenomena had some connection with the small whirl reported in Mr. Pedler's useful weather bulletin issued daily from the Meteorological Office. The squall itself was seen from an early hour in the evening as a tract of dark streaks hovering ominously high up behind the accumulating white clouds away in the north-east and east, and threatening to quench the light north-west wind blowing in the estuary.

Subsequent information concerning this sudden thunderstorm shows that it was one of the class, referred to lately in NATURE by the Hon. Ralph Abercromby, which extend in an almost straight line for maybe hundreds of miles. It was felt at nearly the same hour all the way from Titighur on the north, to a point forty or fifty miles south of the Hughli Pilot Station—or, say, nearly 200 miles. S. R. ELSON.

SCIENTIFIC SERIALS.

Journal of the Russian Chemical and Physical Society, vol. xx. fasc. 8.—On the properties of allene, by G. Gustavson and N. Demianoff. The existence of this isomer of allylene ($CH_2 : C : CH_2$) was foreseen since 1872; now, it has been obtained by the action of zinc dust and alcohol on $C_3H_4Br_2$. It is an uncoloured gas, the smell of which resembles that of allylene. It smokes freely when burning.—A note on the atomicity of bore, by G. Gustavson.—On unorganized ferments, by N. Kravkoff, being an inquiry into the properties of pure vegetable diastase.—On a general law of contraction during the formation of solutions of salts, by A. Geritsch. The author comes to the conclusion that the contraction (δ) is proportional to the produce of the respective percentages of water and salt in the solution $\delta = C(100 - p)p$; for Na_2CO_3 the constant C is $= 0{\cdot}0086$, and the contraction calculated by means of the formula for solutions at from 3 to 13 per cent. are very near the contractions directly measured by Gerlach and Mendelejeff.—On the solutions of sulphuric acid from the molecular point of view, by M. Teploff.—On the heat of combustion of stilbene, the mononaphthenes, and some organic acids, by J. Ossipoff.—An apparatus for the demonstration of thermic conductibility, by O. Chwolson.—On a new method of measuring the index of refraction, used by E. Forsch for measuring the same in the lenses of the Pulkowa refractor.—A note on the mutual influence of electrized bodies, by A. Stepanoff.

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, January 17.—Mr. W. Carruthers, F.R.S., President, in the chair.—On behalf of M. Buysman, of Middleburg, Mr. B. D. Jackson exhibited a series of careful dissections of *Nymphæa cærulea* collected by Dr. Schweinfurth in Egypt.—Mr. D. Morris exhibited specimens of drift fruit from Jamaica, where he had collected no fewer than thirty-five different kinds brought by the Gulf Stream from the mouths of the Orinoco and Amazon. Although the species exhibited had not been determined with certainty, it was believed to be probably *Humiria balsamifera*, Aud., the flower of which is figured by Eichler ("Flora Brasiliensis," vol. xii. part 2, p. 440, pl. xcii. fig. 1), but the fruit undescribed. It was commonly known in French Guiana as *bois rouge*, and from it was obtained a gum used medicinally and burnt as incense. An interesting discussion followed, in which Mr. J. G. Baker, Mr. Rolfe, and Mr. Breese took part.—Mr. T. Christy exhibited a material felted from Manilla hemp, and waterproofed, very strong and light, and particularly useful for surgical bandages, for which purpose it was highly recommended by army surgeons.—Mr. F. Crisp exhibited some specimens of agate so curiously marked as to lead to the erroneous supposition that they inclosed fossil insects and Crustacea.—A paper was then read, by Mr. J. G. Tepper, on the natural history of the Kangaroo Island grass-tree, *Xantharrhœa Tateana.* This tree grows abundantly in Kangaroo Island, South Australia, in poor gravelly and sandy soil, intermixed with ferruginous concretions, and attains a height of from 6 to 14 feet, with a diameter of 6 to 18 inches, and a floral spike of from 10 to 19 feet. It is thus a most conspicuous plant, and lends a peculiarly weird aspect to the country it occupies. Its rate of growth is described as very slow, old settlers having remarked but little change in individual trees after thirty years' observation. The most remarkable feature in the structure of the stem is the formation of a dense ligneous central core immediately above and connected with the roots, exhibiting numerous annular zones traversed by transverse (medullary) fibres. The flowers are borne in a dense spike upon a smooth peduncle. Individually they are inconspicuous, of a whitish colour, and develop a strong odour and abundant nectar during the warmer part of the day, when they are visited and fertilized by Hymenopterous insects, the most remarkable being a large metallic-green carpenter-bee (*Xylocapa*), which tunnels out cells in the dead flower-stalks. An interesting discussion followed upon the botanical position of the grass-trees, and the antiquity of the type, in which the President, Mr. A. W. Bennett, Mr. J. G. Baker, Mr. Morris, and Mr. Rolfe took part.

EDINBURGH.

Royal Society, December 17, 1888.—Sir W. Thomson, President, in the chair.—Mr. R. Kidston read a paper on some fossil plants from Teilia quarry, Gwaunyscor, near Prestatyn, Flintshire.—Dr. G. Sims Woodhead communicated an abstract of the results of an inquiry into the causation of Asiatic cholera. The first part (general) was by Dr. Neil McLeod and Mr. W. T. Milles. The second part (with special regard to the reproduction of the disease) was by Dr. McLeod. The disease was shown to be due to the comma bacillus. In forty out of forty-four cases this bacillus was present; while, in a large number of spurious cases, it was only once or twice detected. It was found that symptoms similar to those accompanying cholera in

man followed injection of the comma bacillus into the stomach of the guinea-pig, care being taken to neutralize the acid products which are fatal to the organism.—Prof. Tait described some preliminary observations with a large rotatory-polarization spectroscope. In this apparatus the light passes through a slit and collimator, a Nicol's prism, a train of cylinders of quartz, a double-image prism, and an ordinary direct-vision spectroscope. The instrument is so arranged that the two spectra produced are in exact juxtaposition side by side, each spectrum being alternately crossed by dark interference bands corresponding to successive rotations of the plane of polarization through 180°. A scale is thus applied to the spectrum, and the interval between successive bands can be subdivided to any required degree of accuracy. Wave-length is thus measured with extreme accuracy by the amount of rotation of the plane of polarization. When the apparatus is used, not with bright-line spectra but, with continuous spectra, the excessive loss of intensity by dispersion which occurs in ordinary spectrometers may be avoided.—Dr. Woodhead communicated a paper by Mr. G. Brook, entitled "Preliminary Remarks on the Homologies of the Mesenteries in Antipatharia and other Anthozoa." After describing the arrangement and musculature in the common shore anemone, *Edwardsia*, Cerianthidæ, in Alcyonaria, Madreporaria, Mr. Brook pointed out that the Antipatharia are generally supposed to be degenerate forms, and to have lost a considerable number of the mesenteries that were present in their ancestors. Mr. Brook, who is examining the *Challenger* collection of Antipatharia, has been able to make sections of twenty-three species, from which he finds that the arrangement, number, and relative development of the mesenteries cannot be explained in accordance with the views current on the subject. With the exception of two pairs of directives the mesenteries do not appear to show the paired arrangement usually looked for. He gives the arrangement of the mesenteries in *Cladopathes* (six mesenteries), *Antipathes* (ten mesenteries), *Leiopathes* (twelve mesenteries), and points out that the arrangement in these forms receives its explanation by a comparison with the order in which the first twelve mesenteries are, according to Lacaze-Duthiers, developed in Hexactiniæ. In *Actinia* and *Sagartia* the first twelve mesenteries are developed in pairs which are not adjoining mesenteries, but are situated one on each side of the stomodæum. The order in which they are developed in *Sagartia bellis* (and in *Actinia equina*?) precisely corresponds *with their relative length* in *Leiopathes*. The first pair to be developed are those corresponding to the transverse mesenteries in Antipathidæ; next follow the two pairs of directives, and afterwards the three pairs which he has termed "secondary" in Antipathidæ. The shortest mesenteries in *Leiopathes* are the last of the six pairs to be developed in *Sagartia*. Evidently, then, the mesenteries forming a pair are originally opposite mesenteries and not adjacent ones. We thus have, in forms with an elongated stomodæum, a true bilateral symmetry. The two pairs of directives limit an anterior and a posterior unpaired chamber. Between these two the cœlenteron may be imperfectly divided into any number of paired lateral chambers. On this interpretation the arrangement in Alcyonaria, *Edwardsia*, Cerianthidæ, *Madracis*, &c., is also easily understood: all are modifications of one plan. In the Hexactiniæ the simple bilateralism is masked, but a careful study of the order in which the mesenteries are developed shows clearly how this is brought about. In all types the mesenteries of a pair are originally on opposite sides of the stomodæum. The two pairs of "directives" come to be *adjacent* mesenteries, for the reason that no new mesenteries are ever formed between them, and with a further development of mesenteries they come to be pushed closer together. As is clearly seen from Hertwig's figures of the embryonic condition in *Peachia*, the other so-called "pairs" of primary mesenteries are not pairs developmentally, as they consist of mesenteries of different ages. They are called pairs because they are arranged in couples, having the retractor muscles on their inner surfaces. In Hexactiniæ the further increase in the number of mesenteries takes place in a modified way. Buds appear which are on opposite sides of the stomodæum between existing "pairs," but, instead of giving rise to a single mesentery as before, each gives rise to two, with the retractor muscles on their inner surfaces. The general plan of development Mr. Brook considers to be as follows. The mesenteries have a radiate arrangement in forms with a round stomodæum; this arrangement becomes bilateral by an elongation of the stomodæum in one axis—the sagittal. In this case the anterior and posterior pairs (directives) come to consist of adjoining mesenteries, whilst the intermediate pairs consist of opposite mesenteries. So long as the folds of the body-wall give rise to only one mesentery each, the simple bilateral arrangement is retained, as in Cerianthidæ (this refers to bilateral arrangement of parts, irrespective of the outline of the polyp). In case the mesenterial rudiments give rise (after the formation of the first twelve mesenteries) to two mesenteries instead of one, the Hexactinian type is reached. In certain Madreporaria (*e.g. Lophohelia*, *Mussa*, and *Euphyllia*) the radiate arrangement appears never to be lost. At any rate, according to Fowler and Bourne, there are no mesenteries distinguishable from the others as "directives," and there is a perfectly radiate symmetry. Such a general plan of development is also found in *Peripatus* and in Vertebrata. In *Peripatus* the blastopore becomes elongated and closes in the centre, but its two extremities remain open as the mouth and anus. The mesoblastic somites are formed in the region in which the blastopore has closed, and these become more numerous as the two extremities become more and more separated. At present, Mr. Brook is only able to indicate the bearing of these views in outline. He hopes, however, shortly to make a more detailed communication on the subject.

January 7.—Prof. Chrystal, Vice-President, in the chair.—At the request of the Council of the Society, Prof. Tait gave an address on the compressibility of water, salt-solutions, glass, and mercury. His address was illustrated by experiments.

PARIS.

Academy of Sciences, January 21.—M. Des Cloizeaux, President, in the chair.—On a point in the question of homogeneous elastic plaques, by M. H. Resal. In this note the author proposes to base the hypothesis relative to the expressions of the tangential dilatations on a supposition of a more general character than that hitherto assumed by geometricians.—On the Hæmatozoa detected by M. Laveran in the blood of the inhabitants of marshy districts, by M. Bouchard. Attention is called to the great importance of the discovery made by M. Laveran ten years ago, and now placed beyond all doubt, that marsh fevers are of parasitic character. They offer the first known example in man of an animal parasitism in which the pathogenic agent appears to be placed at the lowest scale of animal life. While most infectious maladies in man and animals are due to vegetable microbism, the most important and widespread infectious disease in man is now shown to depend on animal microbism. The parasite observed by M. Laveran in Algeria has since been found in France, Corsica, Italy, Russia, Madagascar, Tonquin, and America, and is identical with the organism more recently detected by Marchiafava and Celli in the blood of people inhabiting marshy districts.—On the elementary terms in the co-ordinates of a planet, by M. Hugo Gyldén. Supplementing his recent communication on this subject, the author here points out that, the convergence of the terms in question being established, their numerical values may be determined by the methods proposed in the paper on the determination of the radius vector in the absolute orbits of the planets inserted in the *Monthly Notices* of the Royal Astronomical Society, London.—On the distribution of the aqueous vapour in the atmosphere, by M. A. Crova. In a previous note (*Comptes rendus*, cviii. p. 35) MM. Crova and Houdaille communicated the results of the observations made last August at Bedoin and on the summit of Mont Ventoux. From those results M. Crova here deduces the mode of distribution of the aqueous vapours at various altitudes. Although only approximate, the calculations show how rapidly the quantity of vapour must decrease with the increase of altitude. The quantity itself also varies greatly from day to day, which is again explained by the fact that the vapours are mainly confined to the lower atmospheric regions, which are most directly influenced by meteorological phenomena.—Note on the new meridian of France, communicated by the Minister of War. The Geodetic Section of the Service Géographique de l'Armée concluded in 1888 the measurement of the angles for the new meridian begun eighteen years ago. The present note embodies a summary report of the main results, from which it appears that the meridian of Delambre and Méchain, useful in their day, can no longer serve as a base for the triangulation of France, or for further researches on the form of the globe. For these purposes the new meridian offers all the necessary elements except for the south-west region, where fresh measurements are required to secure complete accuracy.—Observation

relative to M. Vaschy's recent note on the propagation of the current in a telegraph line, by M. L. Weiller. The author questions on theoretical grounds some of M. Vaschy's conclusions, on which has been based an application for a patent. M. Weiller submits a specimen of a telephonic cable with two conductors constructed for the purpose of obtaining by self-induction the compensation of the electro-static capacity. In this apparatus the increase of the coefficient of self-induction is obtained by inclosing the chief copper conductor in a covering of soft iron wire.—Observations of the partial lunar eclipse of January 16, 1889, made at the Observatory of Lyons, by M. G. Le Cadet; and at the Paris Observatory, by MM. D. Eginitis and Maturana. The observations at Lyons were made with the Gautier equatorial *coudé*, and at Paris with the west equatorial in the garden. In the latter place the atmospheric conditions were favourable at the commencement and towards the close of the eclipse; but during the middle period the sky became overcast, preventing the complete observation of all the phases.—Experimental verification of M. Charles Soret's method for measuring the indices of refraction in crystals with two axes, by M. Louis Perrot. In a previous note (*Comptes rendus*, cvii. p. 176), M. Soret showed that the three chief indices of refraction in a crystal with double axis may be deduced from the limiting angles of total reflection on any given faces. M. Perrot has now verified this method by experiments with ordinary tartaric acid, employing a Liebisch refractometer for the purpose.—On the electric conductibility of salts in solution, by M. Lucien Poincaré. The author finds that the high polarization of a silver electrode plunged into a saline solution, such as the nitrate of potassa, falls immediately to zero by adding a trace of the nitrate of silver. From this phenomenon follows a means of greatly simplifying in certain cases the method hitherto employed by MM. Bouty and Poincaré for measuring the electric resistances of saline solutions.—Papers were contributed by M. Ch. Antoine, on the expansion and compression of atmospheric air; by MM. C. Vincent and Delachanal, on the extraction of sorbite; by M. J. Meunier, on the dibenzoic acetal of sorbite; by M. A. de Lapparent, on the relation of the acid eruptive rocks to the phenomenon of the solfataras; and by M. H. Morize, on the Widmanstätten figures, illustrated by two photographs obtained in direct and diffused light.

Berlin.

Physical Society, December 28.—Prof. Kundt, President, in the chair.—Dr. Ritter demonstrated, with the help of an electric arc-lamp, the action of the ultra-violet rays on the electrical discharge at the negative pole.—Dr. Lummer spoke on photometers, and deduced, from the experiences gained with the existing instruments, the following as requirements in their construction: in the first place, the surfaces whose brightness of illumination is to be equalized must not be separated by even the narrowest intervening space; and in the second, the outline of the surfaces must be sharply defined. The first of the above requirements is satisfied in Bunsen's grease-spot photometer, but not the second. A further drawback arises from the fact that the grease-spot reflects light, and the paper allows some to pass through, so that both the spot and the surrounding surface are always illuminated by both sources of light. An ideally perfect photometer ought to reflect no light from its grease-spot, and be impermeable to light over the rest of its surface. The speaker, working in conjunction with Dr. Brodhun, had obtained the above desiderata by purely optical means. When two total-reflection prisms are placed with their hypotenuse surfaces in juxtaposition, and two of the surfaces of the glass-cube thus formed are illuminated by light from bright surfaces, then on looking through the combination of prisms the only light which reaches the eye will be that which enters laterally, whereas that which enters from the opposite side cannot reach the eye. When a drop of Canada-balsam, whose refractive index is very nearly the same as that of the glass, is placed between the opposed hypotenuse surfaces, total reflection is done away with at the place where the drop lies, and thus by illumination from *one* side only either a bright spot is seen on a dark ground or a dark spot on a bright ground. When the illumination is made from *both* sides, equality of illumination can be easily established by adjusting the relative distances of the sources of light until the spot disappears entirely. The drop of Canada-balsam very soon loses its sharply-defined edges, hence some other mode of procedure became necessary. The central portion of the hypotenuse surface of one of the total-reflection prisms was left untouched, while the rest of the surface was ground to a slightly spherical shape. When the surfaces of the prisms were now firmly pressed together, light passed without hindrance across the point of close contact of the surfaces, whereas it was totally reflected at all other points. By this means an ideally perfect "grease-spot" was obtained, which was permeable to light, but reflected none, while the surrounding area reflected light completely and allowed none to pass through. A third method for obtaining an ideally perfect grease-spot consisted in etching figures on one of the reflecting surfaces of the prisms; the etched portions were perfectly transparent, the rest of the surface reflected light completely. The speaker exhibited photometers constructed according to the above methods, and proved theoretically that the ideally perfect grease-spot bears to the real one the ratio of 1 to 2, according to the constants determined in the Imperial Physico-Technical Institute. The sensitiveness of the new photometer is about 1 per cent.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Realistic Elementary Geography: W. G. Baker (Blackie).—Elementary Inorganic Chemistry: A. H. Sexton (Blackie).—Insect Life, vol. i. No. 6 (Washington).—Mineral Statistics of Victoria, 1887.—Report of the Secretary for Mines (Melbourne).—Les Levers Photographiques et la Photographie en Voyage: Dr. G. Le Bon (Paris, Gauthier-Villars).—Annuaire pour l'an 1889, par le Bureau des Longitudes (Paris, Gauthier-Villars).—A Text-book of Human Physiology, 4th ed.: Dr. A. Flint (Lewis).—Manuel Pratique de Cristallographie: G. Wyrouboff (Paris, Gauthier-Villars).—An Illustrated Manual of British Birds, Parts 8, 9, 10: H. Saunders (Gurney and Jackson).—Annali di Agricultura, 1888: Prof. Giglioli (Rome, Rotta).—The Assistant to the Board of Trade Examinations: Captain Forbes (Relfe).—State Museum of Natural History, Albany, N.Y., 41st Annual Report (Troy).—A Text-book of General Astronomy: Dr. C. A. Young (Ginn).—La Legge del Diretto in Rispetto alle varie leggi di Natura: Prof. G. A. Longo (Catania, Martinez).—New Commercial Plants and Drugs, No. 11: T. Christy (Christy).—A Bibliography of Indian Geology: R. D. Oldham (Calcutta).—Report of the Commissioners on Inland Fisheries and Game for Year ending December 31, 1888 (Boston).—Journal of the Royal Statistical Society, December (Stanford).—Archives Italiennes de Biologie, Tome xi. Fasc. 1 (Turin, Loescher).—Himmel und Erde, Heft 5 (Berlin, Paetel).—Beiblätter zu den Annalen der Physik und Chemie, 1888, No. 12 (Leipzig, Barth).

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THURSDAY, FEBRUARY 7, 1889.

EARTHQUAKES.

Les Tremblements de Terre. Par F. Fouqué, Membre de l'Institut (Académie des Sciences), Professeur au Collège de France. (Paris: J. B. Baillière et Fils, 1888.)

IN the introduction to his little volume on earthquakes, Prof. Fouqué observes very justly that it is only in recent years that seismology has begun to shape itself to the lines of an exact science. Its students have of late concentrated their attention on questions susceptible of direct attack by observation and experiment. The older seismologists made the mistake of attempting to take the citadel by storm, and failed. The younger school of investigators, proceeding more gradually, have at least succeeded in showing how enormously complex the problem of earthquakes in their origin and propagation really is. The older seismologists were for the most part men with little knowledge of mechanics, and their fundamental mistake was that they under-estimated the difficulty of the problem in its mechanical aspect. Setting to work with a preconceived and quite false idea of its simplicity, they used such observational data as were at their disposal to build up an elaborate structure of inference and hypothesis—a structure very ill adapted to bear the shock of the first earthquake that formed the object of scientific measurement. The foundation on which the new school builds its science is exact seismometry; and so far, little, if anything, more than the foundation is laid. It is less than ten years since instruments of precision were introduced, capable of giving complete information as to the manner of motion of the ground. We now have sufficiently full and exact knowledge of the nature of the motion which takes place at one or another point of the earth's surface in the affected region while an earthquake is going on. The elaborate recording seismographs which have been brought to something like perfection by a few enthusiastic workers in Japan have analyzed this motion as completely as can be desired. But beyond this we as yet know next to nothing with any certainty about the real character of an earthquake. The relation which exists between the motion at one point and that at another, the manner of the motion below the surface, the transformations which the seismic waves undergo *en route*, are subjects hardly touched; and no seismometric observations have as yet been made, in a single case, from which conclusions may be drawn with any certainty as to the position of the origin and the nature of the originating disturbance. These are matters which used to be glibly settled by reference to a few projected stones or cracked walls, or to the stoppage of some village clocks: if the new seismometry has not yet thrown much light on them, it has at least shown how gross was the former darkness.

It is, then, not a little surprising to find Prof. Fouqué write a book on earthquakes without so much as a chapter on seismometers. He excuses himself from taking up this branch of the subject on the curious ground that its extreme importance makes it deserve a special treatise, and further, that, as the instruments are being improved from day to day, "nous pensons que la description des séismographes et microséismographes gagnera singulièrement à n'être exposée en détail que dans quelques années." Readers of NATURE, who have had the opportunity from time to time in these pages of seeing what present-day seismographs are able to do, will scarcely agree with the author; and granting, as we very well may, that many improvements have still to be made, the results already achieved in exact seismometry are surely such as not only to justify but to demand some description of these appliances in any new treatise on earthquakes.

In fact, however, M. Fouqué has been better than his word, for, in speaking of the "intensity" and the components of earthquake motion, he has given some slight account of seismographs and seismograms. But the account is far from adequate, and is not free from serious errors. We find the old fallacy restated, that the position of the "epicentre" can be determined by observation of the azimuths of the oscillations; that the bearing of the origin is given by the direction in which pendulums are set oscillating or objects are thrown down. Anomalous cases are spoken of, but not a word is said to explain that the cases which are styled anomalous form, not the exception, but the rule, because the chief oscillations are in general not of the normal but of the transverse type. With regard to the mechanical theory of seismographs, the author is completely at sea. It is now well known that the first essential in seismometry is to secure a point of reference by having a steady mass pivoted or hung in nearly neutral equilibrium; that a stably-hung mass like a common pendulum will not do, because it acquires oscillation through the more or less close agreement between its period of free swing and the period of the successive seismic impulses. Nothing could be worse than a pendulum with the period of which these impulses happened just to agree. Nevertheless, M. Fouqué says, without hinting dissent:—

"M. Cavalleri admet, d'après ses observations, que, dans un tremblement de terre, le meilleur pendule au point de vue de l'indication des intensités est celui dont les oscillations sont synchrones avec la durée de l'ondulation du sol; les pendules à fil long donnent le tracé le plus étendu quand les mouvements du sol sont lents; l'inverse a lieu quand les vibrations sont rapides. Par conséquent, pour obtenir un tracé, qui soit l'image aussi fidèle que possible de l'intensité de la secousse, il faudrait avoir une série de pendules d'inégale longueur, et considérer exclusivement, parmi les tracés obtenus, celui qui offre les dentelures les plus allongées."

Nothing could be more complete than the misapprehension shown in this last sentence. Other indications lead one to conclude that the author's acquaintance with seismometry is not intimate, and that it has not been formed at first hand. His references to original sources of information are rare. He gives a fairly good account of the work of Milne and Gray on the measurement of the speed of propagation of artificial disturbances through the soil—a subject the author has himself investigated—but of the work of Ewing in measuring natural earthquakes, and of the continuation and extension of it by Sekiya, he is apparently ignorant. Ewing's horizontal pendulum seismograph is not described, and his duplex pendulum seismograph, although mentioned, is wrongly classed as an instrument that records the phases of the

motion in their relation to the time. The account that is given of the labours of Italian observers in the field of microseismometry is meagre and unsatisfactory, and the work of M. d'Abbadie and the Darwins in this connection is not so much as alluded to.

The only part of M. Fouqué's book which can be said to make any addition to existing knowledge is that which deals with the experiments conducted by the author and M. Michel Lévy on the speed of propagation of artificial disturbances through the earth's crust. In the first instance their method was the same as that used by Mallet and by Abbot; the seismoscope was a basin of mercury, in which the observer detected the arrival of the shock by watching in a telescope for the disturbance of the reflected light. In a second series of experiments, the personal equation of the observer was got rid of by causing a convergent pencil of light reflected from the basin to fall on a revolving photographic plate. The light was intercepted by a shutter which opened, through electric connection with a seismoscope at the origin of the shock, when the blow was given which caused the disturbance to be propagated. Then, until the arrival of the earth waves blurred the image, a sharply defined circular arc was photographed on the plate, the length of this arc serving to measure the time of transit of the waves. In other instances, where the explosion of dynamite formed the source of disturbance, the explosion was produced by an electric discharge which was made to photograph itself on the plate, thus registering the instant at which the disturbance originated more sharply than by the method of the shutter. Besides observations on the surface of the ground in various localities, and with various qualities of vibrating medium, some were made entirely underground. The author and his colleague made use for this last purpose of a mine at Commentry, by causing the explosion to take place in one gallery, 470 feet below the surface, while the seismoscope was set first on the surface of the ground and then in a second gallery 280 feet below the other. As a general result, it was noticed in all cases that the first thing to reach the seismoscope was a series of very small vibrations, which preceded the arrival of the principal shock. In surface propagation this principal shock was not unique: it was followed by several others, although the initial disturbance at the distant source had consisted in a single blow.

Notwithstanding the care and pains which have evidently been bestowed on the author's experiments, the results, as regards speed of propagation of seismic waves, seem to be subject to some uncertainty. The intervals of time actually measured were too small, and what may be called the personal equation of the apparatus was too large. The whole amount by which the record lagged through inertia of the apparatus and other causes is estimated to be 0·301 seconds. Taking one set of experiments (at Creuzot), the recorded time-interval, when the seismoscope was 490 metres from the source, was 0·105 seconds; but to this small quantity we have to add the above large error of 0·301 seconds before deducing the velocity. It should be added, however, that observations made at a more distant station gave results according well with the velocity so deduced. The velocities ranged from about 3000 metres per second in granite to 300 metres per second in sand. The results as to rocky soils are of the same order of magnitude with those of Abbot, and with the velocities which have been inferred from laboratory experiments on the density and elasticity of rocks. The figures given refer to the rate of propagation of the group of waves, as measured by the arrival of the first sensible motions—the motions, namely, which form the advancing edge of the group. But the group widens as it travels, and a much smaller speed would be deduced by reference to the passage of the principal wave or waves.

The first and main part of M. Fouqué's book, entitled "A General Study of Earthquakes," concludes with an interesting detailed account of these experiments. The second part will appeal to a wider circle of readers. It is a narrative of the principal earthquakes which have been felt from 1854 to 1887, including those of San Salvador in 1854, 1873, and 1879; of Simoda, in Japan, in 1854; of Ischia in 1883; of Andalusia in 1884; and of the Riviera in 1887. The account is pleasantly written, and is embellished by a number of photo-engravings showing the mischief wrought by these destructive shocks.

PERIPATUS.

Studies from the Morphological Laboratory in the University of Cambridge. Vol. IV.—Part 1. A Monograph of the Development of *Peripatus capensis.* Part 2. A Monograph of the Species and Distribution of the Genus *Peripatus.* By Adam Sedgwick, M.A., F.R.S. (London: 1888.)

IN these two numbers the editor has reprinted the five papers from his pen on *Peripatus*, which have appeared in the *Quarterly Journal of Microscopical Science* between 1885 and 1888. From the patient detail with which he has followed the developmental changes, and from the power of generalization from observation which he displays, this research may well be regarded as a model for those who are beginning embryological study. Memoirs such as these, on the other hand, are rarely distinguished for clearness of expression and lucid phrasing, and the one before us forms unfortunately no exception to the generality.

Since the observations of Moseley and Balfour, it has been anticipated that several difficult morphological problems presented by the Arthropoda would receive their solution from a study of the ontogeny of *Peripatus*, and in Mr. Sedgwick's hands this hope has been largely realized. The only other recent workers in this field, Miss Sheldon, Mr. Sclater, and Dr. von Kennel, have studied forms from New Zealand and the West Indies; and while the observations of the latter are in many points at variance with those before us, some of the disagreement is undoubtedly due to the different development of the different species. Like most of such primitive types, the species of *Peripatus* are widely and discontinuously scattered, and exhibit considerable structural and embryological discrepancy. Such a discrepancy occurs at the outset. The ovum of the New Zealand form is large, and consists mainly of deutoplasm; that of the Cape species, the subject of the present memoir, is smaller, and, while now actually devoid of yolk, forms a loose reticulum of protoplasm which appears to imply its former presence between the meshes: in both of these

segmentation is meroblastic. The ova of the West Indian *Peripatus*, again, are yet smaller, and totally devoid of yolk, and the segmentation is apparently complete. In other words, the reduction in size of the ovum, due to loss of yolk, which is still in process in *P. capensis*, has been achieved in the West Indian forms, and its effect shows how easily the phylogenetic significance of segmentation-types may be lessened.

We can only mention a few of the more important facts and generalizations of Mr. Sedgwick's memoir. Of great interest is the observation that the embryo, at any rate at and until the gastrula stage, is a syncytium, *i.e.* the various cells of which it is composed are only incompletely marked off from one another, being connected by radiating protoplasmic strands. Such a syncytial condition the author regards as more primitive than the complete separation of the segmentation-spheres from each other. Another point in the early ontogeny is that no part of the nucleus of the unsegmented ovum enters that central portion of the syncytium which becomes differentiated into endoderm; and in this connection some recent observations of Hickson on segmentation in *Millepora* are of value. The ovum in *Millepora* is almost devoid of yolk, while those of the Hydrozoa generally possess a large quantity; and we venture to think that an earlier yolked condition probably occurred in *Millepora*, though Mr. Hickson has pronounced to the contrary. The segmentation-nucleus breaks down into a number of deeply-staining fragments, which become scattered through the cell, and eventually arrange themselves as the nuclei of the blastula; and it is at any rate possible that a similar phenomenon occurs in the formation of the endoderm of *Peripatus*, since Mr. Sedgwick describes (p. 26) "small particles of a deeply-staining matter, which are neither visible in the unsegmented ovum nor in the gastrula stages, and which are not to be distinguished from nuclear chromatin." From whatever source, amitotic nuclei presently appear in the endodermal vacuolated protoplasm, and the enteron is formed by the confluence of these vacuoles. A solid gastrula is thus produced. These facts lead Mr. Sedgwick to discuss the course of the evolution of Metazoa from Protozoa. He pronounces in favour of a "nucleated Infusorian-like animal, with possibly a mouth leading into a central vacuolated mass of protoplasm," for the transition-type, as against a colonial Protozoan; and declines to accept Metschnikoff's hollow blastula as an even more primitive form than the solid gastrula.

The nephridia, the existence of which is one of the most remarkable features of *Peripatus*, present two special modifications, those of the third somite becoming the salivary glands of the adult, and those of the twenty-first functioning as generative ducts. The generative glands themselves are formed as two continuous tubes from the dorsal sections of somites 16-20 by a separation from the ventral sections and absorption of the septa. With reference to the cœlom, Mr. Sedgwick comes to several important conclusions. The mesoblastic bands appear as a proliferation of nuclei at the lips of the blastopore, which arrange themselves in groups round a succession of cavities to form the future somites; a mode of connection generally taken to imply an obscured enterocœle. This primary enterocœlic system of cavities is represented in the adult merely by the generative glands and the nephridia; the latter are, as the author insists, not connected with, but actual parts of, the cœlom, and open each into a hitherto undescribed vesicle in the leg, which at no period communicates with the perivisceral space. Heart, pericardium, and perivisceral cavity are the outcome of spaces *secondarily* excavated in the mesoderm in connection with a vascular system, and are best designated by Lankester's term hæmocœle. Such a hæmocœle is characteristic of Mollusca and Arthropoda, and Mr. Sedgwick's deductions tend to show that in the latter group also the generative glands and ducts and the excretory antennary glands are the sole remnants of the true cœlom.

In the second part, which deals with the genus from a systematic stand-point, Mr. Sedgwick criticizes the various forms hitherto described. He recognizes nine good species, of which two are new: four from South Africa, two from the Australian region, and three from the Neotropical. The coloured plates which illustrate this section are most creditable to the lithographers of the Cambridge Scientific Instrument Company. In future volumes of the "Studies" an *exact* reference to the place where the original paper is to be found would often spare trouble to the student of zoological literature.

THE TEACHING OF CHEMISTRY.

The Fundamental Principles of Chemistry practically taught by a New Method. By Robert Galloway, M.R.I.A., F.C.S. (London: Longmans, Green, and Co., 1888.)

THE first thing that strikes one in taking up this volume is that it requires cutting. This is a considerable drawback to the student working from it, especially in those cases where the description of an experiment is continued on the next page, and to the mere reader it involves a trouble that ought not to be imposed upon him. But a far more serious fault is the absence of even an attempt at an index. Whether the author, the publisher, or the binder is to blame for this omission is not obvious, but the fact remains that the book is incomplete.

The difficulty as to where the beginner shall begin must have presented itself in some form or other to every earnest teacher. Shall the facts come first in their then necessarily isolated condition? or shall the student begin with theories, making for himself so many mental pigeonholes into which the facts as they come may be put away in an orderly manner?

The majority of teachers at the present day prefer to have something to classify before they attempt a classification with their pupils, and in so doing we think they adopt a perfectly sound and natural method. The earnest student is anxious to get on from the very first day of his course, he craves to get hold of something tangible; and the teacher who treats him like an empty reservoir, that is to be elaborately prepared and carefully tested as to perfection of soundness before any water is admitted to it, will ignominiously fail.

Probably every teacher of chemistry has found difficulty

in getting his students to understand in a satisfactory way the effects of changes of temperature and pressure upon gases individually and in general, after the student has performed the ordinary elementary experiments upon the principal gases. But turning to this subject in the present hand-book, we find that the student has to study these effects before he has seen or read of any gas whatever, unless we suppose that "the gas" at gas-works, or the air mentioned in earlier chapters, will be retained in the student's mind and applied by him to the rules given. In the questions set on this part of the subject, "a gas" is the vague expression almost always used, for fear, we presume, it should be imagined that the rules given are more intimately connected with oxygen than with hydrogen, &c.

In the early pages the learner is introducd to gases by the statement that gas-holders are "employed at gas-works for holding the gas," and is then instructed, without even the suggestion of an experiment, how to collect gases over water and in other ways, how to transfer gases to the lecture table, how to burn substances in gases, to burn gases themselves, to generate gases when heat is required and when heat is not required, and so on. The student, having got this abstract information in all its minuteness of practical detail, is expected to keep it in his memory, and to work and study through nearly two hundred pages dealing fully with, to him, a vast variety of complex subjects, before he can apply it to practical use in relation to hydrogen. By dint of much searching (for there is no index, and hydrogen does not appear to be mentioned at all in the meagre contents table) we have found a paragraph headed "Hydrogen" at p. 213. In this page no experiments are set down to be done, and the first suggestion of any practical exercise is the statement that "it can be obtained, as has been shown (Experiment 400), by electrolyzing water." The past and future are here confused, for Experiment 400 is twenty-two pages further on. This is apparently an unintentional memory exercise for the student. A few lines below, it is stated that "it is usually obtained by the action of H_2SO_4 or HCl on Zn or on Fe (see note, p. 183)." At this page we find a jar of hydrogen is required for an experiment (to extinguish burning phosphorus with), and in a note a method by which hydrogen "may be prepared" is given, with far too little description for a beginner and far too much for anyone else. We venture to predict that before many students have worked through this volume, one will be found to march off with a jar to the "gas-works" to get it filled with hydrogen, with the full conviction that he is carrying out, if not the specific instructions before him, at least an alternative way set down in the book to get his hydrogen to extinguish his phosphorus with.

There is a large measure of truth in the old saying that "example is better than precept," and this when translated into chemical language tells us that "experiment is better than theory." Theories in chemistry are of no use whatever to the student except as they enable him to remember, classify, and utilize his facts; and if the theories are to be divorced from the facts, or if the facts are only to be introduced as if they were accidental illustrations of the theories, then the study of—so-called—chemistry becomes as useless as the study of the dead languages. We consider that any method of teaching that tends to lead the student of chemistry to regard the theories he has to learn as anything more than suggestions that will be of assistance to him, is calculated to injure whatever of scientific capability he may possess. Good and useful theories have been believed in, and they have had to be modified, enlarged, or rejected as the growing richness of facts has demanded more extensive ideas. To teach the theories without the facts is to teach the fallible side of the science, and to make the theories more important than the facts is to attempt to balance a pyramid upon its apex.

OUR BOOK SHELF.

Treatise on Meteorological Apparatus and Methods. By Cleveland Abbe, A.M. (Washington: Government Printing Office, 1888.)

METEOROLOGICAL observations have been made more or less continuously since the days of Ferdinand II., Grand Duke of Tuscany, who first organized systematic observations in the year 1653. A full account of the progress which has been made since then in securing data of greater accuracy is contained in the book before us, which forms the forty-sixth appendix to the Report of the Chief Signal Officer to the United States Government. There are five different sections, one being devoted to temperature, one to pressure, one to atmospheric movements, one to aqueous vapour, and the last to the measurement of rain and snow. Each section commences with a general statement of the object to be attained, then the formulæ for correction are discussed, and finally there are descriptions of the most accurate instruments which are at present available. Every form of meteorological instrument hitherto conceived seems to find a place in this wonderfully complete treatise. Besides the ordinary instruments, all the self-recording arrangements are described, and their relative merits discussed. Diagrams of most of the instruments are also given. Those who have but a slight acquaintance with the subject will no doubt be surprised at the number of different methods of determining the same data, and at the number of corrections which it is necessary to make before the results can lay claim to scientific accuracy. The methods and standards adopted by the International Bureau of Weights and Measures are fully considered in every case where they are applicable.

The treatise will be invaluable to all meteorologists, and will undoubtedly do a good deal towards extending the usefulness of meteorological observations generally. Other treatises on optics, electricity, and actinometry are to follow.

New Zealand of To-day. By John Bradshaw. (London: Sampson Low, 1888.)

Round about New Zealand. By E. W. Payton. (London: Chapman and Hall, 1888.)

IN each of these books there is a full and interesting account of the present condition of New Zealand. Mr. Bradshaw's indignation has been excited by some of the hasty judgments expressed by Mr. Froude in "Oceana," and "New Zealand of To-day" may be regarded as to some extent an answer to Mr. Froude's criticisms. Mr. Payton's book consists of "notes from a journal of three years' wanderings in the Antipodes," and the impression produced by his narrative is not essentially different from that of Mr. Bradshaw's more polemical work. Both writers believe strongly in the future of New Zealand, and express warm admiration for the great results already achieved by the colonists. Yet it cannot be said that

either writes extravagantly, or that, in describing the social, industrial, and other characteristics of the colony, they have allowed themselves to be unduly swayed by mere feeling. They have, of course, a good deal to say about the Maoris, and it is worth noting that each refers to habits and physical conditions which cannot but tend to hasten the decay of that interesting race. A strong liking for whisky is unfortunately characteristic of most Maoris, and Mr. Payton remarked that the state of drunkenness appeared to have a great fascination for them. "I once saw a Maori that I knew," he says, "walking up and down the veranda of an hotel, and looking very much disgusted about something. On my asking him what was the matter, he told me he had had thirteen glasses of whisky, and *couldn't* get drunk!"

LETTERS TO THE EDITOR.

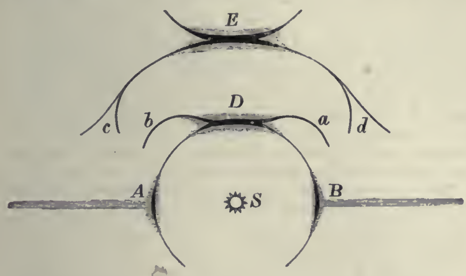
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Solar Halo.

BETWEEN 1 and 2 p.m. of January 11, a solar halo, so remarkable as to deserve some notice in the columns of NATURE, was observed and sketched by myself and several of my pupils. The mock suns A, B, and D (see diagram below), appeared to be at the usual distance of about 22½° from S, and the halo at E about the same distance from D.

A and B were quite bright, but D and E were nearly twice as brilliant, and blazed with gorgeous prismatic colours.

The parhelic circle—observed by Prof. William Ellis on April 1, 1886 (NATURE, vol. xxxiii. p. 535)—was very bright. It extended only from the mock suns A and B outwards from S to about 120° from the latter; and on the right branch of this circle was another mock sun (not shown in diagram) at the distance of about 90° from B. This last sun, as well as the visible portions of the parhelic circle, was formed of pure white light, and the latter was everywhere parallel with the horizon.



But perhaps the most remarkable part of the phenomenon was the forking of the arc *c* E *d* at the ends *c* and *d*, and the concave recurving of the arc *a* D *b* (convex to S at D) at the ends *a* and *b*. These forkings and recurvings were very distinctly visible at about 1.30 p.m., traced in fainter prismatic hues.

There was a light cloudy haze covering the southern two-thirds of the sky, while the remainder was clear. Calm moderate weather both preceded and followed the phenomenon for some days.

EVAN MCLENNAN.

Brooklyn, Iowa, U.S.A., January 14.

[The altitude of the sun is not given, but (according to Bravais) it must have been less than 30°, because of the extreme vividness of the tangent arc to the halo of 46°. This also accounts for the "recurved" appearance of the tangent arc to the halo of 22°. The apparent bifurcation of the halo of 46° is too rudely drawn to afford the means for a rigorous investigation. As sketched, it may be due solely to diversity of inclination (*balancement*) of the axes of the ice-crystals.—ED.]

Seismic Disturbance at Venezuela.

ABOUT the middle of November 1888, there was a notable seismic disturbance in several places of Northern Venezuela. On the 13th, at 4h. 30m. a.m., a rather heavy concussion was felt at Caracas, and eastward as far as Rio Chico, where it caused some damage. On the 17th, two shocks were noticed at Cumaná, viz. at 5h. 8m. a.m. and 2h. p.m. It is reported that their force diminished towards the east, so that they were scarcely perceptible at Carúpano. On the same day two shocks (1h. 45m. and 5h. 15m. p.m.) damaged in a somewhat serious manner a large number of houses at Guanare (69° 20′ W. of Greenwich, 8° 45′ N. lat.); two more were felt at the same place on the 18th at 3h. p.m., and on the 19th at 1h. 10m. a.m. The ultimate sign of the paroxysm was observed at Caracas on the last-named day, a few minutes before five o'clock in the afternoon. The zone of disturbance extended from Carúpano to Escuque (63° to 70° W. of Greenwich), and embraced the whole mountainous part of Northern Venezuela. In some cases the wave-motion is said to have been plainly north-east to south-west; but the maximum of disturbance (first shock at Guanare) showed decidedly a direction from north to south, as results from the numerous cracks in damaged walls and the way in which free-standing objects were thrown off their bases. The clock at the telegraph station, which hangs on a wall running east to west, was likewise instantly stopped. Dr. Lisandro Alvarado, a physician who resided at Guanare, who communicated these facts to me, informs me at the same time that the cracks emerge in an angle of from 75° to 80°. It is therefore very likely that the centre of the shock was not far from Guanare towards the north, where the crystalline schists of the Cordillera break through the overlying clay-slates and Cretaceous rocks, which form the northern margin of the great plains or *llanos* of Venezuela. Guanare lies on the very edge of these plains (185 metres above the sea), where the Cretaceous formation rather abruptly is met by the extensive deposit of conglomerate which covers the plains. Any disturbance in the raised strata forming the southern slope of the Cordillera will thus manifest itself with particular intensity in the vicinity of this border-line. The whole disturbance belongs, of course, to the class of tectonic earthquakes, as, indeed, do all those which happen now and then in this country.

A. ERNST.

Caracas, January 6.

Opportunity for a Naturalist.

CAPTAIN JUAN PAGE, of the Argentine Navy, who is now in London, and read a paper on the exploration of the Rio Vermejo and Rio Pilcomayo at the last meeting of the Royal Geographical Society, has undertaken a new expedition for the survey of the Pilcomayo from the Paraná to the frontiers of Bolivia. Captain Page would be glad to give a place on the staff of this Expedition to a naturalist, who would thus have an opportunity of investigating the almost unknown fauna and flora of the Gran Chaco, through which the Pilcomayo runs. The Expedition will start from Buenos Ayres in June next, and be absent about six months. The naturalist would have to find his passage out to Buenos Ayres, and home, and his own equipment and collecting-materials, but on joining the Expedition would be free from charges. I should be glad to put any qualified person who might wish to avail himself of this excellent opportunity of exploring a most interesting country in communication with Captain Page.

P. L. SCLATER.

Zoological Society of London, 3 Hanover Square, London, W., February 4.

Mass and Inertia.

DR. LODGE (NATURE, January 17, p. 270) seems to have misunderstood the bearing of my letter on mass and inertia (January 10, p. 248).

I was careful to point out that my remarks on the advantages of a *force*-time-length system of units had reference solely to *procedure in teaching*. Dr. Lodge, failing to observe this, objects to the suggestion because it does not immediately afford an absolutely permanent, universal unit of force. It was not intended to do so. Anyone who has learnt dynamics and attained clear ideas, appreciates the convenience of the *inertia*-time-length system for the purposes of the record. But the teacher's business is with those who have not yet learnt, but who, knowing nothing

yet of inertia, are—in this country, at any rate—already accustomed to pounds or ounces as the practical units of force. My suggestion is simply, "Don't swap horses while you are crossing the stream."

Dr. Lodge appears to object to my using the word inertia in the sense of the coefficient *m*. But he does exactly the same in his own book on "Mechanics" (p. 49); and the usage is, I think, quite common.

A. M. WORTHINGTON.

R.N.E. College, Devonport, January 26.

As a student and teacher of physics who has come much into contact with engineers and other artisans, I venture to say a few words on the vexed question of dynamical units now under discussion in your pages. It seems to me that it would be a distinctly retrograde step to adopt the proposals which Mr. Worthington makes in a recent number of NATURE. It would amount virtually to a return to the cumbrous and discredited system of units in use in British text-books of dynamics before the appearance of "Thomson and Tait," and the introduction of the Gaussian units of mass, force, &c.

It is certain that, whether the word "pound" be properly used to denote a unit of force or not, a common usage of the term is to denote a certain quantity of matter—that which has the same gravity at the same place as the so-called standard of weight. This is a standard quantity of matter and is a constant. Now in dynamics the primary property of matter is inertia, and inertia alone. When we compare the masses of bodies dynamically, we compare only their inertias; and that the forces of gravity on different bodies are proportional to their masses we have from Newton's pendulum experiment, &c. It seems natural and convenient, therefore, starting from this primary property of matter, to take the unit of mass as we find it defined, and give to that unit the unit of inertia. Then if the numeric of mass of a particle be m, of its acceleration dv/dt, the numeric of the inertia-reaction is $m \cdot dv/dt$ simply. The plan proposed by Mr. Worthington would introduce quite gratuitously the relation of his "unit of inertia" to the unit of mass, a relation which has been in the past—and would, I fear, be again—a great source of confusion to the student.

It is to be remembered, further, that the Gaussian system of units has been adopted by most civilized nations for practical electrical work. Certain units are constantly used in electrical engineering, which are simple multiples or submultiples of the various derived units in this system. It is too late in the day to change all this, and thereby run the risk of throwing things into the state of chaos from which with great labour and trouble they have been rescued. Hence the engineer, whatever units he uses for steam-pressures, &c., must, if he is taught dynamics at all, be taught how to express results given in gravitational units in terms of units independent of locality, or any other varying circumstance. It seems to me desirable, therefore, on the ground that the Gaussian system of units is in use in a great and growing department of engineering, to adopt it in our teaching at the outset. The true relations of other units is then got at once, and unfailingly.

My experience as a student and as a teacher is all in favour of the system and nomenclature followed by the persons whom Prof. Greenhill (I think) called "precisionists." Words are, of course, used in more senses than one in popular language; but if a popular word, such as "pound" or "weight," is to be adopted for scientific use, a restriction of its meaning to one sense is absolutely necessary if confusion is not to result. This, at any rate, is the principle on which scientific nomenclature has proceeded hitherto. This precision in the use of terms is absolutely necessary in teaching, and confusion of thought cannot be avoided without it. Of course there is want of consistency—no teacher can be perfectly precise; but that is hardly an argument for throwing precision overboard altogether.

Methods of teaching, after all, must stand or fall by their results, and I should like to join my testimony to that of those who say from experience that the Newtonian method in its original simplicity, with the system of units which Gauss gave, and which has produced so great and far-reaching scientific results, is the best way of approaching the study of dynamics. Students properly taught in this way have no difficulties beyond those inherent in a confessedly difficult subject.

ANDREW GRAY.

University College, Bangor, January 28.

Use of Sucker-Fishes in Fishing.

WITH reference to Mr. Sclater's note in NATURE of January 24 (p. 295), on the use of the *Remora* in fishing, I would like to call attention to the use of sucker-fishes by the aboriginal inhabitants of Cuba. Ferdinand Columbus ("Churchill's Voyages," 1704, vol. ii. p. 616) says these people used the sucker-fish to catch both other fish and turtles. These fishes when tied "by the tail run themselves against other fish, and by a certain roughness from the head to the middle of the back, they stick so fast to the next fish they meet, that, when the Indians perceive it, drawing their line, they draw them both together."

Lightcliffe, Yorkshire, January 26. H. LING ROTH.

Remarkable Rime and Mist.

THE extraordinary rime described by your correspondents was also experienced here (at 425 feet above sea-level) in January.

Though not an unusual occurrence in severe weather, this has never been equalled in my recollection.

The freezing fog lasted three days, each succeeding one appearing to add to the thickness of the rime, which culminated on the 6th, when it was difficult to believe that the trees were not covered with snow. On that date I measured one of the sheaves of spiculæ attached to a terminal shoot of a beech-tree, and found it very nearly $2\frac{1}{2}$ inches in length. This, of course, was rather exceptional. E. BROWN.

Further Barton, Cirencester, February 1.

IT seemed to me scarcely necessary to mention the amount of what may be called, for the sake of brevity, "sooty matter," in the rime referred to by Mr. Maw (p. 295). Some of the products of combustion are frequently restored to the ground without contact with water particles; but many are carried about in the atmosphere for a considerable time, and are returned to the earth through aqueous precipitation. I am not sure that the subject of the varying results of analyses of rain-water, obtained under various conditions of weather, has received the amount of attention which it deserves. The heavy rains of our summer thunderstorms seem to contain less sooty matter than is brought down in drizzling rain, when we have made the necessary allowances for direction and force of wind, hygrometrical and thermal conditions, type and quantity of previous rainfall, &c. This is probably due to the fact that rain-drops of the thunderstorm fall from the greater altitude and fall more vertically through the lower strata of the atmosphere. I should, however, like to learn from some readers of NATURE whether the larger rain-drops may not also, from the motion of air which they produce, treat some of the particles of sooty matter with the kindly neglect shown by them to the midges. Snow (as, I suppose, most people have observed by the sense of taste, without chemical analysis) contains, when melted, more sooty matter than rain, and I should have expected the inhabitants (including, of course, the tobacconists) of certain localities on our globe to feel rational gratitude to those slanting flakes which, in their voyage through our air, cleanse it of its sooty particles at those seasons when we are most fertile in producing the latter. But the drifting fogs which traverse a considerable area of land where there are factories, chimneys, &c. (their water-particles moving in lines nearly concentric with the earth's surface, and at no great height above it), should give the air a more thorough washing than is provided by the more common forms of precipitation. The ice-crystals produced by such fogs necessarily furnish, when melted, a maximum of sooty matter.

The letter from Mr. Lowe (p. 319) confirms what was anticipated, that the fog and rime were considerably less in the west of England than in the Midlands. It is perhaps contrary to the rules of good taste for me to criticize the words of so great an observer as Mr. Lowe, but is it not a contradiction of the laws which govern atmospheric phenomena, considering the great distance between true cirri and the fog described, to suppose that the upper surface of the latter "rapidly changed to cirri clouds"?

Speaking of mist, it is almost impossible not to refer to the very interesting article by Prof. J. H. Poynting, F.R.S. (p. 323). May it not be possible that the quivering so often seen in a summer haze is, after all, the result of evaporation, as Wordsworth, the poet of Nature, himself seems to have thought in his use of the word "steam," in "The Excursion."

ANNIE LEY.

PENETRATION OF DAYLIGHT INTO THE WATERS OF THE GENEVAN LAKE AND INTO THE MEDITERRANEAN.

A SPECIAL COMMISSION appointed by the Society of Physics and Natural History to study the colour and transparency of the waters of the Lake of Geneva have investigated the extreme limit reached by daylight in the depths of the lake.

Many naturalists have investigated this interesting subject Amongst others, Prof. F. A. Forel, of Morges, spent much time in investigating the Lake of Geneva. Using paper specially prepared for photography, M. Forel arrived at very interesting conclusions as to the greater transparency of the water in winter than in summer. But the paper was not sufficiently sensitive to permit of the determination of the exact limit to which the light of day extends in the depths of the lake.

Another Swiss naturalist, M. Asper, engaged in the same research, and examined several lakes, especially Zürich and Wallenstadt, for the extreme limit of the penetration of daylight. He made use of dried photographic plates of gelatine-bromide of silver. Proceeding in the same way as M. Forel, he put his plates in in the night, and took them out the following night, thus neglecting the action exercised on the sensitive plate by the light which still exists in the sky, even on a moonless night, at the moment of putting in and taking out.

This action, however, should not be neglected, and its neglect is the source of considerable error. The researches of M. Asper were carried out in lakes where the waters are less pure, and therefore less transparent, than those of the Genevan lake; they are therefore special to the lakes which he studied, and do not bear upon our lake. M. Asper, having found light at the lowest depth to which he immersed his plates (140 metres), could not answer the question as to the limit of penetration.

1. *On the Extreme Limit of the Penetration of Daylight into the Waters of the Lake of Geneva.*

The recent work, like that of M. Asper, consisted in exposing photographic plates at various depths in the deepest parts of the lake. The rapid gelatine-bromide plates of Munkhoven were used. A special apparatus, warding off from the sensitive plate all light other than that which really penetrated at the depth to which it was plunged under the water, was added. This apparatus, constructed by the Genevan Society for the Construction of Instruments in Physics, consists of a box of rectangular shape, of brass, 40 centimetres in length and 20 in breadth, containing in the middle a sensitive plate fixed by wedges. This frame is closed in the upper part by two brass shutters, gliding into grooves with double borders, separated from each other by means of a strong spring contained in the bottom of the box, so as to leave the plate entirely uncovered. At the bottom of the frame a strong shaft is fixed in the form of a ⊥, which bears the axes of rotation of two levers coupled in the form of scissors. Each of the levers ends in the upper part in a fork whose teeth pass from one side of the frame to the other, and lean against two branches which each of the shutters bears. Under the action of the inner spring the shutters separate, and with them the two arms of the lever. A weight is suspended at the opposite extremities of the two levers, and acts on them like the pressure of the fingers closing a pair of scissors; the two forks approach one another, and with them the two shutters, which then cover the plate entirely and defend it from any luminous action from without. A hook fixed to one of the shutters, turns on the apparatus when it is shut, and hangs in the groove of the other shutter, preventing them thus from separating during the transport from the dark chamber to the site of the experiment; it is raised only at the moment that the apparatus is attached to the sounding-line and worked on by the weight; as soon as the sounding-weight reaches the bottom, it opens under the action of the antagonistic spring; it closes again immediately it is withdrawn, and the weight leaving the bottom recommences to act. The depth having been ascertained by previous sounding, the length of the cord is regulated by which the weight is suspended to the apparatus, 100, 200, 300 metres for instance, at the distance desired from the surface of the water. After exposure for a certain time, the apparatus is withdrawn and carried into the dark room established on the ship in order to change the plates, and, if necessary, to develop them immediately.

The duration of the exposure has been about ten minutes. The development has been effected by means of oxalate of iron, with which the workers acted on each plate for ten minutes. The plates were all covered by the same emulsion.

Experiments have been made near Evian, where the lake presents a pretty wide plain at 310 metres depth. M. Marcet was twice kind enough to put his steam yacht, the *Heron*, at the disposal of the Commission; and Prof. Forel, of Morges, was kind enough, not only to lend his sounding-line, but to aid by his advice and his experience.

August 16, 1884, weather clear, sun bright: (*a*) at 237 metres, two plates, one at 12.30, the other at 1.7; (*b*) at 113 metres, a plate at 2.30; (*c*) at 300 metres a plate at 2.44.

September 23, 1884, dull, but very clear weather, thin, pretty luminous clouds, light wind varying from east to north, we exposed: (*d*) at 147 metres, a plate at 1 o'clock in the afternoon; (*e*) at 170 metres, a plate at 2.26; (*f*) at 113 metres, a plate at 3.3; (*g*) at 90·50 metres, a plate at 3.34.

For the sake of comparison, M. Fol had, on August 15, at 10 p.m., exposed on a clear but moonless night: (*h*) a plate in the open air for ten minutes; (*i*) a plate in the open air for five minutes.

It was found that plate *c* (300 metres in depth) had received no luminous impression whatever. It was the same with plate *a* (237 metres). Plate *e*, at 170 metres, was lightly veiled, almost like plate *i*, exposed for five minutes during the night. Plate *d*, at 147 metres, had been vividly impressed, more than plate *h*, exposed in the night for ten minutes. Of the two plates at 113 metres, the plate *f* of the second day is much darkened, whilst plate *b* of the first day is no more impressed than plate *d* of the second day. Plate *g*, exposed at 90 metres, is so impressed that characters which had been traced on the back are only incompletely reserved on the dark background of the developed layer.

On comparing the results obtained in the two days of experiment one is struck by the fact that the photographic effect was much stronger on September 28 than on August 16.

From these two attempts the conclusions are drawn:—

(1) That daylight penetrates into the waters of the Lake of Geneva in September at 170 metres depth, and probably a little beyond that; that at this depth the amount of light in the day is almost similar to that perceived in a clear moonless night.

(2) That at 120 metres the action of transmitted light is still very strong.

(3) That in September, in dull weather, light penetrates in greater abundance and more deeply into the water than in August in fine weather.

Later experiments will show us whether this difference is to be attributed to the greater transparency of water in autumn and winter, which the experiments of M. Forel ascertained beyond doubt, or if the light diffused by the clouds penetrates more deeply than the more or less oblique rays of the sun.

Before these experiments M. Asper had exposed plates of gelatine-bromide in the Lake of Zürich at depths be-

tween 40 and 90 metres, in the Lake of Wallenstadt from 90 to 140, and he obtained an effect from all. He put them in in the night, left them exposed a whole day, and took them out the following night. But, as the exposures of plates *h* and *i* have shown, the darkest night is still light for a plate of rapid gelatine-bromide.

2. *On the Extreme Limit of the Penetration of Daylight into the Waters of the Mediterranean Sea.*

After being assured by the experiments in the Genevan lake that their apparatus worked well, the Commission desired to make similar experiments in the sea, in which the greater transparency of the water would lead one to suppose that the extreme limit of the luminous rays would be at a still lower level.

No satisfactory experiments had yet been made in regard to this, for the experiments of the *Porcupine* were not carried out, M. Siemens's apparatus refusing to work.

Owing to the kindly mediation of Dr. J. Barrois, Director of the Zoological Station of Villefranche-sur-Mer, the *Albatros* was put at the disposal of the Commission for several days in the spring of 1885.

The method of procedure was the same as for the experiments in the lake, only that it was important to preserve the sensitive plate against the chemical action of the salt water by adding a thick layer of varnish to the bitumen. The luminous impression was made by the back of the plate and through the thickness of the glass. Repeated washings with essence of turpentine and alcohol sufficed to remove the varnish before proceeding to the development. As before, oxalate of iron was used.

The experiments took place on March 25 and 26, 1885, and were favoured by calm and fine weather. The depths wanted were found near Cape Ferrat, from 400 to 600 metres.

A. From 10.30 to 10.40, plate exposed at the depth of 200 metres to start with.

B. From 12.45 to 12.50, at a depth of 280 metres.

C. From 11.30 to 11.40, at a depth of 345 to 350 metres.

D. From 10.55 to 11.5, at a depth of 360 metres.

E. From 10.15 to 10.25, at a depth of 380 metres. This experiment took place under exceptionally favourable circumstances; there was no breeze, the ship remained perfectly stationary, and the line perfectly vertical.

F. From 1.20 to 1.30, cloudy but pretty light, at a depth of 405 to 420 metres. All these plates except F were exposed during bright sunshine.

Plates A and B were found to be very much impressed. On the plates C, D, and E the strength of the impression diminishes very regularly with the increase of depth. On plate E the strength of the impression is notably inferior to that of an exposure of the same duration in the air, on a clear moonless night. It may be compared to that of a shorter exposure, five minutes only, in the latter conditions. Plate F does not bear the least trace of any impression whatever. It is no doubt to be regretted that this last experiment did not take place, like the others, in clear weather. But the degree of the impression of plate E, of 380 metres, is already so weak that it may be pretty safely concluded that the extreme limit could not be more than 20 metres lower. On the other hand, the experiments in the Lake of Geneva have shown that the dispersion of the sunlight by a light layer of clouds does not bring about a notable diminution in the depth which it may attain in the water.

It is concluded, then, from these experiments, that in the month of March, in the middle of the day, with a bright sun, the last rays of daylight stop at 400 metres from the surface in the Mediterranean.

3. *Effects of the Seasons on the Limit of Penetration of Daylight in the Waters of the Lake of Geneva.*

The experiments of M. Forel, mentioned above, showed that photographic paper dipped in the lake is blackened in winter to a depth of 100 metres, whilst in summer it is not blackened beyond 45 metres. It is interesting to know whether this variation of transparency with the season belongs only to superficial layers, or if the same law holds good also at lower levels.

March 18, 1885, the Commission went into the middle of the lake on the *Sachem*, steam yacht of M. E. Reverdin, which its owner put at their disposal. As in former experiments on the lake, M. Forel was present. The weather was pretty clear; a light layer of clouds dispersed the light without arresting completely the direct rays of the sun. The following plates were exposed: (*k*) from 9.20 to 9.30, at 158 metres; (*l*) from 10.0 to 10.10, at 192 metres; (*m*) from 10.30 to 10.40, at 235 metres; (*n*) from 10.10 to 11.20, at 240 to 245 metres; (*o*) from 11.48 to 12.23, at 280 to 300 metres.

The duration of exposure was uniformly ten minutes for all, save the last, which remained uncovered, at 280 metres, for 35 minutes. In spite of that, not the least trace of impression was visible either on this plate or on plates *m* and *n*. The plate *l* was very faintly impressed, almost like plate E, of 380 metres in the sea. Plate *k*, at 158 metres, is of nearly the same force as C.

These experiments show that the extreme limit of the action of daylight in the lake in winter is a little beyond 200 metres.

A comparison between this series of experiments and the preceding shows that the light only descends 20 or 30 metres lower in March than in September; the difference is perhaps a little more considerable in the month of August. The layers of water situated below 100 metres escape the law of variation of transparency established by M. Forel for the more superficial layers; the variations of temperature accompanying the seasons, on the effect of which M. Forel bases his theory, not being sensibly felt beyond a certain depth.

Compared to the series of plates exposed in the lake, the series brought from the Mediterranean is striking by its slower and more regular gradation. This gives rise to the idea that whilst in the lake the light is promptly intercepted by deep layers more or less troubled, in the Mediterranean the absorption of the pure water would be the principal if not the only factor in the arrest of the luminous rays.

4. *On the Penetration of Light in the Depth of the Sea at Different Times in the Day.*

The preceding experiments led to very exact determinations of the extreme limit of the penetration of daylight in the waters of the Lake of Geneva and in those of the Mediterranean.

The following year, pursuing the same kind of experiments, the relations which exist between the depth to which light reaches in the water and the inclination of the sun or the variations in the amount of light were investigated.

The Report continues:—

"As we no longer sought a single limit, but a series of limits at fixed times during the day, we required a series of plates exposed at the same instant at different depths and capable of comparison with one another. Instead of a single large apparatus, like that which we had been using, we used twelve small ones, constructed on the same principle, which we placed at regular intervals of 20 metres along the cord. These apparatus were also furnished by the Genevan Society for the Construction of Instruments in Physics. They consist of a little rectangular frame of brass, in which glides in double grooves the drawer containing the sensitive plate, and which an interior spring tends always to open. The frame is fixed by two rings of brass, which at their upper part allow the axis of rotation of a lever to pass. The apparatus is suspended to the sounding-cord by an arm of this bent lever, whilst the lower arm acts on a spring fixed to the

drawer. The lower part of the cord is attached to a cross-bar which unites the bottom of the two rings. When the cord is stretched under the action of the sounding-weight, the upper arm of the lever falls into the prolongation of the cord, in a vertical position, and the lower arm keeps the drawer close. When the action of the sounding-weight ceases, the drawer opens under the action of the antagonistic spring. The process is the same as in the large apparatus, but in a slightly different form, permitting a much smaller and lighter order, which is more suitable to the superposition in series. To avoid the lower apparatus interfering by their additional weight with the opening of the higher ones, when the action of the sounding-weight ceases, each apparatus and the corresponding cord were exactly counterbalanced by a floater of glass in the form of a vial, inclosed in a net and attached to the cord immediately below the apparatus which is wished to be free. Under these conditions the apparatus all open simultaneously, immediately that the sounding-weight touches the bottom, and also close all together at the moment when the cord is drawn up and the sounding-weight begins again to act on them. The simultaneous exposure of several plates at various depths is thus obtained, and it is possible to follow the decreasing action of the light with the depth in one and the same experiment, all circumstances being equal."

The sensitive plates used in these experiments were those of extra rapid gelatine-bromide of M. Lumière, at Lyons. They were protected by a varnish from the action of the sea-water. The duration of the exposure and that of the development were both ten minutes, as in the preceding experiments.

They were carried on in a locality presenting a depth of about 500 metres, so that the purity of the water and the limit of the light were not influenced by the nearness to the bottom. The place chosen was 1300 to 1500 metres from the Cape of Mont Boron, separating the strand of Villefranche from the Gulf of Nice.

Amongst the series of plates obtained the following succeeded well and are particularly instructive.

Series A.—Between 1.15 and 1.25, April 7, 1886, the sun being about 60° above the horizon. The sky was very clear and the sun brilliant; a moderate breeze from the east made little waves.

Plate 1. Exposed at 430 metres: no trace of luminous impression.

Plate 2. Exposed at 390 to 393 metres: a very faint trace, but yet a clear one.

Plate 3. Exposed at 350 metres: a still faint impression.

Plate 4. Exposed at 310 metres: a strong impression.

Plate 5. Exposed at 270 metres: a very strong impression.

Plate 6. Exposed at 230 metres: completely blackened, as were the following.

The limit of light is, then, very exactly towards 400 metres in April, in the middle of the day, in fine weather. This is as complete a confirmation as possible of the conclusion arrived at in the preceding experiments.

Series B.—Between 8.20 and 8.30, April 5, 1886. Sky veiled by a uniform layer of white clouds thick enough for the sun to project no shadow. Moderate breeze from the east.

Plates 1, of 450 metres, and 2, of 415, have no impression.

Plate 3, of 350 metres, presents a very slight impression, a little less strong than that of plate 3 (390 metres) of Series A.

Plate 4, of 315, is of the same force as plate 3 of Series A.

Plate 5, failed by accident.

Plate 6, of 245, and the following, are completely blackened.

Series C.—Between 6.5 and 6.15, April 8. The setting sun was hidden by a bank of black clouds. The rest of the sky was pretty clear, with some little cirro-strati with a faint white light. The light was altogether faint, and like that which is generally found when the sun has just set. The surface of the sea was little agitated, with a slight breeze from the west.

Plates 1, of 400 metres, 2, of 340 metres, and 3, of 300 metres, have no trace of impression.

Plate 4, of 260 metres, is of almost the same force as plate 3 of Series A.

Plate 5, of 220 metres, similar to plate 4 of Series A.

Plate 6, of 180 metres, like plate 5 of Series A.

Plate 7 and the following, completely blackened.

The limit in this last series may be placed with all probability at 290 to 295 metres from the surface.

We see from these experiments that the layers situated at 300 metres are lighted every day, not for a very short time, but all the time that the sun is above the horizon; at 350 metres the light penetrates at least during eight hours daily.

According to the tables that M. Holatschek has drawn up for the latitude of Vienna, especially after the photochemical experiments of MM. Bunsen and Roscoe, the actinic intensity of the light of the blue sky would be on April 21, 33 at 8.30 in the morning; 38·07, at noon; and 14·18 at 6 p.m.; that of the sky and the sun at once would be on an average, in April, 75 at 8.30 a.m., 133 at noon, and 15 at 6 p.m.

According to these figures, the depth that the actinic rays attain in the sea after the setting of the sun is very remarkable. The Commission wait, however, to have more numerous experimental proofs to try to calculate a formula of absorption of which they have to determine the constant for sea-water.

5. *New Experiments on the Effect of the Seasons on the Limit of Penetration of Daylight in the Lake of Geneva.*

The results of these, which require to be confirmed under better conditions, seem to indicate that the difference of transparency between the waters of the lake in winter and in summer is greater than one would have thought. On the other hand, the want of agreement with the experiments of Series 3 leads one to think that the distribution in extent and depth of the layers of troubled water brought by the affluents of the lake is subject to variations difficult to foresee and to appreciate.

The Report concludes by referring to certain improvements in the apparatus.

THE REPORT OF THE KRAKATÃO COMMITTEE OF THE ROYAL SOCIETY.

PART IV. of this Report, on the optical phenomena, by the Hon. F. A. Rollo Russell and Mr. E. Douglas Archibald, comprises 311 pages of letterpress, or the major portion of the work. Owing to the enormous mass of material which went on accumulating for nearly four years from the date of the eruption, as well as the complexity surrounding the optical phenomena, which in some cases were entirely novel, and in others differed both in quality as well as intensity from their normal analogues, it was plainly a work of some considerable difficulty to decide how best to arrange and discuss the data, as well as to avoid arriving at hasty conclusions from the first indications of appearances which continued in part right up to the beginning of 1887. When the wonderful sunsets appeared in this country, the idea of their being connected with the eruption of Krakatão was first suggested and traced out with remarkable clearness by Mr. J. Norman Lockyer, F.R.S., in his article in the *Times* (December 8, 1883).

From the results of certain experiments by himself, he

had concluded that, while under ordinary circumstances aqueous vapour preferentially stops by absorption the more refrangible blue rays, under certain circumstances somewhat analogous to the conditions under which the blue and red solutions of gold were obtained by Faraday, aqueous molecules exist which stop the red rays and transmit the blue. Such so-called "red molecules" were found to be larger than the "blue molecules."

Combining this with the facts which indicated an unusual extension of volcanic dust and vapour into the air by Krakatão, as well as the succession of dates, the occurrence of white suns in the Indian Ocean and blue suns at a distance and at noon, and the initially rapid progress of the coloured suns and twilight glows round the equator, and their gradual spread to the extra-tropics (at first only faintly realized through lack of sufficient data), he constructed the hypothesis that all the optical effects witnessed in England in November and December 1883 were, like those which preceded them, nearer the equator, traceable to the products of the August eruption of Krakatão, carried thither by the upper currents of the atmosphere. Although, in one or two minor details, such as a supposed south-to north line of coloured suns over India, the vastly greater mass of evidence ultimately collected by the Committee enabled them to arrive at a more correct conclusion, yet, in its main features, such as the east-to-west current along the equator, and the concatenation of such at first apparently unconnected phenomena as a sunset in London and a volcanic eruption in Java, the work of the optical section of the Committee has practically resulted in filling in the framework sketched out by Mr. Lockyer. As time progressed, fresh data came pouring in, which not merely testified to the universality of the phenomena over the north and south temperate zones, but helped to fill up the gaps which necessarily occurred over the oceans and near the equator.

In spite of all these links in the chain of circumstantial evidence, many persons still continued to doubt the connection of the extra-tropical twilight glows with the analogous appearances in the tropics. In the case of the blue and green suns, the evidence even at first, was too strong to allow much doubt that they were in some way or other connected with the eruption. Yet even so, the rate at which they travelled (from 70 to 80 miles an hour) along the equator was too much for some persons, whose powers of imagination could hardly grasp the enormous scale on which the operations were conducted; while in the case of the extra-tropics, all sorts of queer and gratuitous hypotheses were put forward to account for what they beheld from their own windows.

Now, a very cursory glance at the general data and evidence, as well as at the maps given in Section III., will, we think, convince the most sceptical that the grand series of optical appearances which were first seen in the neighbourhood of Krakatão on the day of its great eruption, extended themselves, at first rapidly in longitude, and then slowly in latitude, until they finally embraced the whole earth. It will also show that their arrival in Europe was but a mere incident in their spread over a region *fifty times as large*. All this, however, has had to be put forward in detail.

Other points which have had to be described or discussed were—

(1) The proximate cause of the abnormal twilights, and an explanation, as far as was possible, of the way in which they differed from ordinary twilights, both in quality and intensity.

(2) The coloured suns, large corona round the sun and moon, and the sky haze or eruption cloud which evidently caused them.

(3) Then came the geographical distribution, the height and duration of the glows, a list of analogous phenomena on former occasions, opinions put forward to account for the present series, and finally a general analysis of their connection with the eruptions of Krakatão in detail, each of which demanded a separate section.

To give some idea of the principal facts and conclusions of this part of the work, we will commence with the abnormal twilights, considered as local phenomena.

The phases of ordinary twilights have been investigated with much attention by Kepler, Le Mairan, Dr. Hellmann, and Dr. von Bezold,[1] of whom the last discussed them with wonderful clearness in 1863, and showed that certain sequences of colour and intensity take place normally, which have apparently been entirely overlooked until the present series brought the subject again into notice.

Thus the normal sunset consists chiefly of a series of bands of colour parallel to the horizon in the west in the order, from below upwards—red, orange, yellow, green, blue, together with a purplish glow in the east over the earth's shadow, called the "counter-glow." As the earth's shadow moves upwards towards the zenith, and passes invisibly across it, a reddish or purplish glow suddenly appears above the coloured layers in the west, in a spot which previously appeared of a peculiarly bright whitish colour. This purple glow is substantially the "primary glow," or, more definitely, "*erste purpurlicht*." It is peculiar in appearing *above* the horizontal colours, and in not extending far on either side of a vertical plane through the sun and the spectator. As this glow sinks on the horizon and spreads out laterally, it forms the first red sunset. After its disappearance, under *favourable conditions*, a second edition of twilight colours analogous to the first commences, with a similar bright spot (*dämmerungschein*) out of which a second purple light appears to be suddenly developed, and sinks on to the horizon as the secondary or "after-glow."

These are the normal phases of a complete sunset according to Dr. von Bezold, and the present series only appear to be abnormal in exhibiting certain peculiar yellow and greenish tints, a less-defined boundary of the earth's shadow, together with a much greater brilliancy, extension, and duration of the first, and particularly of the second, purple glows. The horizontal layers were less conspicuous than usual, and the abnormal extension of the purple light made it appear as though there was an inversion of the usual order of tints from below upwards.

In order to explain these and other peculiarities which we have not scope to describe, Mr. Russell starts with the observed fact of a sky haze which, in the tropics, tended to transmit blue or green rays in preference to red, and assuming that all the usual elements which are included under the term "optical diffusion" were present, viz. diffraction, refraction, and reflection, describes what should be the effects (1) assuming a haze composed of opaque particles, and (2) one composed of very thin reflecting plates into which condition a large proportion of the pumice ejected from Krakatão is shown to have been transformed. His conclusion is that the distinctive features of the Krakatão glows were due mainly to reflection from these fine laminæ, of rays already tinted in a certain order, by diffraction through the dust of the haze layer and the lower atmosphere, as well as by the selective absorption which ordinarily takes place in the more humid horizontal layers near the earth's surface. The direct as well as diffuse reflection by the plates and opaque dust, which lay, as Mr. Archibald has shown in Section IV., at a height of from 50,000 to 100,000 feet, of rays tinted in succession as both the direct and reflected twilight boundaries followed the descending sun, and the peculiar transmissive quality of the stratum for the more refrangible rays, appear to afford a reasonable explanation of the peculiar silvery glare, the unusual colouring, and the unusual extension of the purple glows.

It is admitted that diffraction played an important part, as it does in ordinary sunsets (Lommel, for

[1] *Pogg. Ann.*, Bd. cxxiii. (1863), pp. 240–76.

example, attributes all the red tints to this cause); but both in this section and those that follow, many considerations are urged against the view held by Prof. Kiessling that the development of the primary glow is chiefly due to diffraction, while the secondary glow is as confidently asserted to be due to reflection. One of the principal objections to the reflection hypothesis in explanation of both the ordinary as well as the present extraordinary development of the purple glow, is its limitation at first to a narrow band, a fact which cannot be explained by absorption, and which is equally at variance with Fresnel's law of reflection from small globular dust, which would be equal in all directions. On the lamina, and particularly the vitreous lamina assumption, however, it is intelligible, since the maximum reflection would then be like that from the sea, in the vertical plane through the sun and the eye.

Moreover, the richly coloured and prolonged secondary glows, which were the most characteristic feature of the Krakatão twilights, are shown by Mr. Archibald, in Section IV., when dealing with their secular duration, to have reached a distinct minimum when the large diffraction corona round the sun, from Prof. Riccò's observations,[1] appeared at its greatest brilliancy; while the curve of their duration on Plate XXXIX., representing Dr. Riggenbach and Mr. Clark's observations, shows that they never again reached the same brilliancy or duration as in the two or three months immediately succeeding their first appearance in Europe. Both these facts aid the conclusion arrived at by Mr. Russell, and indorsed by Prof. Kiessling, that they were reflections by the haze stratum of the primary glows. But if these were reflections, the question naturally arises, why not the primary also? and until more effective arguments are brought against this view, as well as Prof. Riccò's objections to Prof. Kiessling's theory of diffraction alone, which are detailed in Section I. (*c.*), p. 250, Mr. Russell's view of the origin of both glows seems to be the more probable as well as reasonable of the two. The haze stratum appears to have been capable of exerting two influences: one, diffraction of the sun's rays by its smallest particles, which, with the absorption and diffraction usually effected by the dust and vapour present in the lower atmosphere, caused the horizontal tinted layers; the other, reflection by its larger particles or laminæ of the horizontal layers, particularly of the lowest red one, when the earth's shadow had arrived at about 25° above the western horizon, and into a position whence the maximum reflective effect could be seen unmasked by a diffusely illuminated background.

The question of the blue and green coloration of the sun is next discussed by Mr. Archibald, particularly with reference to its intrinsic characteristics and physical origin. In Section VII., in which the distribution of the twilight glows and the blue suns on their first circuit of the globe is compared, it is shown that the mean limit of the band of coloured suns was about 11° north and south of the latitude of Krakatão right round the equator, while that of the glows lay 5° beyond this on either side. Along the latitude of Krakatão the colours were mostly white or silvery, and in one or two cases coppery. The colours thus evidently depended on the density of the stream, the glows appearing on its borders or fringes where it was less dense. A similar relation to density appears from a study of the diurnal changes with varying solar altitude, the sun appearing to change from blue near the zenith, through green, to yellow, or disappearance on the horizon. No direct physical explanation of such a phenomenon appears forthcoming, since, according to the physical laws enunciated by Lord Rayleigh and Prof. Stokes, the diffraction of light by particles of the same order of magnitude as a wave-length tends to sift out the shorter blue, and preserve the longer red waves of light. Repeated reflections by small particles tend to the same result.

[1] Section I. (*c.*), p. 241.

The explanation proposed by Mr. Norman Lockyer in his article in the *Times* attributed the blue colour of the sun to differences in absorbing power of particles of different sizes, the larger particles being supposed to transmit more of the rays near the red end of the spectrum and the smaller those of shorter wave-length. The difference of size has been shown by Prof. Kiessling to be inoperative so far as scattering is concerned, since for particles whose magnitude is of the same order as that of a wave-length of light, the only case to which Prof. Stokes's law applies, the intensity of the scattered blue rays will be always sixteen times that of the red rays. It can therefore only be explained as an effect of absorption, due to some particular absorptive property of the materials which composed the haze. The phenomenon of a blue or green sun has been observed under natural conditions, many of which are quoted, and in most cases where the air was filled with fine dust from a great variety of sources. It has also been artificially reproduced by Prof. Kiessling with dust-filled air and vapour of water, and particularly of sulphur. Several accounts are given in Section V. of blue suns seen in connection with former eruptions, and Mr. Whymper's observation during an eruption of Cotopaxi is conclusive as to the ability of the finest volcanic ejecta to cause such an appearance. The problem which still awaits solution is, What was the precise nature of the particles or gases which produced the absorption? It seems probable that they were metallic sulphides.

Mr. Archibald next deals with the sky haze and its peculiar effects, more particularly on astronomical definition. Here again it seems to have possessed a selective absorption of the red rays, for in two separate lunar eclipses, 1884 and 1885, the usual coppery tint of the moon was conspicuously absent. He then passes on to the peculiar large corona round the sun and moon, which was first observed by Mr. Bishop at Honolulu on September 5, and which, though less striking than the twilight glows, was, if anything, more uncommon, more constant, and more prolonged in duration. It was a true diffraction corona with a reddish border, and of almost exactly the same size as the ordinary ice-halo, viz. 45° in diameter. It lasted from September 5, 1883, up to October 15, 1886, since which date it has entirely disappeared. Its diameter has afforded an approximate determination of the mean radius of the smaller dust-particles composing the haze, which Mr. Archibald calculates to be 0·00006 of an inch. Its connection with, and independence of, the glows is discussed at length, but we have not space to refer to it.

In Section II., Mr. Russell gives a list of the first appearances of all the optical phenomena from the beginning of 1883 to the end of 1884, from which date the local duration of the glows is carried on by Mr. Archibald in Section IV. up to the end of 1885.

In Section III., Mr. Russell works out the geographical distribution of the optical phenomena, including blue suns and glows, up to the end of 1883, by which time they had virtually covered the whole earth. The general conclusion is, that the phenomena all propagated themselves (with the exception of a narrow offshoot towards Japan) at first due west from Java, at a rate of about 76 miles an hour right round the earth parallel to the equator, and in a band symmetrically disposed for 16° on either side of the latitude through Krakatão. A second circuit with wider limits, 30° north and south of Krakatão, was traced at the same rate, after which the motion became indistinguishable. They then gradually spread in latitude, and ultimately the haze which caused them appears to have invaded our latitudes, like the anti-trade, from south-west to north-east. These circumstances may be best realized from a survey of Mr. Russell's maps, especially that showing the successive limits of the appearances for the first nine days succeeding the eruption.

When Mr. Lockyer first pointed out his lines of coloured suns converging towards Krakatão, the data were too scanty for him to recognize that the apparent line through Ceylon, Ongole, and Madras, was due to a widening of the main east to west stream after its first circuit of the globe. As far as the motion of this stream was indicated by African and South American observations, he was perfectly correct, though nothing was previously known as to its velocity or even actual existence. The march of the optical phenomena which is shown in Mr. Russell's maps is indeed the only direct evidence we have of the fact that at 100,000 feet above the earth in the immediate vicinity of the equator, the air in August, and probably, as Mr. Archibald shows, at other times, moves in a rapid and constant current from east to west. Both in Section III. (*b*) and Section VII. he discusses this question in detail, and shows its agreement with the theory of the general circulation of the atmosphere, as well as the motions of the upper clouds as far as they have been observed.

In Section IV., Mr. Archibald investigates the height of the stratum, from observations in all parts of the world where the durations of the primary or secondary glow have been recorded with any attempt at accuracy. Proceeding on the hypothesis that the primary glow was a first reflection of the sun's rays by the stratum, and the secondary a reflection of the primary glow, for which ample evidence is adduced, he concludes that the height of the upper or middle part of the stratum above the earth, diminished from 121,000 feet in August 1883, to 64,000 in January 1884, the lower limits being practically indeterminate. Also, since from Dr. Riggenbach's and Mr. Clark's observations, the glows continued less brilliantly and less prolonged after the first few months right up to the end of 1885, while a decided minimum in the duration, and therefore presumably the height of the reflecting layer, was reached in April 1884; the important conclusion is arrived at, that by that date the larger and more effectively reflecting particles had descended to a lower level, leaving the finest particles suspended at nearly the same elevation as at first. This is further corroborated by the remarkable fact that the large corona reached its maximum intensity during the same month.

In Section V., Mr. Russell gives an interesting list of former eruptions and accompanying atmospheric effects, similar in many respects to those under discussion. During 1783 and 1831, the dates of the famous eruptions of Skaptar Jökull and Graham's Island, &c., and the two years of perhaps the greatest eruptive activity antecedent to that of Krakatão, the *after-glows* and other optical effects were most conspicuous, and from an examination of other eruptions and sequelæ, the general correspondence between the two phenomena seems fairly proven.

Section VI. is a *résumé* of opinions collected by Mr. Archibald, against and in favour of the volcanic origin of the phenomena. Besides its value in exhibiting every aspect of the question, it affords a curious illustration of the narrowness and breadth of human imagination, especially when dealing with phenomena whose universality and minor details were at first only partially realized.

Finally, in Section VII., Mr. Archibald gives a general analysis of the connection between all the optical phenomena and the eruptions of Krakatão both in May and August, in which the various objections on the ground of the initially rapid transmission of the appearances, insufficiency of fine solid ejecta, length of time of its suspension, and the occurence of *apparently* similar phenomena on dates previous to the great August eruption are discussed in turn. The time of suspension of the finest dust in particular, is shown—by an application of Prof. Stokes's formula,[1] $V = \frac{2g}{9\mu'}\left(\frac{\sigma}{\rho} - 1\right)a^2$, for the velocity of a small particle descending in air, and in which viscosity is properly considered—to be *over two years* between 50,000 and 100,000 feet, even assuming the particles to be spherical, which is the most unfavourable supposition. If, as is most probable, they were thin plates, the time would be much longer. A final summary is then given of the direct and local connection between the optical phenomena and the eruptions both of May and August, which the subsequent discovery of the relative though minor importance of the May eruption rendered necessary.

[1] Where σ, ρ, are the densities of the particle and the fluid respectively, a the radius of the particle (supposed spherical), and μ' the index of friction $= \frac{\mu}{\rho}$, where μ is the coefficient of viscosity. Its value is either $(0{\cdot}116)^2$ or $(0{\cdot}14)^2$.

In Part V., Mr. Whipple discusses the somewhat sparse data which show that a magnetic disturbance was generated by Krakatão and travelled round the world at a rate varying from 760 to 900 miles per hour, but the data are too uncertain to allow any definite conclusions to be drawn.

We cannot conclude without drawing attention to the fact that the study of the Krakatão sequelæ has not merely enlarged our conceptions of volcanic powers and the continuity of atmospheric circulation, as well as yielded positive information of great value to different branches of science, but has opened up fresh problems in optical and meteorological physics, the attack and solution of which will stimulate research as well as materially advance the boundaries of our present knowledge of these subjects.

SCIENCE AND THE REPORT OF THE EDUCATION COMMISSION.

THE Final Report of the Commissioners on the Elementary Education Acts, which has excited so much attention of late, deserves to be noticed from a point of view other than any that has yet been taken—that is, the attitude of the Commission towards the teaching of science in elementary schools. In a rather lengthy chapter on "Manual and Technical Instruction," the ground taken by the Commissioners is very clear. The teaching of science in our elementary schools, they say, is yet in its infancy. The importance of science teaching has been so far recognized that simple object-lessons are obligatory in all infant schools. This is a mode of teaching which the Report recommends to be extended. Thus in agricultural districts arrangements could be made for giving practical instruction of the simplest character in the principles of agriculture, the growth and food of plants, and their diseases; and similar instruction in the elements of dairy work might be given to the girls in these districts. The point here laid stress on is one that everybody who knows anything of former attempts to teach science to the children in Board-schools will feel the need of. Science, the Commissioners emphatically say, can never be taught to children out of books alone. No doubt much useful and entertaining knowledge is taken from text-books on science by children; but, to use the words of the Report, the true learning of science cannot be said to begin till the learners are taught to use their own senses in the study, and to rest their acceptance of scientific truth, even the most elementary, not on what they are told, but on evidence supplied by their own observation. To show the curious system of instruction pursued in our schools, it is worth while mentioning that though those simple object-lessons which lie, we are told, at the foundation of scientific instruction are compulsory in infant schools, yet immediately the child leaves the infant school they are discontinued until he reaches Standard V., after which, if he ever gets any scientific teaching, it is out of books alone. One witness considered it ridiculous that English is compulsory and elementary science optional, for, he said, English includes grammar and recitation for boys, the latter of which, he thinks, is far less likely to be useful in a manufacturing district than

elementary science. The preponderance of opinion amongst the teachers examined is that no subject is better calculated to awaken the interest and the intelligence of the scholars than science. So far as the present teaching of elementary science is concerned, it is in a most unsatisfactory condition. Mr. J. F. Buckmaster says that, in comparison with forty years ago, this department of school work has retrograded; and Sir Henry Roscoe agrees with the statement that the teaching of this subject is falling off. According to the latter witness, our system of education is intended to form clerks and not artisans: it is purely a literary system; and if we wish to preserve our manufacturing supremacy it must be changed. In fact, whatever has been done by the manufacturers and artisans of this country is owing in no degree whatever to the educational system, which rather retards industrial progress than otherwise; whilst on the Continent, where manufactures have made such strides of late, everything is owing to the technical schools and the scientific training of those intended for artisans. The vast majority of Board-school boys are intended for the workshop rather than the office: why, then, not fit them for the ordinary duties of life? If this be the object of elementary education—that is, the fitting of scholars in general for those duties which they will most probably be called on to perform—then, the Report says, elementary instruction in science is only second in importance to elementary instruction in reading, writing, and arithmetic. But the fact is, that though girls get some kind of training in, for example, needlework and cookery, there is no such thing as technical instruction for boys. Science, especially mathematical, mechanical, and physical science, is not only the foundation, but an essential part of technical instruction. With regard, however, to the teaching of science, certain warnings are necessary. In the first place, science teaching should not be allowed to interfere with the scholar's general instruction—that is, it should not be introduced too early in life. In the next place, the average teacher is useless as a science master. Even when by previous training the teacher is suited, he has scarcely time to devote to the preparation necessary to lecture clearly and to perform experiments accurately and neatly. Therefore, the Report suggests, the example of London, Liverpool, and Birmingham should be followed by School Boards, in engaging the services of a skilled science lecturer, who will go from school to school in a specified district. For example, in Birmingham, according to the evidence of Dr. Crosskey, the Chairman of the School Management Committee of that town, the demonstrator or an assistant visits each boys' and girls' department once a fortnight. He takes four departments a day, two in the morning and two in the afternoon. Two and a half hours a fortnight are given to science. The class teacher is present during the lecture, and recapitulates it to his scholars, who are bound to bring him written answers to questions thereon. Mechanics or elementary natural philosophy are the favourite subjects with boys,—sometimes they are taught electricity and magnetism; and the girls, domestic economy, considered as the application of chemistry and physiology to the explanation of matters of home life, and sometimes animal physiology. Not only have very great results been achieved by this system in the percentage of passes, but it is noticeable that the attendance is always largest on "science" days. Three years ago a lecturer was appointed for the East End of London, and he had at one time as many as two thousand boys under instruction in mechanics alone. The work has been so successful that three assistants have been appointed. Many witnesses justly complained of the kind of examination papers set in elementary science. One paper of questions is given at length in the Report. It is for boys of the Fifth Standard—that is, for boys between eleven and twelve—and consists of seven questions, of which a specimen, taken at random, may be given: "Quest. 2. In what bodies may you say that molecular attraction is balanced by the repulsive force of heat?" and written answers had to be given. Hence the suggestion of many experienced witnesses that younger children should be examined orally is taken up and recommended by the Commission.

Our system, or rather want of system, of education in elementary science is spoken of by the Commission as introductory to the wider subject of manual and technical instruction, to which they devote many pages of their Report. Into this we shall not follow them, but content ourselves by quoting a few sentences from the Report. "If it should be thought that children ought to receive some instruction in manual employment, other than that which the elementary schools available for their use can give, we are of opinion that the best way of meeting the need would be through the establishment of a workshop in connection with some higher institution, which might be willing to receive into the workshop boys of exceptional ability, or others to whom it was considered desirable to give this instruction. One such central institution could do its work better and cheaper than a number of scattered institutions, whilst nothing could be easier than to make arrangements for attendance at this central workshop being substituted on one or two afternoons in the week for attendance at the elementary school." To illustrate what might be done in this way, the case of the Seventh Standard School at Birmingham is quoted. This is to some extent a technical school, and no pupil is admitted unless he has previously passed in Standard VI. The subjects taught are reading, writing, and arithmetic, according to the Code; and, in addition, mathematics, plane geometry and projection, machine construction and drawing, magnetism and electricity, theoretical and practical chemistry, freehand drawing, and the manipulation of wood-working tools. These subjects are not generally taught to all pupils, but are divided into three divisions, to one of which, as a rule, the student confines his attention. The first division is the machine construction division, and includes mathematics, projection, machine construction, electricity, freehand drawing, and workshop; the second division is the chemistry division, and includes mathematics, projection, chemistry (theoretical and practical), freehand drawing, and workshop; the third division, the electricity division, includes mathematics, projection, theoretical chemistry, electricity, freehand drawing, and workshop. In the second year (the course is three years in duration) the scholars spend three hours a day in the workshop. This system, it is said, has produced excellent results.

NOTES.

The Council of the Royal Meteorological Society have arranged to hold at 25 Great George Street, Westminster (by permission of the Council of the Institution of Civil Engineers), on March 19-22 next, an Exhibition of Instruments connected with atmospheric physics invented during the last ten years, especially those used for actinic and solar radiation observations. The Exhibition Committee invite the co-operation of all who may be able and willing to send contributions. The Committee will also be glad to show any *new* meteorological instruments or apparatus invented or first constructed since last March; as well as photographs and drawings possessing meteorological interest.

We learn that Prof. Fitzgerald and Mr. Trouton have been conducting experiments confirmatory of Hertz's magnificent work. Lately, using parabolic mirrors after the manner Hertz recently described, they have observed the phenomenon of the polarization of radiations by reflection from a wall 3 feet thick. They observed long ago, and exhibited publicly at the opening meeting of the Experimental Science Association last November, that stone walls are quite transparent to these radiations, as they

should be, and consequently should not reflect radiations polarized perpendicularly to the plane of incidence at a certain incidence. This is what has been observed, and it has been decided that the plane of polarization is the plane of the magnetic disturbance. They next tried reflection from sheets of glass, and obtained no results; but, as Mr. Joly suggested, the experimenters were practically observing the black spot in Newton's rings, for the sheet of glass was much thinner than a wave-length, which is about 30 centimetres. Some rough observations at various incidences from the wall seem to show interference at some and not at other incidences due to the same cause as Newton's rings.

THE German Government have granted the sum of £27,500 to repair the building of the University of Berlin, and to erect new lecture-rooms, staircases, and corridors, and for the heating and lighting apparatus. The Government have also given £36,500 to the Natural History Museum, besides £2500 for books. A further sum of £1000 is to be devoted to the purchase of physical apparatus and an anatomical cabinet.

AT the Epidemiological Society, on February 13, Mr. Victor Horsley will read a paper, illustrated by lantern photographs and micro-photographs, on "Rabies, its Treatment by M. Pasteur, and its Detection in Suspected Cases."

A TECHNICAL LABORATORY for special instruction in dyeing and bleaching has just been opened in connection with University College, Dundee. This technical portion of the chemical department consists of a completely fitted dye-house, a laboratory, and a museum for technical samples, more especially connected with the textile industries of the district. Practical instruction in the dye-house was begun by Prof. Percy Frankland last week.

THE *Kew Bulletin* for February consists of a list of such hardy herbaceous annual and perennial plants as have matured seeds under cultivation in the Royal Gardens, Kew, during the year 1888. These seeds are available for exchange with colonial, Indian, and foreign botanic gardens, as well as with regular correspondents of Kew. The seeds are for the most part available only in moderate quantity, and are not sold to the general public. The compiler points out that, as compared with the list previously published (*Kew Bulletin*, February 1887), the number of names inserted in this list is far fewer. This, he says, has arisen on account of the unfavourable conditions experienced during the summer of 1888, when, owing to prolonged rains and absence of sunlight, many plants did not mature seed.

WE learn from *Petermann's Mitteilungen* for January that a Central Meteorological Institute was established on April 7, 1888, at San José, Costa Rica, under the Ministry of Instruction, with Señor Pittier as Director.

AN Intercolonial Meteorological Conference was held at the Melbourne Observatory from September 11–15 last, under the presidency of Mr. R. J. Ellery, all the Australian colonies, New Zealand, and Tasmania, being represented. The Chairman stated that the chief object of calling the Conference together was "with a view of improving the intercolonial weather system, and for the advancement of Australian meteorological science generally." The following are among the principal resolutions adopted: (1) that the amount of information sent by telegraph should be reduced as far as possible, by reducing the number of stations; (2) that, with the telegrams, a forecast be sent from each colony for that colony; (3) that no meteorological forecast or prediction made in one colony and having reference to any other colony shall be communicated by telegraph to any other person or destination than the meteorologist of the colony to which such prediction refers. This motion was carried with one dissentient, Mr. Wragge. A long discussion ensued as to the best mode of thermometer exposure. Mr. Todd stated that he had fully tested the Stevenson stand; he thought it was quite impossible for anyone to say positively what was the best form of exposure. He was going to adopt the Stevenson screen for his out-stations, but was not necessarily going to put it only 4 feet from the ground. The Conference generally agreed that they should all work towards uniformity in the matter as far as possible. On the question of the supply of self-registering instruments to certain stations, Mr. Russell said that he had found the most diverse results from Richard's instruments; some gave satisfactory results, and others were perfectly useless. Sir James Hector said he had been fortunate with the instrument; as an assistant to reading the weather, it was most efficient. On the question of cloud nomenclature the forms given by Mr. Abercromby were approved of; the Chairman urged the importance of having them lithographed in colours, and he undertook to get copies of Mr. Abercromby's photographs so prepared, and sent round for approval to the members of the Conference. Various other questions were discussed, including the connection of climatological observations with hygiene, and the reduction of barometer observations to sea-level.

FRENCH students of ethnography are much interested in the group of Lapps who are at present being exhibited at the Jardin d'Acclimatation, Paris. The other day the group was increased by one, a daughter having been born. The new-comer is called "Parisienne."

THE Geographical Society of Paris has just opened an interesting exhibition of photographs, dresses, weapons, boats, and other objects brought a short time ago from Greenland by M. Charles Rabot.

FOR many years the Kazan Society of Naturalists has devoted a good deal of attention to its collection of skulls, which is now one of the richest collections of the kind in Russia. Dr. Malieff gives, in the Memoirs of the Society (vol. xix. 2) a catalogue of the collection, with the chief measurements and indexes of each skull separately. The Slavonian and Russian skulls—partly from the seventeenth and eighteenth centuries, and partly more recent—are the most numerous; they number about three hundred. The old Bulgarian skulls, represented by thirty-seven examples, come next. The Permians, the Votyaks, the Tchuvashes, and the Tcheremisses are well represented. The like may be said of the Mordovian and the old Finnish skulls derived from the grave-mounds (*kurgans*). The Tartarian skulls are represented both by samples found in *kurgans*, and by modern specimens. Eighteen Kazan skulls found at Astrakhan are especially worthy of note.

RUSSIAN botanists are busily exploring the floras of the various divisions of the Empire, and the number of floras of separate provinces is rapidly increasing. At the same time, they are devoting closer attention to the delimitation of the various floras of European Russia, their chief efforts being directed towards the establishment of the separation lines between the flora of the black-earth or steppe region, and the flora of the forest region. M. Korzchinsky's new work on the northern limits of the former in the Government of Kazan, accompanied by a map which illustrates the conclusions of the author, is a valuable contribution to the subject (Memoirs of the Kazan Society of Naturalists, vol. xviii. 5). Full lists of the species characteristic of separate districts are given, together with a review of the literature dealing with the phenomena. Contrary to views formerly maintained, the author holds that in Eastern Russia there is no such separate flora as might be considered intermediate between the forest and the steppe

floras, and that the latter penetrates within the forest region in the shape of isolated and well-defined oases; while a mixed vegetation, containing species characteristic of the steppes, intermingled with representatives of the meadows of the forest region, is met with only sporadically on the banks of some rivers, on the southern slopes of some hills, or on the outskirts of the forests. He distinguishes, on the contrary, a vegetation which has its own special characters over very wide tracts—from Astrakhan to Yaroslav—and is met with on the banks of the Volga, Kama, and Vyatka, as well as on the banks of the lakes which formerly were connected with the rivers and their old beds.

THE *Annuaire du Bureau des Longitudes* for 1889 contains the usual astronomical and physical tables, and much useful information relating to meteorology and other subjects. There is an admirable list of variable stars, with their positions corrected for 1889, and the dates of maxima and minima. The particulars of cometary orbits are brought up to Olbers's comet of 1887. With the exception of the memoirs at the end, however, there is little novelty. M. Tisserand contributes a very useful paper on the measurements of the masses of celestial bodies, including planets, asteroids, satellites, comets, and binary systems; a better account of the application of the laws of gravitation to this purpose would be difficult to find. Perhaps the most interesting paper is that contributed by M. Janssen, on his memorable expedition to Mont Blanc for the purpose of deciding whether certain lines in the solar spectrum are due to oxygen in our air or in the solar atmosphere. The results of the observations made on Mont Blanc showed that these lines were due to our own atmosphere, as they were weaker there than at lower levels.

WE have received the *Annuaire de l'Académie Royale de Belgique* for 1889, this being the fifty-fifth year of publication. Besides the academical calendar for the year, it contains a general history of the organization and all particulars relating to each department. It also gives accounts of the Commissions delegated to superintend the publication of national biography and history. Details of the prizes and medals at the disposal of the Academy, and the recipients of them from the dates of their institution, are also given. About 340 pages are devoted to biographical notices of deceased members, which are accompanied by admirable portraits, and full particulars of the works of each. It is interesting and gratifying to note the frequent occurrence of "Leopold II., Roi de Belgique," in connection with the work of the Academy.

ANOTHER important paper is contributed by Prof. Emil Fischer and Dr. Tafel to the number of the *Berichte* just received, upon the syntheses of glucose and mannite. Not only has the method of synthesizing glucose been perfected, and large yields of it obtained, but it has also been shown to ferment with yeast like natural glucose, and also like glucose to yield, on reduction with sodium amalgam, the hexhydric alcohol mannite. The first process by which the synthesis was effected, and which was fully described in NATURE, vol. xxxvii. p. 7, consisted in treating acrolein dibromide with baryta-water, the sugar left in solution being afterwards precipitated by phenyl hydrazine in the form of a peculiar compound with the latter substance of the composition $C_{18}H_{22}N_4O_4$. This phenyl hydrazine compound was then reduced with zinc dust and acetic acid, the product being a base which, on treatment with nitrous acid, parted with its nitrogen, leaving a solution from which glucose was extracted by means of absolute alcohol. This method of obtaining glucose from the phenyl hydrazine compound was both exceptionally difficult in manipulation, and gave a very small yield of the sugar. The Würzburg chemists have recently discovered a far better process. The finely-powdered phenyl hydrazine compound is warmed with concentrated hydrochloric acid, when it dissolves to a clear dark red liquid. On cooling, phenyl hydrazine hydrochloride crystallizes out and is filtered off. The diluted filtrate is neutralized with hydrated lead carbonate, decolorized with animal charcoal, again filtered, and the solution made slightly alkaline with baryta-water. A syrupy substance of the constitution $CH_2OH \,.\, (CHOH)_3 \,.\, CO \,.\, COH$, which differs from glucose in possessing two atoms of hydrogen less, is at once precipitated in combination with lead. The precipitate is next treated with sulphuric acid and again neutralized with barium carbonate. The filtered solution, at last obtained almost pure, is evaporated *in vacuo* upon a water-bath, and the syrup extracted with absolute alcohol; on evaporation of the alcohol, the syrup is left behind in the pure state, and solidifies on cooling to a hard amorphous mass. It is only necessary now to reduce the aqueous solution of this syrup with zinc-dust and acetic acid, when the two additional hydrogen atoms are taken up and glucose formed. The zinc is precipitated from the filtered liquid by sulphuretted hydrogen, and the filtrate evaporated *in vacuo* upon the water-bath. On extraction of the syrup with absolute alcohol, and subsequent addition of ether, the sugar is precipitated in colourless flocculæ, which rapidly coalesce into a sweet-tasting syrup of pure glucose. By this method so large a yield has been obtained that fermentation experiments have been possible; and with beer-yeast this artificial glucose is found to rapidly ferment, evolving abundance of carbonic anhydride at the ordinary temperature. It reduces Fehling's solution, and only differs from natural dextrose and lævulose in being optically inactive, consisting, as it probably does, of a mixture of equal molecules of the right- and left-handed varieties. Further, by action of sodium amalgam, it is readily reduced to a compound of the formula $C_6H_{14}O_6$, which crystallizes in fine plates and melts at 165°; in fact, possesses all the properties of the hexhydric alcohol, $C_6H_8(OH)_6$, mannite.

THE *Board of Trade Journal* publishes a memorandum which has been drawn up in the Fisheries Department of the Board of Trade relative to the Sea Fisheries Regulation Act, 1888. The memorandum says that the Board of Trade may on the application of any county or borough Council create a sea fisheries district, and define its limits, and provide for the constitution of the local committee, which body is to have power to make by-laws regulating sea-fishing and oyster-fishing, to impose fines and forfeitures for the breach of such by-laws, and to appoint fishery officers for enforcing the observance of the by-laws within the district. Fishery officers will be empowered to search any vessel or vehicle used in fishing or in conveying fish.

THE *Zoological Record* for 1887 has just been issued. The editor is Mr. Frank E. Beddard. He explains that the only alteration of importance in the present volume is that authors' names have been printed throughout in capitals. It will be found that this renders clearer the references in the systematic part of the several records.

WE have received the first number of *Art and Literature*, published by Messrs. Maclure, Macdonald, and Co., Glasgow. It is well printed and illustrated, and the only fault we have to find with the new venture is that the word "Science" is not included in the title.

THE fourth part of Cassell's excellent "New Popular Educator" has been issued. This part, which contains contributions to the study of geography, physical geography, physiology, astronomy, and other subjects, is accompanied by a map of England, and is well illustrated.

A VALUABLE bibliography of Indian geology has been compiled by Mr. R. D. Oldham, Deputy Superintendent of the Geological Survey of India. It contains a list, as nearly complete as possible, of books and papers relating to the geology of British India and adjoining countries, published previous to the end of the year 1887.

ARNOLD GUYOT'S "Earth and Man" has been recently published for the first time in French. The French edition is not a rendering from the English; it is the original from which the English edition was translated.

AT the monthly meeting of the Royal Institution of Great Britain, on February 4, the special thanks of the members were returned to Mr. William Anderson for his present of a portrait of Prof. Mendelejeff.

IT is reported from North Central Norway and Sweden that wolves are very numerous this winter. They have reappeared in districts where they have been unknown for many years.

THE beaver is getting very scarce in Sweden, but recently a colony has been discovered near the mountain Middagsfjeldet, in the province of Jemtland, in the heart of Sweden.

IT is reported from the districts around the Christiania Fjord that sparrows have almost completely disappeared from those parts this winter.

SOME French students in Paris are distinguishing themselves by wearing a peculiar cap, the *bérêt* of the Pays Basque, to which is joined a small badge, varying in colour according to the Faculty or School (Scientific, Literary, Law, Medical, &c.) the possessor belongs to. The students of Montpellier have been better inspired in reviving the cap worn in the old school at the time of Rabelais.

WE have received the Calendar for the current academical year of the Imperial University of Japan. The record of events for the past year notes that in June the Tokio Observatory was founded. It is formed by the amalgamation of the University Observatory, the Astronomical Section of the Home Department, and the Astronomical Observatory of the Imperial Navy; the whole being placed under the control of the University, which is now intrusted with the duty of publishing the Astronomical Almanac. By a new arrangement, also, the income of the University from tuition fees and other minor sources is for the present to be accumulated year by year with the object of forming a capital fund for future use. The same is to be done with the fees of other educational institutions under the Education Department. At the time when the Calendar was issued, 276 students were attached to the Faculty of Law, 246 to that of Medicine, 91 to Engineering, 45 to Literature, and 36 to Science. The total number of graduates was 954.

THE additions to the Zoological Society's Gardens during the past week include a Golden Eagle (*Aquila chrysaëtus*), European, presented by Mr. Thos. Barclay; an Alligator (*Alligator mississippiensis*) from Florida, presented by Mrs. G. Peacock; two Himalayan Monauls (*Lophophorus impeyanus*) from the Himalayan Mountains, deposited; an Aard Wolf (*Proteles cristatus* juv. ♂) from South Africa, purchased.

OUR ASTRONOMICAL COLUMN.

THE COLOURS OF VARIABLE STARS.—In his new Catalogue of Variable Stars, noticed in NATURE, vol. xxxviii. p. 554, Mr. S. C. Chandler gave to each star a number indicative of the depth of the red tinge in its light, and in a more recent paper he has explained how these numbers were obtained. Two independent methods were followed: the first, the one proposed by Klein, and adopted by Schmidt and Dunér, in which the stars were arranged according to a scale on which 0 corresponded to pure white light, 1 to the slightest perceptible admixture of yellow with the white, and so on with 4 for full orange, and 10 for the purest red light known to us amongst the stars, and as seen in such examples as R Leporis. In spite of the vagueness of the definition of this scale, Mr. Chandler found by experience that the process of thus referring the impressions of colour to these imaginary standards could be effected with greater precision than he had supposed, or would naturally be inferred by an observer previous to trial. His second method seems, however, to promise more precision, and is both simple and ingenious. It consisted in estimating the relative change in brightness effected in two stars by the interposition first of a blue, and then of a red, shade-glass. Thus, supposing a red and white star appeared of the same brightness when viewed without any shade-glass, the white star would seem decidedly the brighter when the blue glass was used, but the fainter when the red glass was interposed, and these differences could be very precisely estimated by Argelander's method, and thus afford definite measures of the differences in colour of the two stars; of course, on an arbitrary scale depending upon the shade-glasses employed. In all, 665 estimates upon 108 telescopic variables were made by the first method, the "decimal scale" method; and 287 "relative diminution estimates," as Mr. Chandler terms his second method, were made upon 77 of the same stars. The mean of the two methods, equal weight being given to each separate observation, was given as the value of the redness of the star in the Catalogue.

Two somewhat important results appear from these observations: first, that the observations are evidently not affected by any serious systematic error depending on the magnitude, for on the average the same colour is given both at maximum and minimum, the recorded differences being small in amount and of varying sign. It would appear, therefore, that the change in the magnitude of a variable does not usually involve a change in its colour. This is an important point, as the more general opinion hitherto has been that such a change does generally take place. The second result is the intimate connection that exists in a variable between length of period and depth of colour. The Algol type stars are strikingly white; the very short-period stars are colourless, or nearly so; and those of longer period show a deeper red the greater the duration of their periods.

NEW MINOR PLANETS.—A new minor planet, No. 283, was discovered by M. Charlois, of the Nice Observatory, on January 28. This is M. Charlois's fourth discovery.

COMET 1888 *e* (BARNARD, SEPTEMBER 2).—The following ephemeris for Berlin midnight is in continuation of that given in NATURE for 1888 December 27 (p. 211):—

1889.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
Feb. 9	23 42 28	4 28·0 S.	0·2598	0·4096	2·6
11	23 41 51	4 18·5			
13	23 41 18	4 8·9	0·2608	0·4196	2·5
15	23 40 47	3 59·3			
17	23 40 19	3 49·7	0·2621	0·4284	2·4
19	23 39 54	3 40·1			
21	23 39 31	3 30·5	0·2637	0·4362	2·3
23	23 39 11	3 20·9			
25	23 38 52	3 11·3 S.	0·2656	0·4429	2·2

The brightness at discovery is taken as unity.

HAYNALD OBSERVATORY (HUNGARY).—The results of the observations of solar prominences at this Observatory during the year 1886 have just been published (Kalocsán, 1888). The observations were made with a telescope of 190 millimetres aperture, and a spectroscope consisting of six flint glass prisms. The chief object was to determine the dimensions of metallic prominences, and their relations to sun-spots. Tables showing all the details of the observations, and the daily and monthly means are given. Prominences less than 20″ in height have been neglected. There are also three plates—the first showing the forms of some of the largest prominences observed, and the second indicating the state of the chromosphere for each day of observation between September 1 and October 31. The third plate, showing the relation of the heights of the prominences to latitude, is particularly interesting. The highest prominences in 1886 occurred in latitudes 18° N. and 37° S., whilst there were secondary maxima in 45° N. and 8° S., and tertiary maxima in 80° N. and 75° S. The secondary maxima do not fall far below the principal ones, but the tertiary maxima are not nearly so well marked.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *FEBRUARY* 10–16.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 10

Sun rises, 7h. 24m.; souths, 12h. 14m. 28·9s.; sets, 17h. 4m.: right asc. on meridian, 21h. 37·5m.; decl. 14° 11′ S. Sidereal Time at Sunset, 2h. 28m.

Moon (Full on February 15, 22h.) rises, 12h. 7m.; souths, 20h. 12m.; sets, 4h. 23m.*: right asc. on meridian, 5h. 36·5m.; decl. 21° 1′ N.

Planet.	Rises. h. m.		Souths. h. m.		Sets. h. m.		Right asc. and declination on meridian. h. m.		° ′
Mercury..	7 23	...	12 45	...	18 7	...	22 8·0	...	8 8 S.
Venus ...	8 41	...	15 6	...	21 31	...	0 29·9	...	4 14 N.
Mars ...	8 28	...	14 19	...	20 10	...	23 42·0	...	2 39 S.
Jupiter ...	4 46	...	8 41	...	12 36	...	18 3·5	...	23 6 S.
Saturn ...	16 18	...	23 52	...	7 26*	...	9 17·1	...	17 1 N.
Uranus ...	22 37*	...	4 0	...	9 23	...	13 21·9	...	7 56 S.
Neptune..	10 44	..	18 27	...	2 10*	...	3 50·8	...	18 25 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb.		h.		
15	...	1	...	Mercury in inferior conjunction with the Sun.
15	...	1	...	Saturn in conjunction with and 1° 5′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.		Decl. ° ′			h. m.	
U Cephei	0 52·5	...	81 17 N.	...	Feb. 12,	19 30	*m*
Algol	3 1·0	...	40 32 N.	...	,, 11,	0 59	*m*
					,, 16,	18 37	*m*
λ Tauri...	3 54·6	...	12 11 N.	...	,, 13,	19 39	*m*
ζ Geminorum ...	6 57·5	...	20 44 N.	...	,, 12,	6 0	*M*
R Canis Majoris ...	7 14·5	...	16 11 N.	...	,, 14,	20 7	*m*
			and at intervals of			27 16	
R Virginis	12 32·9	...	7 36 N.	...	Feb. 11,		*M*
U Coronæ	15 13·7	...	32 3 N.	...	,, 14,	20 34	*m*
Y Cygni	20 47·6	...	34 14 N.	...	,, 11,	5 40	*m*
			and at intervals of			36 0	
δ Cephei	22 25·0	...	57 51 N.	...	Feb. 12,	0 0	*m*
R Lacertæ	22 38·3	...	41 47 N.	...	,, 15,		*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.		Decl.		
From Camelopardalis ...	110°	...	62° N.	...	Slow.
,, Monoceros	120	...	5 S.	...	Slow.
Near β Ophiuchi	263	...	2 N.	...	Very swift.

GROWTH OF OUR KNOWLEDGE OF THE NEBULÆ.

OUR present knowledge of those celestial bodies which we term nebulæ may be said to date from a paper by Sir William Herschel on nebulous stars, published in 1791 (Phil. Trans., vol. lxxxi. p. 71). It is perfectly true that we have not here the first recorded observations of nebulæ: several observers before Sir William Herschel, and Sir William Herschel himself, had previously referred to them. All observers previous to Sir William Herschel, among whom we may include Kepler, Tycho Brahé, Halley, and others, were of opinion that the nebulæ were composed of something differing entirely in its essence from stars. There was no question whatever of their being simply clusters of stars considerably removed. Tycho Brahé, in the record of his observations of the new star observed by him in Cassiopeia, suggested that it was in some way generated from an ethereal substance, and to him the Milky Way was composed of the same material. This ethereal substance was liable to dissipation by light and heat, and in this way he accounted for the ultimate disappearance of the star. Kepler shared this opinion, and it may be stated that it was generally accepted at the time that Sir William Herschel began his observations of nebulæ about the year 1780. His first important paper, however, did not deal with these objects: it had reference to the motion of the sun in space (Phil. Trans., vol. lxxiii., published in 1783). In this memoir he points out the universal sway of gravitation in the celestial spaces; and the infinite possibilities opened out by such an all-prevailing and pervading cause seem, although he does not state it in terms, to have led him to the conclusion that such ideas as Brahé's and Kepler's were invalid. His first real survey of the nebulæ appears in his paper of 1784 (Phil. Trans., vol. lxxiii.) He began by observing those bodies which had already been recorded in the *Connaissance des Temps* for 1783, and then those further afield; and it is not a little remarkable that in this first paper he describes almost every distinct form of nebulæ which has been observed from that day to the years about 1846, when Lord Rosse brought a still more powerful instrument than Herschel's largest to bear upon these objects. He noticed that in certain parts of the heavens there was a marked absence of stars, and that this was so invariably followed by the appearance of nebulæ on the confines of the empty region that he records in his memoir that after passing over one of them he was in the habit of giving the word to his assistant to "prepare for nebulæ." This strengthened his view as to the power of gravitation, and as to nebulæ being masses of stars produced by it.

In another paper published in the next year (Phil. Trans., vol. lxxv.) he shows evidently that to him the nebulæ of all orders which he had discovered were simple agglomerations of stars, and he refers to the action of gravity in bringing about such condensations. In the next year (Phil. Trans., vol. lxxvi.) he published the first catalogue of a thousand nebulæ, and gives his first classification, one based upon brightness (p. 466). In 1789, that is three years later (Phil. Trans., vol. lxxix.), he published his second catalogue, and it is clear from the text that he still considered nebulæ to be all distinct star clusters. It required another interval of three years before the possibility of their nature being in any way distinct was brought fairly before his mind. In 1791 (Phil. Trans., vol. lxxxi.) he published his remarkable paper on "Nebulous Stars properly so-called." In this paper it will be seen how convincing was the line of argument which Herschel followed to bring him ultimately to the conclusion that in the bodies which he observed there was either a central body which is not a star, or a star involved in a shining fluid of a nature totally unknown to us (p. 83).

This conclusion seems to have made a profound impression upon Herschel's mind, and we had to wait for ten years before he returned to the subject. He did so in 1801 (Phil. Trans., vol. ci.), in a paper detailing "Astronomical Observations relating to the Construction of the Heavens, arranged for the purpose of a critical examination, the result of which appears to throw some new light upon the organization of the celestial bodies." In this paper he classifies all the different kinds of nebulæ which were then known to him, and specimens of which, as has been before stated, he really seems to have glimpsed in his paper of 1784. He points out that, in the classification which he proceeds to give, the bodies under consideration are treated in such a manner that each shall assist us to understand the nature and construction of the others; and he endeavours to attain this end by assorting them into as many classes as are required to produce the most gradual affinity between the individuals contained in any one class and those contained in that which precedes and that which follows it (p. 271). He remarks: "This consideration will be a sufficient apology for the great number of assortments into which I have thrown the bodies under consideration."

His classification may be stated as follows:—

1. *Extensive diffused nebulosity.*—Under this title he includes faint nebulosities stretching and branching over various portions of the sky, which he was the first to discover by means of the enormously increased optical power which he brought to bear. He states that "they can only be seen when the air is perfectly clear, and when the observer has been in the dark long enough for the eye to recover from having been in the light" (p. 274). He gives fifty-two of these diffused nebulosities, which he had observed in the nineteen years from 1783 to 1802. He remarks that "extensive diffused nebulosity is very great indeed; for the amount of it, as given in the tables, is 151·7 square degrees; but this, it must be remembered, gives us by no means the real limits of it;" and he finally adds, "it will be evident that the abundance of nebulous matter diffused through such an expansion of the heavens must exceed all imagination."

2. *Nebulosities joined to nebulæ.*—He refers to fourteen

objects in which real nebulæ are distinctly associated with the above diffused nebulosity.

3. *Detached nebulosities.*—He next mentions six cases in which, instead of the extensive diffusion referred to under the first head, the nebulosity is found detached.

4. *Milky nebulæ.*—He here remarks that when detached nebulosities are small we are used to call them nebulæ, and he shows that the nebulosities and the nebulæ, whatever may be their appearance, as well as those expressly called by him "milky," partake of the same general nature.

5. *Milky nebulæ with condensations.*—He refers to the brightest portions of the nebula in Orion as an indication of what he means by condensation; then to another in which the greatest brightness lies towards the middle; and then he adds:—"By attending to the circumstances of the size and figures of this nebula we find that we can account for its greater brightness towards the middle in the most simple manner by supposing the nebulous matter of which it is composed to fill an irregular kind of solid space, and that it is either a little deeper in the brightest place, or that the nebulosity is perhaps a little more compressed. It is not necessary for us to determine at present to which of these causes the increase of brightness may be owing; at all events it cannot be probable that the nebulous matter should have different powers of shining, such as would be required independent of depth or compression" (p. 282).

6. *Nebulæ which are brighter in more than one place.*—He associates the general swelling of the nebulous matter about the places which appear like nuclei with the unequally bright places in the diffused nebulosities, and further on he refers to universal gravitation "as a cause of every condensation, agglomeration, compression, and concentration of nebulous matter."

7. *Double nebulæ with joining nebulosity.*—He points out that "in fifteen objects two nuclei or centres of attraction have been observed, and that if the active principle of condensation carries on its operation a diffusion of their at present united nebulosities must in the end be the consequence" (p. 285).

8. *Double nebulæ not more than 2′ from each other.*—He points out that there are twenty-three of this class.

9. *Double nebulæ at a greater distance than 2′ from each other.*—Of these he gives a hundred examples, pointing out that "there are not more than five or six which differ so much in brightness from one another that we can suppose them to be at any very considerably different distance from us" (p. 288), and he further adds that "equal brightness or faintness runs through them all in general."

10. *Treble, quadruple, and sextuple nebulæ.*—He refers to twenty treble, five quadruple, and one sextuple object of each kind.

11. *Very narrow long nebulæ.*

12. *Extended nebulæ.*

13. *Irregular nebulæ.*

14. *Nebulæ that are of an irregular round figure.*

15. *Round nebulæ.*

16. *Nebulæ that are remarkable for some peculiarity of figure or brightness.*—He ascribes this irregularity to the as yet imperfect concentration of the nebulous mass in which the preponderating matter is not in the centre (p. 300).

17. *Nebulæ that are gradually a little brighter in the middle.*

18. *Nebulæ which are gradually brighter in the middle.*

19. *Nebulæ that are gradually much brighter in the middle.*

20. *Nebulæ that are suddenly much brighter in the middle.*

21. *Round nebulæ increasing gradually in brightness up to a nucleus in the middle.*

22. *Nebulæ that have a nucleus.*

23. *Round nebulæ that show a progression of condensation.*

24. *Round nebulæ that are of an almost uniform light.*

25. *Nebulæ that have a cometic appearance.*

26. *Extended nebulæ that show the progress of condensation.*

27. *Nebulæ that draw progressively towards the period of final condensation.*

28. *Planetary nebulæ.*

In addition, Sir William Herschel in his various papers gives drawings illustrating the classification which has been above referred to (Phil. Trans., vol. ci. Plates 4 and 5, and vol. civ. Plate 11). A more elaborate set of plates illustrating the various gradations of the different forms will be found accompanying Sir John Herschel's catalogue (Phil. Trans., vol. cxxiii., 1833, Plates 9, 10, 11, 12, and 13). In these illustrations will be found some forms of great interest not referred to by the elder Herschel. Long parallel nebulæ, for instance, with a dark streak separating them, and elliptic and ring nebulæ. With these exceptions, all the illustrations readily fall into Sir William Herschel's classification.

In the valuable paper of Sir John Herschel, to which reference has been made, there is evidence to show that he gives up the idea of nebulous matter distinct from stars advocated by his father. He says: "If the nebula be anything more than a cluster of discrete stars, as we have every reason to believe, at least in the generality of cases, no pressure can be propagated through it" (Phil. Trans., 1833, vol. cxxiii. p. 502). Coming down to the work of Lord Rosse, we find that as early as 1846 he had convinced himself almost completely that no such thing as so-called nebulous fluid existed. In a letter to Nicol ("Architecture of the Heavens," p. 143) under date March 19, referring to the nebula of Orion, he states that he could "plainly see that all about the trapezium is a mass of stars, the rest of the nebula also abounding with stars and exhibiting the characteristics of resolvability strongly marked."

The magnificent observations of the nebulæ made by Lord Rosse will be found in the Philosophical Transactions (R. S.) for the years 1850 and 1861, the latter giving an account of the work done by the 6-foot, and in the Scientific Transactions of the Royal Dublin Society for 1880. In the volume for 1861, p. 702, Lord Rosse seems rather inclined to withdraw the very definite letter which has been previously quoted, and states that, "When the letter R, meaning that the nebula is resolvable, has been used, he does not attach much importance to the expression of opinion it conveys, because the question of resolvability can only be successfully investigated when the air is steady and the speculum is in fine order."

This state of uncertainty, however, did not last long, for in 1864 Dr. Huggins and Dr. Miller demonstrated that the spectrum of several planetary and other nebulæ which they examined, instead of giving spectra like those of the stars, gave one of bright lines, one of the lines being due, as they asserted at the time, to hydrogen; the other, as it lay very near a line of nitrogen, was supposed by them to represent "a form of matter more elementary than nitrogen, and which our analysis has not yet enabled us to detect" (Phil. Trans., 1864, p. 444). Then they wrote:—"It is obvious that the nebulæ (that they had examined) can no longer be regarded as agglomerations of suns after the order to which our sun and the fixed stars belong. We have in these bodies to do no longer with a special modification only of our own type of suns, but find ourselves in the presence of bodies possessing a distinct and separate plan of structure."

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE forty-second annual general meeting of the Institution of Mechanical Engineers took place at 25 Great George Street, Westminster, by permission of the Council of the Institution of Civil Engineers, on January 30 and 31, and February 1, the President, Mr. Charles Cochrane, in the chair.

The three papers down for reading and discussion were: supplementary paper on the use of petroleum refuse as fuel in locomotive engines, by Mr. Thomas Urquhart, Locomotive Superintendent, Grazi and Tsaritsin Railway, South-East Russia; on compound locomotives, by Mr. R. Herbert Lapage, of London; on the latest development of roller flour milling, by Mr. Henry Simon, of Manchester.

The author of the first paper states that his object is to bring before the Institution the more recent results of his experience in the use of petroleum refuse as a locomotive fuel, now being used on an unprecedented scale on the Grazi and Tsaritsin Railway. Since the publication of the original paper in 1884, nothing new in principle has been discovered, and the same appliances have been used, having undergone very slight modifications, dictated by experience and constant observation. The whole of the 143 locomotives under the author's superintendence, as well as various stationary boilers of various types, have been fired with petroleum refuse, to the complete exclusion of all solid fuel, as well as in all the heating furnaces at the Company's Central Works at Boriooglebsk. The petroleum refuse is burnt in the form of a spray, being blown into the furnace against a brick structure, serving the double purpose of a reservoir for the heat, and against which the spray is broken up. Many experiments were made with a variety of forms of

brickwork inside the furnace or fire-box, which are duly described by the author. The spray-injector is of course illustrated. On this depends the efficient working of the furnace. The oil is blown into the furnace by means of a steam jet. Experiments have been made on the use of compressed air instead of steam; and, from what could be observed during a two months' trial, the complication and cost of the extra gear would not be recouped by a sufficient economy in fuel consumption. The effect of petroleum fuel on the boilers, after five years' experience, appears to be less destructive than when firing with anthracite, which is particularly destructive to the fire-boxes and tube-ends. The author states that the petroleum flame produces in reality no more detrimental effect on the fire-box and tubes than a wood flame, owing to the protection afforded to the more important parts by the fire-brick lining; moreover, petroleum refuse contains no sulphur, which is so prevalent in all coals, and so injurious to the metal of the fire-box and tubes. The evaporative value of petroleum refuse appears to be very high. With an effective steam pressure of 125 pounds per square inch, the highest evaporative duty of the fuel in the author's locomotives has been 14 pounds of water per pound of fuel, in comparison with the theoretical evaporative value of 17·1 pounds. The actual efficiency of the fuel is therefore nearly 82 per cent., the tables giving an evaporation, under the same conditions, for good English coal, of 12·16 pounds of water.

Mr. Urquhart's paper will be read with great interest by those following his example in using various oils and tar as a fuel for locomotives and stationary boilers. Provided a cheap source of fuel in the form of petroleum refuse or oil can be relied upon, the many reasons for raising steam in this way are obvious, to say nothing of the possibility of the machinery being kept free from all the dirt necessitated by the use of coal on a locomotive.

The object of the paper on compound locomotives, by Mr. R. Herbert Lapage, is to furnish an account of some recent practice in designing and working two-cylinder compound locomotives. The advantages of compounding—that is, expanding the steam in more than one cylinder—is due to the difference of temperature between the boiler steam and the exhaust being distributed over two cylinders, with the important result that there is not so much difference as in the ordinary locomotive between the temperature at the beginning and that at the end of the stroke in each cylinder; consequently there is less initial condensation and less re-evaporation of condensed steam, and a more uniform pressure on the pistons throughout the stroke; and owing to the more constant and even pressure on the pistons, the turning moments about the driving axle are more uniform, giving less sudden strains to the machinery generally, thereby increasing the life of the machine. The fact that so little attention has until recently been paid to the compounding of locomotives appears to be owing to there having hitherto been considerable complication of parts, in connection both with obtaining a simple device for starting the engine and of equalizing the power developed in the high and low pressure cylinders. These objections have now been thoroughly overcome in what is known as the Worsdell and Von Borries system, in which the two-cylinder compound locomotive has been brought to a high pitch of efficiency. The author of the paper describes a six-wheel-coupled goods engine which was sent out in 1886 to the Entre-Rios Government Railway, having been built by Messrs. Dubs and Co., Glasgow. This engine was built on the compound principle, after investigating the excellent results obtained by the Worsdell and Von Borries system. The dimensions of the cylinders are—high pressure, 16 inches diameter; low pressure, 23 inches; both cylinders having a stroke of 24 inches; the working pressure being 175 pounds per square inch; diameter of driving wheels 3 feet 9 inches. The total weight in working order is 37 tons, probably having about 30 tons useful weight for adhesion; the cut-off of the valves in ordinary running being in the high pressure cylinder 40 per cent., and in the low pressure 50 per cent. Various trial trips were made with this compound on the Caledonian Railway, the work done without doubt showing the power and efficiency of the engine. In the locality where this engine is working coal costs at least £2 a ton; presuming an ordinary engine runs 30,000 miles a year at 25 pounds of coal per mile, it will have burnt 335 tons, which, at £2 per ton, costs £670. The compound, effecting a saving of about 20 per cent., will accordingly save £134 in a year. It is found that a compound locomotive of less weight can haul as heavy a train at the same speed as an ordinary engine, provided the adhesion is sufficient, with the economy of from 14½ to 20 per cent., and as the cost of the compound is no greater than such an engine, the 20 per cent. or £134 per year saved is a net saving to the engine. Compound express locomotives working the heaviest service, which run about 3000 miles per month, are found to do some 15 per cent. more mileage between shop repairs than the ordinary engines of the same size and class.

The fact that two important papers should have been read before the Institutions of Civil and Mechanical Engineers respectively points to the conclusion that the compound locomotive has out-grown the experimental stage. Mr. Lapage says nothing in his paper about the "Webb" system, and probably this is a sign of the "survival of the fittest." The Worsdell engine requires little, if any, alteration in the primary parts of an ordinary engine. The number of working parts is not increased, and the strains set up in the engine are more uniform and less intense than in the ordinary engine, less steam is used, and therefore the boiler is not worked so hard—in fact, everything in connection with the working of these engines points to less general wear and tear of parts, and therefore longer life to the machine.

The last paper on the list, on the latest development of roller flour milling, by Mr. Henry Simon, deals with the extraordinary revolution which, during the last ten years, has been in progress in the manufacture of flour by the substitution of the roller system for the ancient method of grinding by stones; and the object of the present paper is to give further information about the subsequent development of roller flour-milling as carried out by the author. The completeness of the revolution that has taken place is exemplified by the fact that practically, in less than ten years, the machinery and methods of corn milling have been radically and entirely altered at the cost of an immense amount of capital. The millstone, dating from prehistoric times, has been almost entirely discarded, and the miller has been constrained to unlearn the old methods, and take up one entirely new, based upon very different principles. The first complete roller-mill, without the use of stones, in England, was built by the author in 1878 for Mr. Arthur McDougall, of Manchester, and in Ireland for Messrs. E. Shackleton and Sons in 1879; the first automatic roller flour-mill in England in 1881 for Messrs. F. A. Frost and Sons, Chester. The total number of complete mills, or important reconstructions of old mills, executed by the author since 1878 amounts to considerably more than 200, varying in cost for machinery, exclusive of motive power, buildings, &c., from £1000 to £40,000 for each mill.

As it is nearly impossible to give our readers an adequate description of this class of machinery without diagrams, we do not attempt the task, but recommend Mr. Simon's very interesting paper to the careful perusal of all practical millers.

NOTE ON THE ACTION OF ACIDS UPON ULTRAMARINE.

At the Birmingham meeting of the British Association in September 1886, I read a short paper "On the Fading of Water-colours." This was published in the *Chemical News*, vol. liv.

Observation and experiments had led me to the conclusion that water-colour drawings in which ultramarine was mixed with reds for the representation of purple and gray tints such as are seen when viewing distant mountains, the shadows of clouds, and other luminous shadows, the colours are liable to suffer from the action of acids such as might be found in the drawing-paper, or in the damp atmosphere of towns where much coal is burnt. The general opinion of artists is one which I believe does not coincide with this view. The same series of experiments had shown that under ordinary circumstances indigo was a colour of great stability compared with many other pigments, and this again was in conflict with the experience of artists. It is not impossible to explain how we have arrived at such different views, and though it would be inconvenient to enter into an explanation in full, it may be considered as within the scope of this paper to record the fact that the colours were washed upon the best drawing-paper, dried in a subdued light, and not exposed to the conjoint action of damp and sunlight.

The matter in hand is the question of the stability of ultramarine in presence of acids. In an old work by M. Constant de Massoul, translated into English in 1812, entitled "A Treatise on the Art of Painting and the Composition of Colours," a short

account of ultramarine is given as follows:—"The basis of this colour is *Lapis Lazuli.* This, added to the long and tedious operation of extracting the Blue, makes this colour very dear. In order to prove the goodness of *Lapis Lazuli*, make it red hot upon a plate of iron; and then throw it immediately into strong white Vinegar. If it loses its colour, it is of an inferior quality. You may likewise form a judgement by its weight, the real Ultramarine being much heavier than the false."

It is stated that the stone comes from Asia, where it is found on the frontiers of Tartary, China, and also from America. Having drawn my conclusions as to the behaviour of ultramarine with acids, from the preparations sold for this pigment, it seemed desirable that the behaviour of the mineral should be studied by itself, and likewise that of the artificial preparation. This latter, I am aware, is variable; some of it is more easily decomposed by acids than other samples, the difference being occasioned by the greater amount of silica in the latter.

I applied to Mr. Gregory, of 88 Charlotte Street, Fitzroy Square, for as many different specimens of *lapis lazuli* and minerals resembling it as were at his disposal. They consisted of a specimen from Chili, two from Persia of magnificent colour, three from Siberia, Trans-Baikal, and a specimen of a blue mineral often mistaken for *lapis lazuli* called *glaucolite.* This also occurs in the Trans-Baikal district.

These specimens were chipped, where fragments of a fine blue colour were to be seen, and the pieces were ground in an agate mortar to an impalpable powder.

A specimen of each was placed in the hollow of a white earthenware colour palette, and moistened with sulphurous acid. All the specimens of *lapis lazuli* were attacked, and in nearly every case completely decolorized. Where the blue colour was not quite destroyed, the powder was examined with a powerful lens, and it was seen that blue particles remained which had not been sufficiently finely powdered. Several minute lumps of the colour were noticed to be etched by the acid, showing white spots here and there. Hence the fineness of the powder has much influence on the facility with which the mineral is attacked. This is usual with all mineral substances.

It was next considered of interest to ascertain whether *lapis lazuli* will stand the test applied to it by Constant de Massoul, and therefore some of the powdered mineral was made red-hot and thrown into dilute acetic acid; after waiting for five minutes, the blue colour was not appreciably diminished, and it is to be presumed that its nature would thus be satisfactorily demonstrated. Under these circumstances, however, the colour is in considerable quantity, and though some of it may be acted on, yet it is not all destroyed, neither is the tint altered. But in the previous experiments, the powder was much finer and in a thin layer, and though there was a slight action immediately, yet it was about an hour before the colour was completely destroyed. The specimens did not all behave exactly in the same way: some were destroyed more readily than others, especially those from Chili and from Persia.

It does not appear, therefore, that my statement concerning the use of ultramarine as a pigment upon drawing-paper requires modification. A wash of bluish-gray, obtained by mixing light red with ultramarine, was handed round at the Birmingham meeting, one-half of which was shown to be of a foxy red tint after treatment with sulphurous acid. This is, of course, beside the question as to whether ultramarine is largely used for gray tints in the form of water-colour by artists.

Touching the mineral glaucolite, its composition, according to the analysis of G. von Rath, quoted in Dana's "Mineralogy," is the following: silica 47·49, alumina 27·57, ferric oxide 1·54, magnesia 0·47, lime 17·16, soda 4·71, potash 0·58, and water 0·48 per cent. It is quite unacted upon by the acids which decompose *lapis lazuli.* It is highly improbable that it has ever been used as a pigment, because in the form of powder its colour is poor.

W. N. HARTLEY.

LONDON ANCIENT AND MODERN, FROM A SANITARY POINT OF VIEW.[1]

DR. POORE began by reminding his hearers that the mere age of London was one of the reasons why it became unwholesome. Roman London was buried deeply amongst rubbish of all kinds, much of which was putrescible, and, therefore, a source of danger in the soil.

[1] Abstract of a Lecture delivered by Dr. G. V. Poore at the Sanitary Institute on Thursday, January 24.

Ancient London was well placed and magnificently supplied with water, for in addition to the Thames there were many streams, such as Westbourne, Tybourne, the Fleet River, Walbrook, and Langbourne, which originally were sources of pure water. All these brooks, however, had become disgracefully fouled, and for very shame had been covered over. One great drawback to the site of London was the proximity of marshy land on every side except the north-west, and formerly from this cause malarial fever and dysentery were great causes of the high death-rate.

In mediæval London, and even down to the eighteenth century, the houses were not so closely packed as they are now. Reference to Aggas's map (time of Elizabeth) would show that there was a great deal of garden ground within the City, and on comparing this map with Newcourt's map (Charles II.) it was evident that just before the Plague and the Fire the crowding of houses had become very much greater than it was in the time of the Tudor monarchs, who discouraged building near or in London.

Parker's map (1720) would also show that after the Fire the houses were not so closely packed as in the days of the Stuarts, for in this map a surprising amount of garden ground is visible within the walls. At this time also Moorfields was not built upon, and remained as a playground and air space as it had done for centuries previously. That mediæval London was very unhealthy, a perfect fever den, there could be no doubt. The Black Death in 1349, and the Sweating Sickness two centuries later, were times of great mortality which struck the popular mind, but it was not till 1593, when bills of mortality were first introduced, that we began to have any certain knowledge of the amount or the kind of disease prevalent. There was reason to think, however, that in the eighteenth century (after the Fire and the Great Plague) the deaths exceeded the births by about 600,000 in the hundred years.

The fatal diseases were mainly fevers—malarial fever, small-pox, typhus, measles, and (latterly) whooping-cough. The causes of the enormous mortality of mediæval London were due—(1) To the marshy undrained soil, fouled with refuse of every kind. (2) The filthy state of the unpaved city, and a perfectly swinish condition of the houses of the lower orders. (3) The ill-nourished and drunken condition of the masses, among whom a taint of scurvy was very common. (4) The condition of superstition and brutality (as evidenced by the punishments and the pastimes) which made any measures of public health impracticable. (5) The management of epidemics was bad, with a total neglect to separate the sick from the sound; and, finally, the medical faculty were scarcely less ignorant and superstitious than their patients.

Turning to modern London, the lecturer said there had been a great and manifest improvement; but when we looked at the low figure which is called the London death-rate, several things must be taken into consideration, *e.g.* (1) The London of the Registrar-General included large districts such as Lewisham, Wandsworth, Fulham, &c., which, in great part, were scarcely urban in character; and these being occupied largely by well-to-do persons, lowered the average death-rate for the whole city. (2) London being a city in which wealthy people abounded, its death-rate must not, in fairness, be compared to a city packed with undiluted operatives. (3) The mobility of the population was so great that this fact must vitiate our statistics, and it was to be remembered that nothing quickened the departure of an individual from London more than ill-health. (4) The age distribution in London was very abnormal. It was largely recruited by selected adults from the country, and there was a great deficit in the extreme ages among which (the very young and very old) death-rate is always highest. (5) Again, the diminishing birth-rate (that for 1887 was 2·8 below the average of the previous ten years) very greatly diminished the death-rate in a city where 158 children out of every 1000 born die before they are one year old.

It was difficult to believe that Londers were very robust when more than 25 per cent. of them had recourse to the public hospitals in the course of the year.

The cause of the diminished death-rate (which was very considerably reduced after every allowance had been made) was due —(1) To the increase of knowledge, not only among doctors, but amongst the people generally, for we must remember that "self-preservation is the first law of Nature." (2) Vaccination, and the modern plan of treating infectious diseases by the prompt separation of the patients, had done a great deal; the total absence

of small-pox and typhus were mainly due to these causes. (3) The cheapness of food, clothing, and fuel had, of course, diminished the tendency to disease, and the ease with which fresh fruit and vegetables were to be got had abolished the taint of scurvy which was so fatal to our ancestors. (4) The water-supply had been improved, and the intake of the water companies was now removed to a portion of the river less tainted with sewage than that formerly in use. (5) Although the system of sewage disposal was an undoubted evil, and had given us three or four epidemics of cholera, and was the foster-mother of typhoid, still it was probable that so far the balance for good was in its favour, because it had removed a good deal of filth from dwellings.

The outlook in the future was dashed by three considerations:—(1) Our system of sewerage and water-supply had increased overcrowding by enabling us to build houses of any height without inconvenience to the occupant, and without any curtilage whatever, and since all sanitarians recognized that overcrowding was the greatest of all sanitary evils, it was impossible to shut one's eyes to this danger.

(2) There was an expensive and menacing "loose end" to our sanitation in the shape of 150,000,000 gallons of sewage pouring into the Thames every day. The only proper destination of organic refuse was the soil, and it was not possible to see the end of the gigantic blunder we had committed in throwing it into the water.

(3) The rapid increase of population along the Valley of the Thames where sewage disposal is on the same lines as in London, must make us apprehensive for our water-supply, because the various tricks played with sewage in the shape of precipitations, &c., were not probably of a kind to make the effluent a desirable or a wholesome beverage. If the evil effects of free trade are to be counteracted, it will be by returning the refuse of our towns free of cost to the impoverished agriculturist. "If we go on as we are going," said the lecturer, in conclusion, "and if our brethren in the colonies follow our bad example, as they appear to be doing, it will be a Chinaman rather than a visitor from New Zealand who will sit in contemplation on the ruins of London Bridge."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Among the scientific lectures this term, we may note the following:—

Prof. Clifton, Acoustics and Magnetism; Mr. Selby, Theory of Electrical Measurements.

Prof. Odling, Four-carbon Compounds; Mr. Veley, Physical Chemistry; Mr. Vernon-Harcourt, Quantitative Analysis.

On the Biological side, the Linacre Deputy-Professor, Mr. Hatchett Jackson, lectures on the Morphology of the Invertebrata, Mr. P. C. Mitchell on the Morphology of the Cell, and Mr. Barclay Thompson on the Osteology of the Sauropsida. Prof. Burdon-Sanderson's subject is the Nervous System. Prof. Green is giving two courses of lectures on Geology, and Prof. Gilbert lectures on the Rotation of Crops and the Feeding of Animals.

On the Mathematical Lecture List we find that Prof. Sylvester is treating of Surfaces of the Second Order (illustrated by models), Prof. Price of Hydromechanics, and Prof. Pritchard of the Elements of the Planetary Theory.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for December 1888 contains the following:—Note on a new organ, and on the structure of the hypodermis, in *Periplaneta orientalis*, by Edward A. Minchin (plate xxii.). The new organ consists of two pouch-like invaginations of the cuticle lying close on each side of the middle line, between the fifth and sixth terga of the dorsal surface of the abdomen. They are covered by the fifth tergum; when exposed they are seen to open by two slit-shaped openings, which open backwards. They are lined by a continuation of the chitinous cuticle, which forms within the pouches numerous stiff, branched, finely-pointed hairs, below which are numerous glandular epithelial cells. As to their function, it is suggested that they are stink glands.—On certain points in the structure of Urochæta, E.P., and of Dichogaster, nov. gen., with further remarks on the nephridia of earthworms, by Frank E. Beddard (plates xxiii. and xxiv.). The important facts recorded about the anatomical structure of the species of these two genera, and on the nephridia in earthworms, do not admit of being further condensed. *Dichogaster damonis*, nov. gen. et sp., is described from Fiji.—On the development of *Peripatus novæ-zelandiæ*, by Lilian Sheldon (plates xxv. and xxvi.). A further supply of living specimens was obtained in January 1888. Twenty-seven out of forty-nine were females. The uteri of all but nine of these were filled with embryos. The stages of development did not allow of all the gaps left in Miss Sheldon's previous paper being filled up, but this paper is a welcome addition to our knowledge. A useful summary of the author's investigations is appended.—Note on the development of Amphibians, chiefly concerning the central nervous system; with additional observations on the hypophysis, mouth, and the appendages and skeleton of the head, by Dr. Henry Orr, (plates xxvii. to xxix.).—Studies on the comparative anatomy of Sponges, ii. on the anatomy and histology of *Stelospongus flabelliformis*, Carter; with notes on its development, by Arthur Dendy (plates xxx. to xxxiii.). This interesting paper may be regarded as the first-fruits of Mr. Dendy's researches into the anatomy and embryology of recent Australian Sponges, and we hope to be long favoured with such. The embryos, "each as large as a small pea," of *S. flabelliformis*, Carter, were found in abundance. Though varying in diameter from about 3 to almost 5 mm., they exhibited nearly the same stage of development. Doubtless we may expect at some future time the whole story of their evolution. The membrane connecting the fringes of the "choanocytes," which have been so clearly demonstrated by Sollas in the Tetractinellida, and the occurrence of which in *Leuconia aspera* has been described by George Bidder, also occurs in this Sponge, and has been called by Mr. Dendy "Sollas's membrane."—On some points in the natural history of Fungia, by J. J. Lister.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 24.—"On the Influence of Carbonic Anhydride and other Gases on the Development of Micro-organisms." By Percy F. Frankland, Ph.D., B.Sc. (Lond.), F.C.S., F.I.C., Assoc. Roy. Sch. of Mines, Professor of Chemistry in University College, Dundee.

Carbonic anhydride, hydrogen, carbonic oxide, and nitrous oxide, were the gases employed in a series of experiments for observing what action was exerted by them on pure cultivations of Koch's comma Spirillum, Finkler's comma Spirillum, and the *Bacillus pyocyaneus*. It was found that hydrogen had the least, and carbonic anhydride the most, prejudicial influence upon these micro-organisms. There is, therefore, no longer any doubt that in the anaërobic culture of organisms hydrogen is by far the most suitable medium for the expulsion of air, whilst carbonic anhydride is not only ill-suited owing to its markedly deleterious action upon many forms of Bacteria, but in many cases is quite unfit for such a purpose.

With carbonic oxide and nitrous oxide it was found that although the development of the *B. pyocyaneus* was checked, yet on removing the cultivations to an *air*-chamber almost the same number appeared as were developed on the original air-control plates. This was not, however, the case with Koch's comma Spirillum and Finkler's comma Spirillum, only a comparatively small number of the organisms surviving the exposure to these gases. Similar experiments made with nitric oxide, sulphuretted hydrogen, and sulphurous anhydride resulted in the complete destruction of the above organisms.

January 31.—"Auto-Infection in Cardiac Disease." By L. C. Wooldridge, M.D., Assistant-Physician, Guy's Hospital.

The author had previously described the fact that the lymph and chyle produce a poisonous influence when injected into the blood. The symptoms so produced have been described by the author as "fibrinogen intoxication." The chief symptoms of this condition already described are intravascular clotting, delay in clotting of the shed blood, great tendency to hæmorrhages, occasionally marked fever. In the present paper the author shows experimentally the following:—

(1) To affect the blood a certain quantity of the fluid of lymph, or the fibrinogen solution, must reach the blood in a given time or no poisoning is produced. A small quantity of the fluid, injected rapidly, will cause instant death. The same quantity, diluted and injected during three or four minutes, instead of suddenly, has no effect at all. The author regards

this as an explanation of the fact that normally the flow of the lymph from the thoracic duct into the blood produces no poisonous effect.

(2) It has long been known that mere mechanical disturbance to the circulation, unless it be of a most extreme character, will not produce dropsy. The ligature of the femoral vein in the dog produces no dropsy. But if previous to the ligature, some of the lymph fluid or fibrinogen solution be injected into the blood, the most severe œdema of the leg is produced, or this accompanied by hæmorrhage.

(3) In cardiac disease and disturbance of the circulation through the lungs there is no reason to suppose that a sudden increase in the flow of lymph ever takes place. But it is certain that the circulation of the blood in the neighbourhood of the thoracic duct is materially slowed in these conditions. This slowing of the circulation acts in the same way as a more rapid injection of lymph, and hence in cardiac disease the conditions for fibrinogen intoxication—auto-infection from the lymph—prevail.

(4) The dropsy, which is so common a symptom of cardiac disease, is commonly explained as being due to the mechanical disturbance of the circulation. This explanation does not harmonize with experimental observations. The fact that even very slight fibrinogen intoxication produces a pronounced tendency to dropsy renders it extremely probable that the dropsy and other symptoms of cardiac disease depend on fibrinogen intoxication.

Physical Society, January 26.—Prof. Fuller, F.R.S., in the chair.—The thanks of the Society were tendered to Mr. Freeman, for presenting to the library a rare and interesting work, "Réflexions sur la Puissance Motrice du Feu, et sur les Machines propres à développer cette Puissance," par S. Carnot, ancien élève de l'École Polytechnique.—Dr. S. P. Thompson read three notes on polarized light, entitled respectively: "The Structure of Natural Diffraction Gratings of Quartz," "Ahrens's Modification of Delezenne's Polarizer," and "The Use of Two Quarter-Wave Plates in Combination with a Stationary Polarizer." Two microscope slides of iridiscent quartz (prepared by the late Mr. Darker), which have recently come into the possession of the author, exhibit remarkable peculiarities. Both act like diffraction gratings, one as if the rulings were about 12,000, and the other about 26,000, to the inch. On examining the specimens by the microscope, it was found that the parts which exhibited the grating effect showed a spindle-like structure, and by micrometer measurements the dimensions of the spindle-shaped bodies were determined to be from 1/1000 to 1/3000 of an inch in diameter, and 1/100 to 1/300 of an inch long. These were much too large to cause the effects noticed, but on closer examination it was found that the bodies were crossed at right angles by fine markings, the distances between which are in close accordance with those deduced from the spectra produced. As a probable cause of the phenomenon, the author mentioned a recent paper by Prof. Judd, "On the Production of a Lamellar Structure in Quartz by Pressure," and suggested the possibility of making diffraction gratings by such means. Ahrens's modification of Delezenne's polarizer consists of a total-reflection prism combined with glass plates and black glass mirror, arranged so that the polarized beam is parallel to the original one. The combination of plates and mirror is adopted so as to give enough light and still keep the polarization sufficiently good. One or two plates laid over the mirror are found to give the best results The fact that a beam polarized by reflection is not coinciden with the original beam, renders it inconvenient, if not impossible to rotate the polarizer, and to overcome this defect, the author has arranged two quarter-wave plates, one of which may be rotated. The first plate circularly polarizes the plane-polarized beam, and the second (or rotating one) re-plane-polarizes it in any desired plane. Objects were shown on the screen to illustrate the degree of perfection attainable by using the new polarizer in combination with the two quarter-wave plates.—A note on a relation between magnetization and speed in a dynamo machine was read by the same author. In a note presented to the Society in June last, it was shown that $\Sigma\rho \,.\, \Sigma R = 4\pi n CS$; where $\Sigma\rho$, and ΣR are the magnetic and electric resistances respectively, n = speed, and C and S the numbers of armature and field windings. By writing the equation in the form—

$$\frac{4\pi CS}{\Sigma R} = \frac{\Sigma\rho}{n},$$

it is seen that, *when the electric resistance is maintained constant, the magnetic resistance is proportional to speed.*—Prof. Herroun read selections from a paper on the divergence of electromotive forces from thermo-chemical data. The fact that the electromotive forces of voltaic cells do not always coincide with calculated values has not hitherto received a satisfactory explanation, and this paper describes an experimental research bearing on the question. Several suggested explanations are given. In some cells the anticipated chemical change does not occur, and some metals become coated with oxide or subsalts; others are affected by dissolved gases, and the hydration or solution of the salts formed may supplement or diminish the E.M.F. of a cell, as well as the absorption or evolution of sensible heat. The question of absorption and evolution of heat is the one chiefly dealt with. If such actions do take place, the total heat evolved by passing a definite current through the cell must depend on the direction of the current, and by inclosing the cell in a calorimeter the difference should be detected. The total heat developed by a current C in t seconds is—

$$\frac{C^2rt \mp eCt}{J};$$

where r is the resistance of the cell, and e the divergence of the observed from the calculated E.M.F., the − or + sign depending on the direction of the current. In the case of mercury cells, which are usually said to give about half a volt excess E.M.F., the heat was found to be independent of the direction of the current. The heats of formation of mercury salts were then re-investigated, and the results showed that Julius Thomsen's numbers (the ones usually accepted) were greatly in excess o the true values. This accounts for the difference between the observed E.M.F.'s and those calculated from Thomsen's numbers. A copper, silver, nitrate cell was tested in the calorimeter, and the reversible heat effect agreed closely with that deduced from the "thermo-voltaic constant," or divergence of observed from calculated E.M.F. Other experiments on tin, lead, nickel, iron, and calcium cells are described, and the chief conclusions arrived at are: (1) the primary factor in determing the E.M.F. of a voltaic cell is the relative heat of formation of the anhydrous salts of the two metals employed; (2) that this E.M.F. may set up chemical changes of a different direction and character from those predicable from the heat of formation of the dissolved salts; (3) that the E.M.F. set up by (1) may be, and usually is, supplemented by the energy due to the hydration or solution of the solid salts, and may have values which accord with the heat of formation of the dissolved salts. The absorption or evolution of sensible heat depends primarily on the attraction between the salts and water, combined with the heat of solution. Finally, the author states that the E.M.F. of a cell gives a more accurate measurement of chemical affinity than that derived from calorimetric observations.

Chemical Society, January 17.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—A cubical form of bismuthous oxide, by Messrs. M. M. P. Muir and A. Hutchinson. When the puce-coloured precipitate produced by adding an excess of potassium cyanide to a boiling solution of bismuth nitrate in dilute nitric acid is repeatedly treated with boiling concentrated potash solution, a residue is left, consisting of tetrahedral crystals of bismuthous oxide, which have a density of 8·838.—Cupric iodide, and the interaction of iodides with cupric salts, by Mr. D. J. Carnegie. By digesting cuprous iodide with iodine and water in a tightly-closed bottle at 80° for a few minutes, the author has obtained solutions of cupric iodide containing as much as 0·82 gramme per 100 cubic centimetres, but has been unable to obtain cupric iodide in the solid state, either from such solutions or by other means. A well-defined basic periodide, $CuI_2, 2CuO, 4H_2O$, was obtained by digesting copper with barium iodide.—Periodates, part 2, by Mr. C. W. Kimmins. The periodates of lead, iron, copper, nickel, cadmium, and silver were described.—Compounds of arsenious oxide with sulphuric anhydride, by Mr. R. H. Adie. A series of compounds, of the formula $As_2O_3 \,.\, xSO_3$, where x = 1, 2, 4, or 8, can be prepared by the interaction of arsenious oxide and either sulphuric acid or sulphuric anhydride.—A compound of boric acid with sulphuric anhydride, by Mr. R. F. d'Arcy.—Notes of experiments with butter fat, by Messrs. A. W. Blyth and G. H. Robertson. The main result of the experiments is to show that butter fat is composed of about 54·5 per cent. of solid crystalline fats, and about 45·5 per cent. of an oil. The authors consider that butter is mainly made up of compound and not simple triglycerides, and have separated a crystalline glyceride, to which they ascribe the formula $(C_4H_7O_2) \,.\, C_3H_5 \left\{ \begin{array}{l} C_{18}H_{31}O_2 \\ C_{18}H_{34}O_2 \end{array} \right.$

—Gawalowski's method for the volumetric estimation of sulphuric acid, by Mr. B. North.—Note on the 1 : 3 homo- and the isomeric hetero-α-β-dichloronaphthalenes melting at nearly the same temperature, by Prof. H. E. Armstrong, F.R.S., and Mr. W. P. Wynne. In their last note on the isomeric dichloronaphthalenes (Proc. Chem. Soc., 1888, 104), the authors brought forward evidence proving that the dichloronaphthalene melting at 61°·5, and characterized by yielding a sulphochloride melting at 148°, is the meta- or 1 : 3-derivative, and pointed out that the dichloronaphthalene melting at 64°, and characterized by yielding a sulphochloride melting at 118°, is the hetero-α-β-dichloronaphthalene. Erdmann and Kirchhoff recently effected the synthesis of a heteronucleal α-β-dichloronaphthalene from parachlorobenzaldehyde, which had a melting of 61°·5 (*Annalen*, ccxlvii. 366). The description of the compound was, however, insufficient to determine its identity, so the authors have prepared it by Erdmann and Kirchhoff's method, and find that it forms a sulphochloride melting at 117°, which, on hydrolysis, yields the pure dichloronaphthalene melting at 63°·5. The authors' conclusions have also received additional confirmation by the synthesis of the 1 : 3 dichloronaphthalene from the 1 : 3 dichlorobenzaldehyde (Erdmann, *Ber.* xxi. 3446). The remainder of the note is devoted to a reply to Erdmann, who, among other matters which are dealt with in the note, calls attention to the existence of two dichloronaphthalenes melting at about the same temperature as if it were an original observation, whereas the fact was first brought under notice by one of the authors at the Manchester meeting of the British Association (B.A. Report, 1887, 231).—The constitution of β-naphthol-α-sulphonic acid (Bayer's acid), by Prof. H. E. Armstrong. The author points out that Witt (*Ber.* xxi. 3489) altogether misrepresents his views on the constitution of β-naphthol-α-sulphonic acid, and quotes passages from his Report to the Manchester meeting of the British Association (B.A. Report, 1887, 231), showing that in his opinion Bayer's acid is a heteronucleal compound, a view, moreover, which has recently found experimental confirmation, inasmuch as the amido-acid corresponding in constitution with Bayer's β-naphthol-α-sulphonic acid has been shown to yield a heteronucleal α-β-dichloronaphthalene (Armstrong and Wynne, Proc. Chem. Soc., 1888, 104).—The sulphonation of naphthalene-β-sulphonic acid, by the same. The so-called new naphthalenedisulphonic acid, for the preparation of which a patent has been taken out by Ewer and Pick, is identical with that obtained by sulphonating potassium naphthalene-β sulphonate with chlorosulphonic acid (Armstrong and Wynne, Proc. Chem. Soc., 1886, 230).

Royal Microscopical Society, January 9.—Dr. C. T. Hudson, President, in the chair.—Mr. T. F. Smith called attention to his further researches on the structure of *Pleurosigma formosum*. He had found not more than three layers, the first consisting of a grating with square meshes, the second had them of diagonal pattern, and the third was composed of alternate rings and squares. He also described *P. angulatum* as giving appearance of a fine grating showing image in each alternate square.—Mr. Crisp exhibited a form of spirit-lamp sent from America, the reservoir of which was facetted instead of globular, so that it could not be upset and might be used in various positions; also Mawson and Swan's camera arrangement for fixing on the front of an ordinary camera; also the binocular arrangement of Messrs. Bausch and Lomb, which, although described in the Journal in 1884, had not until the present time been seen in this country; also another arrangement for rotating a number of objects so as to bring them in succession under the objective of a microscope.—Mr. A. D. Michael gave an interesting *résumé* of his paper on the internal anatomy of *Uropoda krameri*. He finds that, although the anatomy is essentially of the Gamasid type, yet the external resemblance of *Uropoda* to the *Oribatidæ*, which deceived Hermann, is accompanied by many internal similarities, while many organs differ considerably from those of *Uropoda obscura*, lately described by Winkler. He describes a curious organ which he calls the "vestibule," forming the outer chamber of the female genital system, and which it is suggested may serve to remove the thin egg-shell at the moment of deposition, producing ovo-viviparous reproduction. The female genital organs form a ring with two oviducts, the tracheæ are unbranched; the alimentary canal, excretory system, and male genital system of the œsophageal ganglion are also described.—Dr. F. H. Bowman's paper on the frustule of *Surirella gemma* was read.—Count Abbé F. Castracane's paper on the reproduction and multiplication of Diatoms, was also read.

PARIS.

Academy of Sciences, January 28.—M. Des Cloizeaux, President, in the chair.—Reaction of oxygenated water on chromic acid, by M. Berthelot. The remarkable character of the reactions of oxygenated water has induced the author to undertake further researches on the phenomena which it manifests in the presence of chromic acid. These studies show that the chemical mechanism of the so-called actions in presence is characterized by three fundamental conditions; (1) the unlimited nature of the decomposition under certain circumstances, here determined, without permanent alteration of the chromic acid; (2) the formation of an intermediate compound forming the "pivot" of the decomposition: (3) the exothermic properties of the oxygenated water and of the total transformation.—True and mixed butylic ethers (continued), by M. E. Reboul. The study of these compounds is here completed with the description of di-isobutylic ether, $[(CH_3)_2CH{-}CH_2]_2O$; and secondary isobutylic ether,

$$\begin{array}{l} (CH_3)_2CH{-}CH_2 \\ \qquad\qquad\qquad\qquad \searrow O. \\ \quad CH_3 \searrow \quad \nearrow \\ \qquad\qquad CH \\ CH_3{-}CH_2 \nearrow \end{array}$$

It is further shown that the ethers (7), (8), (9), (10), anticipated by theory, are not produced by the method generally employed. No. 7 (di-secondary ether) has been obtained by Fresnel by a different process.—On M. Marignac's gadolinium, by M. Lecoq de Boisbaudran. The elementary nature of the earth Yα, discovered by M. de Marignac, and since named gadolinium, has been denied by Mr. W. Crookes, who holds that this substance consists of samaria with the greenish blue of yttria, and some of the other yttria bands added to it. M. de Marignac has consequently subjected gadolinium to a fresh analysis with the results here described. M. de Marignac's researches having been interrupted by the state of his health, his papers have been placed in the hands of M. de Boisbaudran, who considers that, although the impurities are not yet entirely eliminated, gadolinium may still be regarded as a new element. He also thinks Mr. Crookes may in this case have exaggerated the difficulties and tedious nature of the preliminary work of fractionation, which, instead of occupying a space of time compared with which "the life of man is all too brief," might perhaps be accomplished in a few weeks. However, he does not deny the extreme difficulty of separating the residuums, which have so far resisted fractionations sufficient to get rid of nearly the whole of Yt and Zα.—On a chromatic circle, an æsthetic recorder and triple decimeter, by M. Charles Henry. These instruments embody a practical application of a theory, the principle of which was communicated to the *Comptes rendus* of January 7, and the chief results of which are here detailed. The chromatic circle has for its object the rational determination of the complements and harmonies of colours; the two other apparatus are intended to facilitate the æsthetic study and improvement of forms.—On the relation between solubility and the point of fusion, by M. A. Etard. The object of the present note, and of the diagram accompanying it, is to show that solubility increases steadily with the temperature, and that it becomes unlimited at or close to the point of fusion of the salt entering into the solution; a given quantity of water may then always dissolve a quantity of any salt. It has been supposed that normally the solubility of salt increases up to a certain point and then decreases. But the facts here verified lead to quite a different conclusion.—New solvents of prussic blue, by M. Ch. Er. Guignet. Experimenting with ordinary prussic blue and with Turnbull blue thoroughly purified, M. Guignet has discovered an easy process for preparing ordinary soluble blue and pure prussic blue soluble in water.—On the quantitative analysis of organic nitrogen by Kjeldahl's method, by M. C. Viollette. The author has subjected this new method to certain tests which yield results somewhat different from those recently communicated to the Academy by M. L'Hôte. He finds that, if applied under the conditions here described, it may prove quite as efficient as the processes of M. Dumas and of sodaic lime, although not more expeditious than either.—On the lime present in the ground in combination with other substances, by M. Paul de Mondesir. Nearly all soils, even the most acid, contain a considerable quantity of lime, not as a carbonate, but in combination with the other elements of the earth. It is here shown that this lime may be eliminated at a low temperature by means of diluted acids.—On the precursors of the Canidæ, by M. Marcellin Boule. The

researches which the author is now prosecuting on the Pliocene faunas of the central plateau of France, have afforded an opportunity of studying remains of the canine group older than those of the Quaternary (Pleistocene) epoch, and tending to throw some light on the origin of existing species. During the Middle and Upper Pliocene there existed a considerable number of species, not only closely related to the present Canidæ, but also anticipating the various living forms of dog, fox, jackal, and wolf. These discoveries tend to overthrow the generally accepted opinion that the present domestic varieties of the dog are all merely artificial modifications of living or Quaternary wolf and jackal types.—Papers were contributed by M. Lerch, on the serial development of certain arithmetical functions; by M. Sauvage, on the regular solutions of a system of linear differential equations; by M. W. Lœwenthal, on the virulence of the cultivated Bacillus of cholera, and on the action of salol on this virulence; and by M. C. Pagès, on the locomotion of quadrupeds.

Astronomical Society, January 9.—M. Flammarion, President, in the chair.—Among the communications were the following:—M. de Meissas sent an observation of M. de Boë's second companion to Polaris.—H.R.H. the Prince of Monaco gave an account of the scientific investigations made on board *l'Hirondelle* during the past four years with a view of studying the general physics of our globe.—M. Moussette described an eye-piece for measuring the size of sun-spots and of lunar objects.—M. Mailhat read a paper on a new mercury-bath for artificial horizons, which had been successfully tried at the Paris Observatory.

BERLIN.

Meteorological Society, January 8.—Dr. Vettin, President, in the chair.—Dr. Sprung spoke on some new apparatus for the registration of rainfall and wind.—Dr. Vettin presented a number of curves representing his measurements of the velocity of the wind, by which he confirmed the results of his earlier observations as to the existence of a maximum velocity at midday in the summer, and at midnight in the winter. The measurements were made with Dr. Vettin's feather manometer. On the discussion which ensued, it was pointed out that the records yielded by this anemometer do not confirm the above results.

Physical Society, January 11.—Prof. von Helmholtz, President, in the chair.—The President opened the first meeting of the current year by a memorial address on Clausius, in which he briefly touched upon his most important works and their significance in connection with the whole range of chemistry and physics.—Prof. Kundt gave a short *résumé* of the researches which he had been carrying on of late years on the behaviour of metals to light. He took as his starting-point Kern's discovery that light which is reflected from magnets undergoes a rotation of the plane of polarization, and fully confirmed this as well as all subsequent observations of the English experimenter. In order to avoid any objections which might be raised against the accuracy of the observed phenomenon, he investigated the rotation produced by extremely thin films of metal, whose production was found, after several preliminary experiments, to be most easily attained by pulverizing the kathode *in vacuo*. The light which was transmitted showed signs of rotation, and as a result of a full experimental investigation all metals were found to exert a dextro-rotatory action on light. This law of the positive rotation of the plane of polarized light could be extended to all simple bodies. The thin metallic films further exhibited a doubly-refractive action which led him to determine the refractive index of the metals, after he had succeeded by means of electrolysis in preparing transparent metallic prisms. The speaker described the methods which he employed in these experiments and exhibited the apparatus which he had used. The result of the experiments is already known. The metals possess a varying refractive power, some exhibiting normal, others abnormal, dispersion. The velocity of light in the several metals followed exactly the same serial order as that of their respective conducting powers for electricity and heat. Since it was possible that the deviation of the rays while passing through the metals did not depend upon a true refraction, the speaker had recently examined the behaviour of the refractive indices of the metals at different temperatures. Metals whose refractive index is large showed an increase of the angle of deviation of light as the temperature rises, and thus all doubt as to the fact that he was here dealing with a true refraction was set aside. A further outcome of these experiments was to show that the velocity of light in metals is dependent on changes of temperature in a way exactly similar to that in which their electrical conductivity is dependent. In order to determine accurately the relationship of the velocity of light to their conductivity, these two values must be measured on one and the same piece of metal. When determining the electrical conductivity in films of metal as thin as those he was using for his optical researches, he found that the greatest difficulty was presented by the measurement of the thickness of the film. In his earlier researches, local thicknesses of 0·11 to 0·14 millionths of a millimetre were measured, values which approximate to the diameter of a molecule. These measurements, the preparation of transparent metallic prisms, and a number of other questions which have become prominent in the course of the above researches, partly carried out by pupils of the speaker, he intends to pursue further.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Life and Correspondence of Abraham Sharp: W. Cudworth (S. Low).—Transactions of the Royal Irish Academy, vol. xxix. Part 3, On Two-nosed Catenaries and their Application to the Design of Segmental Arches: T. Alexander and A. W. Thomson (Williams and Norgate).—A Treatise on Statics, vol. ii. fourth edition, corrected and enlarged: G. M. Minchin (Oxford, Clarendon Press).—Encyklopædie der Naturwissenschaften, Chemie, Zweite Abthg. 49 and 50 Liefg. (Breslau).—Encyklopædie der Naturwissenschaften, Botanik, Erste Abthg. 58 Liefg. (Breslau).—Results of Meteorological Observations made in New South Wales: H. C. Russell (Sydney).—Studies from the Laboratory of Physiological Chemistry, Sheffield Scientific School of Yale University for the Years 1887–88, vol. iii.: edited by R. H. Chittenden (New Haven).—Nautical Monographs, No. 5, the Great Storm off the Atlantic Coast of the United States, March 11–14, 1888: E. Hayden (Washington).—Annuaire de l'Observatoire de Bruxelles: D. Folie (Bruxelles, Hayez).—Butter Making in Denmark: S. Hoare (Norwich).—Le Climat de la Belgique en 1888: A. Lancaster (Bruxelles, Hayez).—Industrial Education in the South: Rev. A. D. Mayo (Washington).—Second Annual Report of the Liverpool Marine Biological Station on Puffin Island: W. A. Herdman (Liverpool).—Insect Life, vol. i. No. 7 (Washington).—Sources of the Nitrogen of Vegetation: Sir J. B. Lawes and Prof. J. H. Gilbert (Trübner).—London Geological Field Class Excursions during the Summer of 1888 (Philip).—Michigan Forestry Commission, First Report (Lansing).—Results of Rain, River, and Evaporation Observations made in New South Wales during 1887: H. C. Russell (Sydney).—Journal of the Society of Telegraph-Engineers and Electricians, vol. xvii. No. 76 (Spon).

CONTENTS.

THURSDAY, FEBRUARY 14, 1889.

ALPINE PHYSIOGRAPHY.

Die Gletscher der Ostalpen. Von Dr. Eduard Richter. Mit sieben Karten, zwei Ansichten, und vierundvierzig Profilen im Text. (Stuttgart: J. Engelhorn, 1888.)

The Alps. By Prof. F. Umlauft, Ph.D. Translated by Louisa Brough. With 110 Illustrations. (London: Kegan Paul, Trench, and Co., 1889.)

THE first of these two works—an elaborate memoir of 306 pages—forms the third volume of the "Handbücher zur Deutschen Landes- und Volkskunde," issued by the Centralkommission für wissenschaftliche Landeskunde von Deutschland. About one-sixth of the whole is occupied by some preliminary remarks, and a discussion of the snow-line and the methods of estimating it. As the author points out, this expression is a rather vague one, and in a later part of the book he indicates that it may be or has been used in four different senses: (1) the lower limit of the patches of *firn* or *névé*, which chiefly depends on the steepness and ruggedness of the mountains; (2) the lower limit of the connected permanent fields of snow and ice, excluding the glaciers which descend from them, which may be called the practical snow-line,—this depends mainly on the nature of a declivity and its aspect; (3) the "climatic" snow-line, a theoretical estimate of the snow-line arrived at by considering climatic factors only; (4) the normal snow-covering, estimated by the line up to which the snow melts away from the mountains. In proportion as the mountains become regular in form, the first, second, and fourth approach one another, and tend to coincide with the third, and all must be considered in arriving at a general estimation of the snow-line for any mountain group.

The author then passes in review the various districts of the Eastern Alps, and enters into details as to the distribution, arrangement, levels, and limits of the snow-fields and glaciers. In this section of the work a large amount of statistical information is collected, which, together with the references to the literature, cannot fail to be of much value to anyone engaged upon questions relating to climate, and especially investigations bearing upon glacial geology.

In the concluding part of the work the author gives a summary of the conclusions which follow from the foregoing collection of facts. These are rendered more readily intelligible by means of a map. A short description of this will perhaps be the simplest way of indicating the general results at which the author has arrived. As its scale is comparatively small, details of mountain topography are omitted for the sake of distinctness; but the chief river courses are clearly shown, and the boundary between the mountain and lowland regions, north and south, is indicated by a thin dotted line. But all the ground above the 2600 metres contour-line (a very rough mean for the snow-line of the whole region) is shaded. Thus treated, we may remark in passing, the map gives an excellent idea of what would be the structure of a group of islands produced by the submergence of a mountain region, and a comparison of it with a map of the Lofoten Islands, or indeed with many parts of the Scandinavian and even North British coasts, is not without interest. The "isochional" levels, as perhaps they might be called, are indicated by a series of dotted lines, these being graduated in hundreds of metres, the zones included between them having the snow-line at heights differing by this amount. Obviously, this is only a rough representation of the facts, because, as the author carefully points out, there are numerous minor variations, even in one and the same district, due to the form or composition of the mountains, the aspect of the slope, and the like.

In the zone of 2500 metres, only a small portion of the Eastern Alps is included, viz. the higher summits of the northern limestone zone, such as the Zugspitz and the Dachstein. The southern limit of this district trends slightly north of east. The line limiting the zone of 2600 metres on the western side runs a little south of Landeck and Innsbruck, and so does not include any important summit; but east of the Brenner it has a rather southerly trend, and practically passes along the crest of the Höhe Tauern. The limit of the zone of 2700 metres runs roughly parallel with the last, till at the Glockner group it turns sharply to the south, and then, bending back, it passes to the south of the Adamello group. Thus the Brenta Alta group, the Marmolata and other peaks of the South Tyrol Dolomites, are in the 2700 metres zone, though a few summits of the Julian Alps, at the extreme east of the map, are marked 2600 metres. There is yet one other contour-line—that including the region where the snow-line is at 2900 metres or even more. This is rudely elliptical in shape, and includes the greater part of the Bernina, Ortler, and Oetzthal groups, together with many of the peaks on the left bank of the Upper Innthal. It is thus obvious that the snow-line is not wholly dependent on mountain form, or disposition, or on general temperature. The Adamello stands well to the south of the Oetzthal, yet its snow-line is full 100 metres lower. The northern part of the Oetzthal is only a very little south of the Glockner, yet the snow-line in the one district is 200 metres higher than in the other. But the configuration of the Oetzthal group is, if anything, more favourable to the accumulation of snow, and one would have expected, at first sight, to find the difference incline in the other direction. It is, then, evident that other climatic factors are of great importance, not the least of these being the amount of precipitation during the winter months. On this subject some interesting evidence is adduced.

The book is in clear good type, and the maps and sections at the end are printed with a blank space, equal to a page, on the inner side, so that they can be kept in sight when the book is read—a very convenient arrangement too often neglected. Most of the illustrations in the text are only diagrammatic sections, but the two "views" of the Gaisberg Glacier and of the Marmolata Glacier are excellent of their kind. The former is really no more than a "pen-and-ink sketch," yet it gives a very good idea of an Alpine view, and is far better than the wretched caricatures of mountain scenery which too often do duty in English scientific text-books. It may be commended to our publishers.

The second volume before us is one larger in size, characterized in parts by the same laborious minuteness

but with little scientific value. It is the work of a geographer "pure and simple," and of one, as we suspect from internal evidence, who knows the Alps better from books than from personal experience. The larger half is devoted to Alpine topography, but this is often hardly more intelligible or interesting than a catalogue of names and altitudes. In our opinion Prof. Umlauft's plan of work is defective. At the outset, instead of impressing on the reader the relation of the Alps to the mountain ranges with which they are connected, he obscures it by an elaborate disquisition on their boundaries. The dominant physical features of the chain and its component ranges should have been at once sketched in bold outline, after which a more detailed description of the several districts might have been given. The reader would then have been furnished with a frame-work on which he could arrange the subordinate facts: now, unless previously familiar with the Alps, he will wander bewildered among a labyrinth of names and statistics. In short, Prof. Umlauft appears to be a geographer of the old school. If he has any scientific knowledge, whether as geologist or naturalist, this book affords not only no evidence in favour, but also not a little to the contrary.

For instance, in any notice of the geology of the Alps his statements are commonly unsatisfactory, and sometimes absurd. For the latter, perhaps, the translator is partly responsible, for occasionally a sentence occurs which is devoid of meaning, such as this, "Many of the lofty peaks are composed of Triassic limestones up to the metamorphic dachstein, which, inclosed in cardita, forms the peak." The paragraphs treating of valleys, lake basins, ice-caves, glaciers, erosion, and weathering, are all inadequate and unsatisfactory: all show traces of the unskilled compiler's hand. For example, in the chapter devoted to the last-named subject, the author, in speaking of "giants' caldrons," omits to state that the most remarkable examples, those, indeed, to which this name is commonly applied, and we think usually restricted, have been made by the action of streams which once plunged down the *moulins* of glaciers. It is quite true that these are not excluded by the author's words, but no hint of their occurrence is given, and no mention is made of the remarkable instance at the "glacier garden" near the Lion monument at Lucerne. It is difficult, without long quotations accompanied by a running commentary, to give an idea of the number of small defects or inaccuracies which abound in the book. We may, however, select, without actual quotation, the pages 127–36, which deal with the orography of the St. Gothard district. Here the Rhone glacier is said to be "over twenty miles long,"—probably fifteen would be nearer the mark. The "granite" of the table-land of the St. Gothard is "famed for its great crystals of feldspar." True, these crystals are fairly large, but not remarkable—much less than those occurring on the Lukmanier, which are often quite 3 inches long. Not the granite but the schist is famed "for the great number of minerals found in it." Among these minerals are enumerated various "zoolites" (zeolites), but neither albite, for which the St. Gothard is a rather noted locality, nor garnets, nor tremolite, nor actinolite, which are so abundant near the Val Tremola. One or two more slight inaccuracies, on which it is needless to dwell, may be noted in these pages.

Perhaps one of the most conspicuous instances will be found on p. 49, in a table of the eleven Alpine peaks which surpass 14,800 feet in height. First comes "Mont Blanc, highest point 15,779"; then, "*id.*, Mont Maudit, 15,651"; and third, "*id.*, Cour Mayeur (*sic*), 15,602." This is misleading. It is quite true that the actual summit of Mont Blanc lies a little north of a slight prominence which from Courmayeur appears to be the summit. The latter, however, is not a separate peak in any respect comparable with the Mont Maudit. It is a case very similar to the "Wengern Jungfrau" and the true summit, and on this principle separate peaks might be manufactured to any extent in the Alps. Further down is a more serious error. We find "Mischabelhörner, Täschhorn, 14,972 feet"; and a little lower in the list, "Mischabelhörner, Grabhorn, 14,949 feet." But the latter, more usually called the Grabenhorn or Dom, is the higher peak, as is correctly stated elsewhere in the volume, though there the altitudes given are not the same. There is yet another error. The author enumerates three of the actual peaks of Monte Rosa; then at the end of the list he places "Monte Rosa, Lyskamm, 14,887 feet," and "*id.*, Weisshorn, 14,804 feet." But the Lyskamm is always regarded as a separate mountain, and the depression between the two, crossed by the well-known Lysjock, though it is only 800 or 900 feet lower, is so wide and well marked as to justify the separation. But to rank the Weisshorn as a peak of Monte Rosa is hopelessly indefensible. The mountains are more than ten miles apart as the crow flies, and separated by the deep trench of the Vispthal.

One rather short chapter is devoted to the fauna and flora of the Alps, and the information there given is extremely scanty, and not seldom inaccurate. For example, the rhododendrons only appear under the vague and misleading trivial name of the "Alpine rose," and it is not even hinted that there is a true *Rosa alpina*. Heaths, again, are hardly to be enumerated among the higher Alpine plants, and azaleas are only represented by the abundant but very inconspicuous *Azalea procumbens*. The account of the fauna is equally unsatisfactory. The chamois receives only a passing mention, and is not enumerated among the animals frequently found above the snow-line. The steinbock (*Capra ibex*) is briefly alluded to under the name of the "wild goat." The birds are vaguely enumerated, and, while undue prominence is given to some, others, which as a rule especially attract the traveller's notice, are passed over in silence. The insects are almost wholly neglected; yet, without entering into many scientific details, it might have been possible to give some idea of the crowds of little blue butterflies (*Polyommatus*) that flutter about the puddles on the pathways, of the coppers (*Lycæna*), ringlets (*Hipparchia*), fritillaries (*Argynnis* and *Melitæa*), clouded-yellows (*Colias*), and Apollos (*Parnassius*), which impress the traveller accustomed only to the European lowlands, when first he rambles among the true Alpine regions. It would be easy to name more than one similar work, by no means of recent date, which in this respect is far superior to Prof. Umlauft's.

The illustrations are numerous, but rather unequal in quality. Some are fairly good, but frequently, while accurate in general effect, having probably been engraved

from photographs, they are defective in character. A glance at the representation of the gorge of the Tamina on p. 48, or that of Monte Cristallo from the Dürrensee (facing p. 86), will indicate the nature of our objection. The rocks might be moulded from plaster or canvas. There is, however, a very clearly printed geological map of the Alps, which appears to us generally accurate, though we doubt the correctness of placing a considerable patch of Silurian and Devonian about the upper part of the Brenner Pass.

T. G. BONNEY.

THE PLANTING AND AGRICULTURAL INDUSTRIES OF CEYLON.

Review of the Planting and Agricultural Industries of Ceylon, and Statistics of the Planting Enterprises in India and the Colonies. By J. Ferguson. Pp. 168. (Colombo: A. M. and J. Ferguson, 1888.)

THIS is a reprint, in the form of a small octavo volume, of information contained in Ferguson's "Ceylon Hand-book and Directory," specially relating to the tropical cultures of Ceylon. It affords much authentic information in a handy and accessible form, and is a valuable summary of the results attained in the cultivation of most economic plants suited to a tropical country. Ceylon itself is a singularly interesting island. It is usually described as the largest, most populous, and most important of the Crown Colonies of Great Britain. It has in recent years become the seat of planting industries which have in one or two instances almost monopolized the markets of the world. It is six times the size of Jamaica, and about five-sixths the size of Ireland. Of its sixteen million acres, at present only about three millions are under cultivation, and these support a population of exactly the same number. The value of the imports and exports amounts to about ten millions sterling. The total number of European residents in Ceylon is under five thousand, while the mixed or coloured population called Eurasians or Burghers amounted to about nineteen thousand. The bulk of the population, amounting to nearly two million souls, is composed of Sinhalese—a remarkably tractable and inoffensive people—while the remainder is made up of Tamils, Moormen, Malays, and Veddahs. The latter are an aboriginal race, comparatively few in number, inhabiting the forests of the north-east.

Although the number of the Sinhalese is relatively so large, they contribute very little to the labour supply of the European plantations. Plantation labour is furnished by Tamil coolies from Southern India. According to a Report published by the Government of Madras, out of a population of thirty-five millions of human beings in that Presidency there are sixteen millions whose annual earnings do not average more than £3 12*s*., or a little over 2½*d*. per day. Thus it is that the plantations of Ceylon, paying about 6*d*. or 9*d*. per day, are abundantly supplied with cheap free labour.

The purely European enterprises consist of tea, coffee —both Arabian and Liberian—cacao, cardamoms, rubber, annatto, vanilla, pepper, fibres, nutmeg, cloves, dye-plants. In these is invested English capital to the amount of about eight millions sterling. The native industries are associated with the cultivation of the cocoa-nut palm—yielding oil, coir, and copra—rice, cinnamon, palmyra palm, kitul or jaggery palm, areca palm citronella and lemon grass, tobacco, cotton, sugar-cane, dry grains such as kollu, millet, kurakkan, maize, and numerous vegetables and fruits. It is estimated that there are nearly fifty million cocoa-nut palms in Ceylon, and the yearly yield cannot be less than about 500 million nuts. Next to the cocoa-nut palm, the palmyra palm (*Borassus flabelliformis*) is regarded as one of the richest plants known. According to a Tamil proverb, "It lives for a lac of years after planting, and lasts for a lac of years when felled." Jaggery sugar is made from the sap, and in the dry, arid regions of the north-east of Ceylon more than seventy million nuts are annually produced. The young sprouting nuts are used as a vegetable. The kitul (*Caryota urens*) is another sugar-palm, which, in addition, yields a coarse black fibre used in broom-making. Cinnamon is essentially a native industry. The island has been famous for this spice "from the dawn of historical records." There is a Sinhalese caste of cinnamon peelers, and these, the Chaliyas, hold practically a monopoly in preparing the bark for the market. The dry grain cultivation is associated with that baneful *chena* practice of recklessly cutting down and burning virgin forests—now, we are glad to notice, in course of being kept within proper bounds. The natives of Ceylon have imitated the Europeans in many industries, but by far the greater number are content to follow in the footsteps of their ancestors, and cultivate only such plants as cocoa-nuts, rice, fruits, and vegetables, necessary to supply their daily wants.

For many years the chief European industry was that of coffee. From 1825, when Sir Edward Baines started the first upland coffee plantation near Kandy, to 1875, when Ceylon exported nearly a million hundredweights, "coffee was king." In 1869, a microscopic fungus (*Hemeleis vastatrix*) made its appearance on the leaves of the coffee-plant. This spread with such rapidity, and with such destructive effect, that within a few years the Ceylon coffee plantations were doomed. The disease extended also to Southern India, to Sumatra, and Java; it invaded Mauritius, Madagascar, and Natal, and reached even the young and promising plantations of Fiji. After twenty years' experience of this pest, the Ceylon coffee plantations have so dwindled that the present exports are only one-tenth of what they once were. Fortunately the decline of coffee was accompanied by the extension of cultivation of cinchona, cardamoms, cacao, and tea Ceylon cinchona has been produced in such quantities that the markets have been completely glutted. In consequence the price of bark has fallen so low that the cultivation is unremunerative. The attention of Ceylon planters is now being concentrated, with their accustomed energy, on the cultivation of tea. Coffee, cinchona, and everything not immediately remunerative are being uprooted to give place to the new staple. Although the industry is not more than ten or twelve years old, Ceylon tea is already being exported to the value of £600,000. Tea therefore bids fair to take the place of coffee, and thus the cloud which has overshadowed the prosperity of the island during the last few years is gradually passing away. Ceylon cacao is excellent, but the industry is small and apparently stationary. It is doubtful whether the island possesses

any really large extent of land suitable for the growth of the cacao-plant. The rubber industry in Ceylon, as elsewhere, is mysteriously unproductive, while the cultivation of vanilla, pepper, and fibres, is only in the experimental stage. The total areas under the various cultivations at present are: tea, 183,000 acres; coffee (Arabian), 77,000 acres; coffee (Liberian), 916 acres; cinchona, 36,000,000 trees over two years old; cacao, 12,000 acres; cardamoms, 5000 acres; rubber-trees, 386 acres; croton, castor-oil, aloes, cinnamon, vanilla, pepper, cloves, plantains, and citronella grass, 7400 acres; gum-trees, fruit-trees, sapan, sapu, cocoa-nuts, areca-nuts, nutmegs, 4600 acres.

Such are a few of the gleanings from this useful account of the planting and agricultural industries of Ceylon. Mr. Ferguson is favourably known as a successful journalist, and as the author or joint-author of numerous publications connected with the island in which he has spent the greater part of his life. Indeed, it would not be too much to say that Mr. Ferguson and his uncle have contributed by their writings in no small degree to promote the various industries upon which the prosperity of Ceylon depends. To those whose interest or whose business is connected with tropical cultures this summary will prove most useful. It covers a wide field, but, so far as Ceylon is concerned, it contains information available in no other way. The historical and statistical facts, no less than the points respecting the treatment of tropical plants, are collected from trustworthy sources, and are of interest wherever such plants are cultivated, and we may add scarcely a single tropical product is passed unnoticed. D. M.

PALÆONTOLOGY.

Die Stämme des Thierreiches. Von M. Neumayr. "Wirbellose Thiere." Erster Band. (Wien und Prag: Tempsky, 1889.)

THE palæontologist has been defined as a variety of naturalist who poses among geologists as one learned in zoology, and among zoologists as one learned in geology, whilst in reality his skill in both sciences is diminutive. The division of zoology into palæontology and neontology is one which is, no doubt, logically defensible, and so would be a division of the subject-matter of zoology into as many branches as there are periods recognized by geologists—cambriontologists, siluriontologists, anthrakontologists, &c. On the other hand, it must be admitted that such divisions seem unlikely to tend to the furtherance of our knowledge of animal life in the past. The fragmentary remains of extinct animals can only be interpreted by the application to them of a very thorough knowledge of the form and structure of living animals, and accordingly it would seem desirable that, as is more usually the case in regard to the study of plant remains than in regard to that of animal remains, the study of palæontology should be relegated to those who also occupy themselves with neontology. The botanists, with few exceptions, pursue this plan; but curiously enough, a special class of palæontological zoologists exists and flourishes. A further advantage to be derived from the suppression of palæontologists seems to be this—that there would be a better chance for the cultivation of true geology, which now, to some extent, has its professorial positions, its museums, and its publications invaded by these specialists. Whatever may be said in favour of the palæcntologist, he cannot be allowed to claim geology as his own; nor should the capable geologist, as is unfortunately and so frequently the case, venture beyond his last, and discourse on zoology in the disguise of a palæontologist, for the disguise cannot effectually conceal his incompetency to deal with zoological problems.

Whilst believing that it is increasingly desirable that the truth of the above propositions should be recognized and acted upon, we are yet prepared to admit that, as a practical division of labour in the great field of zoological science, palæontology must be recognized. Human knowledge does not develop according to abstract conceptions of the relations of one branch of study to another, but on a much more homely basis, open to philosophical objections of the most severe character. The way in which things have presented themselves to the hands and minds of students in consequence of practical demands or special opportunities of study is that which has determined the existence of the various divisions of natural history and the consequent groups of naturalists devoted to one or another unphilosophically limited pursuit. The collecting of "fossils," the hammering out of the fragments of a past world from their stony graves, the cultivation of the faculty of recognizing the significance of minute and detailed portions of fossilized shells and bones, is a definite hobby, which has excited the enthusiasm and stimulated the ingenuity of a long list of remarkable men, such as Woodward, Mantel, Barrande, and a host of less-known collectors. It seems not improbable that, were the remains of extinct animals in our great public and educational collections classified and placed with those of recent forms, an injustice, not to say an injury, would be done to the special phase of scientific activity which has produced these collections, and the important knowledge of the history of life on the earth which they represent. The morphologist, dealing with the complete structure of recent forms, is liable to neglect all but the more perfect remains brought to light by the collector of fossils; whilst, on the other hand, the palæontologist interprets the most obscure fragments, and speculates, it may be audaciously, but not unwisely, upon the significance of all that comes to his hand.

The volume which has led to these remarks is the first part of a treatise which is improperly named. It is not a treatise upon the pedigree of the animal kingdom, but an account of extinct animals treated in zoological order—in fact, a manual of palæontology. As an introduction, there is an extensive essay on Darwinism, and a discussion of Lamarckism and the causes of variation, which are becoming more and more the absorbing topics of the day. Dr. Neumayr does not appear to make any contribution of general importance to the discussion; but he makes the doctrine of descent and the Darwinian theory of natural selection the guiding principles of his treatise. The neontologist, if we may venture to call anyone by that name, will find in Dr. Neumayr's pages many facts of great value in the elucidation of biological problems, and a number of excellent woodcuts. The work promises to be one of considerable size, since this first volume consists of six hundred pages royal

octavo. It is written from the point of view of the author's nationality, and naturally such new matter as it contains is chiefly in reference to the palæontology of the Austrian Empire. E. R. L.

OUR BOOK SHELF.

Text-book of Physiography. By Edward Hull, M.A., LL.D., F.R.S. (London: Deacon and Co., 1888.)

FROM the Director of the Irish Geological Survey we should naturally expect a text-book of exceptional merit, but we must confess at the outset that he has disappointed us. In the first place, he does not seem to have a clear conception as to the scope of his subject. Physiography is essentially an introduction to the study of natural forces and their effects, and consequently not only comprises the various movements and physical features of the earth, but also includes a study of the various forms of energy and the properties of matter. Of the latter, Prof. Hull has written nothing. Again, it is difficult to see for what class of students the book has been written. It is evidently not for beginners, being avowedly addressed to those who have access to the Transactions of the learned Societies; and the treatment is far too superficial for advanced students.

The first part of the book is designated "Astronomical and Introductory," the earth being considered in relation to the other planets. The questions of latitude and longitude, and a chapter on the moon, fall under this head. This part of the subject is treated so briefly that very careful reading will be necessary on the part of those who are not previously acquainted with it.

Part II. deals with "Terrestrial Physics and Dynamics," and discusses the form and structure of the earth, volcanoes, and earthquakes. The theory of a viscous stratum beneath the earth's crust is put forward as confidently as if it were the law of superposition of strata, and all reference to the objections which have been urged against it is omitted. It would be hard to compress more debatable matter into a page than has been effected in the diagram which illustrates this theory (p. 55).

The physical features of the globe, such as surface forms, oceans, coral islands, tides, air currents, and the functions of rivers and glaciers, are treated in Part III. Terrestrial magnetism also falls under this head, and this is really an excellent outline of the subject, as far as results go; but practically nothing is said about the instruments which are employed. As a compromise between the views of Darwin and Murray regarding coral reefs, Prof. Hull suggests (p. 110) that "the volcanic islands and banks of organic materials are themselves planted on a floor formed by the surface of a continent, which once occupied the region of the Central and Western Pacific." We will leave the opposing parties to form their own opinions as to the value of this suggestion.

The geographical distribution of plants and animals forms the subject of Part IV., and here there is little to complain of.

The book is illustrated with thirteen coloured plates and maps, and eleven diagrams. Some of these, as, for example, the map showing the lines of equal magnetic variation and declination for the British Isles, are excellent. The whole book bears traces of having been written hastily, and we cannot but regret that the author of "The Coal-fields of Great Britain" should have added one more to the already too large number of text-books that seem to present physiography as a subject in which no originality is possible.

The Clyde, from its Sources to the Sea. By W. J. Millar, C.E. (London: Blackie and Sons, 1888.)

MR. MILLAR has succeeded in writing an interesting book about the Clyde, and about Glasgow in particular. The subject is worthy of the care devoted to it by the author, for what river or city in the United Kingdom has more varied industries to boast of, and where are the applications of science more numerous?

Probably no river owes its improvements more to the engineer than the Clyde. We are told how eminent engineers were called in, and surveys made, in order to deepen the river and make it more navigable; Smeaton and James Watt each had their turn, and afterwards many well-known men in their time reported on the same subject. The result is that the Clyde of to-day is able to float the largest ocean steamers in its harbour, a state of things of which the people of Glasgow are justly proud.

The growth of the steam-ship, of course, occupies much space, since it was on the Clyde the first successful attempts at steam navigation were made. These are duly described, and the boats illustrated. On recent Clyde-built ships our author has much to say, and, among other things, he gives an account of some experiments conducted by the late Mr. William Denny to investigate the relation existing between speed and resistance of ships. Messrs. Denny, at their ship-building yard at Dumbarton, have constructed an experimental tank with all the requisite machinery for the purpose, thus carrying on the investigations initiated by the late Dr. Froude.

This volume gives one a good insight into the varying industries carried on in Glasgow and its neighbourhood, and contains much general information about the district. The book is well written, nicely printed and illustrated, and should find a place in the libraries of the citizens of St. Mungo, and others interested in the progress of the district.

A Playtime Naturalist. By Dr. J. E. Taylor, F.L.S. (London: Chatto and Windus, 1889.)

DR. TAYLOR explains that he has a liking for intelligent English lads, "just as some people have for blue china and etchings"; and he "ventures to think the former are even more interesting objects." Accordingly he has written this little book for the instruction and entertainment of his "human hobbies." The work contains abundant evidence of the author's knowledge and enthusiasm, and any boy who may read it carefully is sure to find something to attract him in the chapters on birds, Lepidoptera, land shells, toads, frogs, newts, invisible life, microscopic plants, and other subjects. The style is clear and lively, and there are many good illustrations.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Climate of Siberia in the Mammoth Age.

PROF. A. NEWTON, in his remarks on my letter, says that the similarity of the birds of Japan and of Europe has been long known. Of course it has. It is an elementary postulate in geographical zoology; but this is not the fact to which I called special attention, and from which I drew my inference. That fact is that, while the birds of Japan and England are in certain species undistinguishable, the corresponding birds of Siberia are sufficiently different to be classed as separate species. This could not be known, in the sense of being proved, until the avifauna of Siberia had been worked out from end to end, resulting in the formation of such a continuous series of skins as that in the possession of my friend Mr. Seebohm.

Prof. Newton goes on to argue that the remarkable fact here referred to is to be explained by the hypothesis that the birds of

Japan and Europe live in insular climates, while those of Siberia occupy an area with a continental climate. I do not think many biologists can accept this explanation. England and Japan are 3700 miles apart. That a single bird whose normal type is found in Siberia should vary from that type in two areas thus remote from one another in precisely the same way is perhaps possible. That a whole string of birds (and I only mentioned a sample) should do the same is, it seems to me, when tested by the doctrine of chances and the infinite variability of bird structure and colour, an impossibility.

Apart from this *a priori* argument, we have the fact—which is, of course, known to Prof. Newton—that Blakiston's line separating Yesso and the southern island of Japan also separates two avifaunas, and that the species on each side of the narrow strait of Yesso are in many cases different, although they live in insular areas close to one another, and subject to virtually the same insular climate, while those of England and Japan, whose climate is not so like, are undistinguishable. I cannot, therefore, for a moment accept Prof. Newton's theory as in any way meeting the facts which are admirably met at every point by the theory which I have propounded, and which is not based on the variation of the birds alone, but upon a whole *catena* of converging evidence from many sides, the evidence of the birds being only a subsidiary support.

I am sorry that I had overlooked Prof. Newton's article in the "Encyclopædia," and am glad that my suggestion about the red grouse, which I can assure him was quite independently made, had already occurred to and been countenanced by so distinguished an ornithologist, against whom I hear continual complaints, which ought to be very flattering, that he writes too little.

Reverting to the main issue, it is a great pleasure to me to have Prof. James Geikie's permission to publish an extract from a letter which he has sent to me, in which he is completely at one with me in the conclusion that, when the mammoth lived, the climate of Siberia was temperate, and that it lived where its remains are found. This is particularly gratifying to me, not only because Prof. J. Geikie is the most learned and voluminous writer upon the so-called Pleistocene age, his stout volumes being marked as much by their extraordinary profusion of references and of facts as by their lucid arrangement, but because upon some of the main conclusions I have arrived at he takes a very different view. Prof. J. Geikie says:—

"I do not need to be converted to the view that Siberia formerly enjoyed a temperate climate. If you will consult the first edition of my 'Great Ice Age' (p. 494), you will see that my belief for the last fifteen years has been that the mammalian remains of North Siberia are the relics of a fauna that lived and died in those now dreary regions. Indeed, I had that notion when I first began to read what had been written upon the subject some five-and-twenty years ago! I was willing, however, to admit the possibility of some of the remains having been drifted north by rivers. But it has always seemed to me inconceivable that this drifting would account for the presence of such great ossiferous accumulations as travellers have described. I likewise long ago discarded the notion of seasonal migrations, such as Dawkins and others have maintained (see *Geol. Mag.*, 1872, p. 164; 1873, p. 49)."

I shall not labour the argument further, nor shall I enlarge upon what I deem to be an inevitable corollary from it—viz. that if the climate of Siberia was temperate when the mammoth lived, and if it lived where its remains occur, on the now bare and almost perpetually frozen *tundra*, it follows that its extinction there must have been followed by a most rapid, if not a sudden, change of climate. The existence of its *undecayed carcasses* in all parts of Siberia, from the Obi to the Indigirka, is consistent only with this conclusion. If the change of climate had been gradual, the flesh of the great beasts could not have been preserved intact, but would have putrefied and decayed. This was long ago seen and emphasized by Cuvier, and even Lyell was constrained to write:—"It is certain that, from the moment when the carcasses both of the rhinoceros and the elephant above described were buried in Siberia, in lat. 64° and 70° N., the soil has remained frozen, and the atmosphere as cold as at this day." Again, he says:—"One thing is clear, that the ice or congealed mud in which the bodies of such quadrupeds were enveloped has never once been melted since the day when they perished, so as to allow the percolation of water through the matrix, for, had this been the case, the soft parts of the animals could not have remained undecomposed." It was to avoid the necessarily awkward inference from this conclusion, for one who preached uniformity so continuously, that Lyell was forced to invoke his theory of river portage, which is no longer tenable, and, so far as I know, is no longer held by any serious student.

HENRY H. HOWORTH.

Bentcliffe, Eccles, February 3.

Peripatus in Victoria.

IT may interest some of the readers of your journal to know that last week, while collecting in a fern-tree gully at Warburton, on the Upper Yarra, Victoria, I had the good fortune to discover two specimens of *Peripatus*, belonging, as I think, to a new and certainly to a very beautiful species.

I hope to publish a full description, with figures, of the species as soon as possible, but I am now preparing for a visit to Tasmania, and some time must necessarily elapse before I can complete the work. I should therefore be greatly obliged if you could find space for this letter in NATURE.

In his "Monograph on the Species and Distribution of the Genus *Peripatus*," recently published in the *Quarterly Journal of Microscopical Science*, Prof. Sedgwick makes no mention of the occurrence of the genus in Victoria; though he describes in detail the Queensland and New Zealand species. In a note in the Proceedings of the Linnean Society of New South Wales (vol. ii. Part 1, 1887), however, Mr. Fletcher has recorded the discovery of the genus in Victoria. He says, "The specimen which I exhibit this evening was given to me a fortnight ago by my friend Mr. R. T. Baker, of Newington College, who had obtained it a few days previously either in or under a rotten log at Warragul, Gippsland, Victoria. It has fifteen pairs of claw-bearing appendages, and has nearly the same dimensions as are given in the abstract referred to; it is therefore in all probability an example of *P. leuckartii*, Sänger."

From Mr. Fletcher's account I am not able to say definitely whether the specimens obtained by me belong to the same species as the single specimen which he mentions; but after carefully studying Prof. Sedgwick's full description of *P. leuckartii*, I am fairly certain that they do not belong to that species, but to a new one which I for the present refrain from naming.

Both of my specimens were captured under fallen logs, where they were lying quite still. The first appeared to be dead soon after it was caught, and was therefore placed at once in alcohol. The second was found under a damp, rotten log, probably of *Eucalyptus*, in the same gully. It was taken home alive and put to crawl about on a newspaper, when it appeared very active. It elongated considerably when crawling, so that the legs came to be much further apart than when the animal was at rest, and when crawling it measured about 39 millimetres in length, excluding the antennæ. When irritated at the head end it ejected a surprisingly large quantity of an intensely sticky fluid, of a whitish colour, from the oral papillæ.

The species has, as in the two already described Australasian forms, fifteen pairs of claw-bearing legs, but it differs very strikingly indeed both from *P. leuckartii* and from *P. novæ-zealandiæ* in the colour and markings of the body. The general tint is brownish red, with only traces in one specimen of the bluish colour so characteristic of the two above-mentioned species. The markings on the body are singularly distinct and well defined, and identical in the two specimens. All down the dorsal surface there runs a median broad reddish-brown or chestnut-coloured band, divided into a series of diamond-shaped patches by regular lateral indentations, one diamond corresponding to each pair of legs. In the middle of this band there is a thin, median, whitish line. On either side the chestnut-coloured band is edged by a narrow black line, which follows the indentations of its margin, and outside this comes a broad band of darker brown, and then, at the edge of the dorsal surface, a narrow band of light brown. The ventral surface is light yellowish-brown, speckled with spots of very dark pigment, especially abundant at the base of each leg. In the mid-ventral line there is a row of white spots, one between the two legs of each pair except the first (?) and the last (where, of course, the genital opening is situated). The antennæ are light brown, closely ringed all the way up with very dark brown or black.

This species, though small, is to my mind even more beautiful than any of those figured by Prof. Sedgwick, and I think there can be little doubt as to its distinctness. The anatomical features I hope to describe at a later date, and perhaps they will throw further light upon its relations to previously described forms.

ARTHUR DENDY.

University of Melbourne, December 18, 1888.

Mass and Inertia.

PROF. WORTHINGTON is perfectly right in saying that in my little book on mechanics I did not carefully and solely use the term inertia in the precise sense I suggested for it in my last letter. The fact that m is really only the coefficient of inertia had not been seized by me when I wrote that book. The idea of calling mass-acceleration inertia simply, was suggested, I believe, by a discussion on Newton's third law of motion in the pages of the *Engineer* some few years back. It is a suggestion which has gradually commended itself to me, and I am calling the attention of the British Association Committee on Mechanical Units and Nomenclature to it.

With regard to the other matter referred to by Prof. Worthington, it scarcely strikes one as a satisfactory plan to have one system of units for teaching and another for actual use. Is it not better to get students to tackle difficulties rather than evade them? OLIVER J. LODGE.

The Crystallization of Lake Ice.

THE percussion figures that Mr. Holland has discovered both are interesting in themselves and seem to be a very handy means of marking off one crystal from another in thin lake ice. Their symmetry about a vertical axis is evidence that the optic axes of the crystals were vertical. The small amount of snow here this winter has afforded unusual opportunities for examining the ice on the Davos lake, and I have found crystals, not indeed equal to those on the Welsh lakes, but still very large. A striking feature in the ice, about a week after it was strong enough to bear skaters, was the presence of a number of hexagonal disks, of all sizes up to a quarter of an inch diameter, with their planes apparently horizontal. Some were regular hexagons, but generally the sides were unequal, though the angles were always 120°. I concluded that within a single crystal all the hexagons would be similarly oriented, and that an interface of two crystals could be distinguished by a sudden twist in the direction of the sides. Judged by this test some of the crystals were at least a foot broad, and in depth no doubt equal to the thickness of the ice, at that time about a foot. To verify this conclusion Mr. Kidd hacked out a piece with the axe, and we prepared a rough plate six inches long and three thick, which we examined in the polariscope. The rings and cross were easily seen, and the plate proved to be all one crystal with the optic axis vertical *in situ*.

These hexagons are not identical with the figures observed by Prof. Tyndall in the path of a sunbeam through ice (described in "Forms of Water"), for at the time I saw them the ice was so cold that water froze rapidly in any hole that was made. A friend describes them as looking more like bits of cover-slip glass than anything else. They were formed, I was told, on a day when the warm Föhn wind was blowing, and the ice, no doubt, was at a thawing temperature. But the puzzle is, why they did not vanish when the temperature fell. They reflected light strongly, far more than Tyndall figures, and in some cases showed the colours of thin plates. I noticed that those that gleamed with reflected sunlight often lay considerably to right or left of the vertical plane through the sun. This showed that their planes were (allowing for refraction) inclined sometimes as much as 10° or 15° to the horizontal, and inferentially that the optic axes of the crystals were tilted an equal amount from the vertical. I hunted about all over the lake for signs of the columnar structure that I described in my article on "The Plasticity of Ice," but only succeeded in detecting it in one place close to the shore. We cut out a piece there and verified the existence of the columns with the polariscope. There were no hexagons in that part of the ice.

Of the St. Moritz lake last winter I can only give a very imperfect account. For at the time I began observations the great depth of snow had sunk the ice, and water had oozed into the snow and there frozen, so that the clear ice was covered with some eighteen inches of hard snow ice. The only part easy of access was where a supply of ice was being cut for the hotels. The process adopted was, after clearing the ice from a certain space, to leave that for a week or two, till the new ice had reached the thickness of a foot, and then cut it again. Both the new ice and the old ice in its neighbourhood was columnar. In one place, however, at some distance from the shore, where we got out a lump of clear ice, we found crystals with the optic axis vertical, and one, at least, three or four inches across.

Davos Platz, January 29. JAMES C. MCCONNEL.

Falls of Rock at Niagara.

THE following passages, which will interest geologists, I copy from the *Montreal Daily Star* of the dates given :—

"Niagara Falls, Ontario, January 7.—Last Friday evening, about 9 o'clock, a large mass of rock fell from the precipice of the Horse-shoe or Canadian Falls, and on Saturday night, at 10 o'clock, another mass broke away. In both cases the noise made alarmed the residents in the vicinity. In the Table Rock House, a stone building, doors were thrown open, and the occupants jumped out of bed greatly excited by the unusual noise and vibration, resembling severe shocks of an earthquake. The same sensation was experienced at the residence of the gate-keeper on Cedar Island, and also half a mile up the river. The effect of these falls on the contour of the cataract is quite marked, the change being from that of an angle at the vertex to the original horse-shoe shape."

"Niagara Falls, Ontario, January 15.—Another piece of rock broke away from the crest of the Horse-shoe on Sunday night. Although the jar was comparatively slight, the shock was distinctly felt at the Table Rock House. The cataract now presents the extraordinary shape of a double horse-shoe, the smaller one caused by the recent displacement being in advance and to the right of the great horse-shoe. Visitors familiar with the shape of the Canadian Falls during recent years will be able to appreciate the change at a glance.

"Thousands of people visited the Falls yesterday and to-day to view the relics of the bridge torn down by the late gale,—this is the upper suspension bridge, close to the Falls, destroyed by the storm of last week,—and also to enjoy the magnificent scenery which Niagara always presents when arrayed in her winter apparel. The contract for a new bridge to replace the one destroyed has already been let, and the work will be completed in ninety days."

Additional facts are here furnished in favour of the opinion that the recession of the great cataract is going on at a rate much more rapid than some have maintained, and more rapid than was estimated by Sir C. Lyell in 1842. Indeed, the rate given by Mr. Bakewell in his work on geology seems to have been nearer to the truth, 3 feet per annum instead of the 1 foot assumed by the author of the "Principles of Geology."

Akron, Ohio. E. W. CLAYPOLE.

Origin of the Radiolarian Earth of Barbados.

THE Barbados infusorial earth is well known for the beautiful specimens of Polycystina which it contains, but concerning the rock itself, its geological position, and probable mode of formation, little has been written.

Schomburgk, in his history of Barbados, gave a general description of it and indicated some of the localities where it had been found, but he did not separate it geologically from the group which he designated the "Scotland formation." One of us having resided in the island for some years has had opportunities of studying the lie of the deposit, and has found that it always overlies the rest of the Scotland beds, and that it generally, although not invariably, intervenes between them and the raised coral reefs which form the surface of the greater portion of the island. It has been found below the coral in certain borings recently made by the Barbados Water-supply Company, and there can be little doubt that it originally formed a sheet of considerable thickness extending beyond the present limits of the island.

The rock itself varies much in composition: in some places it is almost purely siliceous, consisting mainly of Radiolaria and Diatomaceæ, whilst in others it is largely calcareous (one sample having yielded as much as 79·9 per cent. of calcium carbonate), containing in places many Foraminifera. The more siliceous specimens agree closely with the descriptions given of those deep-sea oozes which contain Radiolaria and are more or less destitute of Foraminifera. We intend to pursue our investigation of the deposit, and to compare it, if possible, with samples of modern Radiolarian ooze, but the facts already known to us render it highly probable that the deposit is part of a raised ocean bed. If this conclusion be confirmed, it will correct the prevalent belieef that oceanic deposits are not to be found amongst the rocks which form continents and continental islands, and will at the same time form a strong and well-nigh invincible argument against the theory of the permanence of oceans, a theory which has recently been discussed and rejected by one of us. J. B. HARRISON.

A. J. JUKES BROWNE.

Natural History in the Field.

WILL you allow me to draw the attention of students of field botany, field naturalists, and those interested in encouraging natural history in the field, especially schoolmasters who may be initiating classes for the study of our native plants, to the high probability of the present year being quite an exceptionally prolific year for all our sun-loving vegetable flora.

The want of sunshine last year kept all our wild flowers back. Nothing had its full development: flowers were late, foliage was thin, colours were dull and undefined, fruit small and without flavour, seeds unripe.

But one season's loss is the next season's gain; in all probability the plants this year will be exceptionally fine, and many plants that are usually small and poor will flourish with unusual vigour, while, not improbably, many plants which seldom show themselves here will this year blossom and become visible.

On this account it might be well to advise the starting of classes for the study this year of field natural history, for students, particularly young students, are encouraged to go on with a pursuit that has been very successful at its commencement.

Chigwell. W. LINTON WILSON.

Detonating Meteor.

ACCORDING to the Jamaica Weather Report for November 1888, a very brilliant meteor was seen at Kingston, Jamaica, on the evening of November 10, at 8h. 52m. local mean time.

It appeared about 30° above the south-west horizon, crossed the heavens, and disappeared about 30° above the north-north-east horizon; and as Kingston is in lat. 18° N., we have for the point of appearance the celestial co-ordinates R.A. 21h. 24m., N.P.D. 113°, and for the point of disappearance, R.A. 3h. 45m., N.P.D. 25°.

Mr. R. Johnstone writes:—"It was by far the brightest meteor I have ever seen, and it so lit up the sky as to cause consternation among many of the negro population. Exactly four minutes afterwards I heard a sound as of a distant explosion, which was not quite so loud as the 9 o'clock gun at Port Royal, heard in due time about four minutes later. The sound was heard by other people in Kingston."

As Kingston is 5h. 7m. W. of Greenwich, the meteor appeared November 11, 1h. 59m. a.m. Greenwich civil time; and therefore the meteor falls within the period November 11–15, which is one of the large-meteor periods, according to the useful summary given in Whitaker's Almanac.

The interval of four minutes between the appearance of the meteor and the sound of its explosion corresponds to a distance of forty-eight miles. I am sorry that the details are at present incomplete in many respects, but inquiry will be made.

MAXWELL HALL.

12 Hartington Road, Ealing, February 2.

MEMORIAL TO G. S. OHM.

A MEETING was held on Thursday afternoon, January 31, in the meeting-room of the Royal Society, the Right Hon. Lord Rayleigh, Sec.R.S., in the chair, for the purpose of promoting the co-operation of English men of science and others in a project, set afoot in the first instance by some of the Professors and other leading men in Munich, of erecting in that city a statue of George Simon Ohm—a man who, although he discovered no new phenomena of very striking importance, yet by the accuracy of his thought, and the clearness of his insight into the true bearings of physical facts, was able to lay one of the principal and firmest parts of the foundation of the noble edifice of modern physics.

The occasion for the proposal at this particular time to erect a memorial to Ohm is the near approach of the hundredth anniversary of his birth, on March 16, 1789. There are, moreover, reasons why this proposal should be, and no doubt will be, taken up warmly in this country. English physicists may recall with satisfaction that the award of the Copley Medal by the Royal Society on November 30, 1841, was the first public or official recognition that Ohm received of the value of his work upon the laws of the electric circuit, and that this award contributed in a very great degree to obtain for his researches the attention and appreciation they deserved. It may not be without interest at the present time to refer to the words in which the Chairman, Sir J. W. Lubbock, Bart, V.P. and Treas., announced the award. The following is from the report of the proceedings at the anniversary meeting of 1841:—

"The Council has awarded the Copley Medal for the present year to Dr. G. S. Ohm, of Nuremberg, for his researches into the laws of electric currents, contained in various memoirs published in *Schweigger's Journal*, *Poggendorff's Annalen*, and also in a separate work, entitled 'Die galvanische Kette mathematisch bearbeitet,' published at Berlin in the year 1827. In these works, Dr. Ohm has established, for the first time, the laws of the electric circuit; a subject of vast importance, and hitherto involved in the greatest uncertainty. He has shown that the usual vague distinctions of intensity and quantity have no foundation, and that all the explanations derived from these considerations are utterly erroneous. He has demonstrated, both theoretically and experimentally, that the action of a circuit is equal to the sum of the electromotive forces divided by the sum of the resistances; and that whatever be the nature of the current, whether voltaic or thermo-electric, if this quotient be equal, the effect is the same. He has also shown the means of determining with accuracy the values of the separate resistances and electromotive forces in the circuit. The light which these investigations have thrown on the theory of current electricity is very considerable; and although the labours of Ohm were for more than ten years neglected (Fechner being the only author who, within that time, admitted and confirmed his views), within the last five years, Gauss, Lenz, Jacobi, Poggendorff, Henry, and many other eminent philosophers, have acknowledged the great value of his researches, and their obligations to him in conducting their own investigations. Had the works of Ohm been earlier known, and their value recognized, the industry of experimentalists would have been better rewarded. In this country those who have had most experience in researches in which voltaic agency is concerned, have borne the strongest testimony to the assistance they have derived from this source, and to the invariable accuracy with which the observed phenomena have corresponded with the theory of Ohm. This accordance it may be observed is altogether independent of the particular hypothesis which may be adopted as to the origin of electromotive force; and obtains equally, whether that force is regarded as being derived from the contact of dissimilar metals, or as referable to chemical agency."

Ohm's book, "Die galvanische Kette," referred to in the above extract, was translated into English by Dr. William Francis, and published in 1841, in the second volume of "Taylor's Scientific Memoirs." The publication of Wheatstone's paper (read to the Royal Society, June 15, 1843), entitled "An Account of several New Instruments and Processes for determining the Constants of a Voltaic Circuit," also contributed in an important degree to attract attention to Ohm's work and to cause its importance to be recognized. We may call to mind also that it was in this country that the necessity of expressing electrical quantities in absolute measure first came to be generally recognized, and that the term "ohmad" or "ohm," suggested by Sir Charles Bright and Mr. Latimer Clark at the meeting of the British Association in Manchester, in 1861, first came into use as the name of a decimal multiple of the absolute unit of resistance convenient for practical purposes. Twenty years later, at the Congress of Electricians in Paris, in 1881, the "ohm" was unanimously adopted as an international standard. The name of the modest German Professor has thus come to be an understood term in the language of every civilized community in connection with the conception which he

was the first to define with perfect clearness, and to show the true bearing of in relation to the connected ideas of electromotive force and strength of current.

At the meeting on January 31, resolutions, moved by the President of the Royal Society and by Sir Frederick Abel, K.C.B., were adopted, expressing the concurrence of those present with the proposal to erect a statue to Ohm, and appointing a Committee to make the scheme known in this country and to collect subscriptions. Dr. Hugo Müller, F.R.S. (who, when a student at the University of Munich, was a pupil of Ohm's), was requested to act as Treasurer of the fund to be collected, and Profs. G. Carey Foster, F.R.S., and John Perry, F.R.S., were appointed Secretaries.

The following memoranda, taken from Lamont's *Denkrede* to the Munich Academy, 1855, may not be without interest at the present time:—Ohm was born in Erlangen, where his family had been settled for several generations. His father, who followed the hereditary trade of lock-smith, was a man of active intellect, and gained a very considerable acquaintance with mathematics and physics. It was in great measure owing to his example and encouragement that his two sons, George Simon and Martin (who afterwards attained great distinction as Professor of Mathematics in the University of Berlin), developed a love for similar studies. In 1805, G. S. Ohm became a student of the University of Erlangen, whither he returned in 1811, after some years spent as a private tutor in Switzerland, and then took his doctor's degree and established himself as *Privatdocent.* For a short time he was a teacher in the *Realschule* at Bamberg, and in 1817 obtained a more important post as teacher of mathematics in the Jesuits' Gymnasium at Cologne. It was while he held this appointment that his ideas as to the laws of the galvanic circuit took definite shape, and that his memorable book, "Die galvanische Kette mathematisch bearbeitet," was written. Soon after the publication of this book in 1827, Ohm presented himself at the Ministry of Education in Berlin, and there met with a reception so little appropriate to the whole-hearted and self-sacrificing devotion to science of which he was conscious, that he felt it impossible to remain any longer in the public service. He was thus driven to spend seven years in the prime of life in a state of deep mental dejection, and with very scanty means of subsistence. The end of this dismal period came in 1833, when he was appointed, by the Bavarian Government, Professor in the Polytechnic School at Nuremberg. The award of the Copley Medal, in 1841, already mentioned, cheered and encouraged him still further, and in grateful acknowledgment he dedicated to the Royal Society his "Molecular Physics." From this time he came to be recognized as one of the leading physicists of Germany, and "Ohm's law" soon found its way into every text-book of physics. In 1849, he was called to Munich as Curator of the Physical Cabinet, and in 1852 he became Professor of Experimental Physics in the University. On July 7, 1854, he died suddenly from apoplexy. For a great part of his life he had a hard fight with outward circumstances; but he seems to have remained throughout singularly simple-minded and unassuming, and at the same time thoroughly honest and conscientious in his work. G. C. F.

THE ROYAL SOCIETY OF EDINBURGH.[1]

At the commencement of the session 1883-84, the Royal Society of Edinburgh entered upon the second century of its existence. Since its foundation it has had among its members men whose fame is national and often world-wide—Joseph Black, Henry Dundas, James Hutton, John Playfair, Adam Smith, Dugald Stewart, Adam Fergusson, James Gregory, Henry Mackenzie, John Leslie, William Wallace, Walter Scott, Maclaurin, Brewster, Forbes, and more recently Clerk Maxwell; and at present it has members whose names will rank as high as these. In the year 1886 the membership of the Society was 507, and was rapidly increasing. The number of papers communicated to it in the period 1883-87 was 317. We shall therefore select for special notice a few of these, which may be taken as typical of the work done by the Society; and it will be seen that its work, if large in quantity, is also high in quality. We agree with the opinion expressed to the Society by the Chairman in his opening address in December 1886, that, "if we include the extra volumes on the Ben Nevis observations, and on the botany of Socotra, . . . the Proceedings and Transactions of the Society during the past three years probably surpass in bulk and importance those of any other Society in the United Kingdom for the same period."

In the department of mathematics, these volumes include valuable contributions to the science of situation, or of those space-relations which are independent of *measure* though not necessarily of *number*, from the Rev. T. P. Kirkman and Prof. Tait. The former writer contributes papers "On the Enumeration, Description, and Construction of Knots of fewer than Ten Crossings," and "On the 364 Unifilar Knots of Ten Crossings;" a note "On the Twists of Listing and Tait," and "Examples upon the reading of a Circle or Circles of a Knot." Prof. Tait gives a "Census of 8-fold and 9-fold Knottiness," and a "Census of 10-fold Knottiness," with a special treatment of amphicheirals. There is also a paper, "Ueber algebraische Knoten," by Prof. Fr. Meyer, of Tübingen.

Dr. Thomas Muir treats of subjects connected with the theory of continued fractions and with the theory of determinants. Dr. Muir constantly aims at the attainment of simplicity through great generalization. An example of this is given in his paper "On the Researches of M. de Jonquières on Periodic Continued Fractions." He points out that many of the theorems given by M. de Jonquières are not new, and that the earlier ones are all special cases of a more general theorem previously published by Dr. Muir himself. He then proceeds to use this general theorem for the purpose of giving unity to M. de Jonquière's work.

Among other papers we note, "The Expansion of Functions in terms of Linear, Cylindric, Spherical, and Allied Functions," by Mr. P. Alexander; a quaternion investigation by Dr. G. Plarr of "The curve on one of the co-ordinate planes which forms the outer limit of the position of the point of contact of an ellipsoid which always touches the three planes of reference;" and a note "On the Hessian," by Prof. Chrystal. M. Hermite contributes a paper "Sur la Réduction des Intégrales Hyperelliptiques," and Prof. L. Cremona gives an "Esempio del metodo di dédurre una superficie da una figura piana."

In a remarkable paper "On the Law of Inertia; the Principle of Chronometry; and the Principle of Absolute Clinural Rest, and of Absolute Rotation," Prof. James Thomson treats of questions on the border-ground between pure mathematics and physics. He discusses "such motions of points in unmarked space, as can have a reference frame relatively to which these motions are rectilinear and are changeless in mutual rate." The problem of finding a reference frame for a known set of such points is worked out in another paper by the same author by a method of mechanical adaptations, and Prof. Tait has given a quaternion solution of it. Prof. Thomson's law of inertia is the equivalent of Newton's first and second laws of motion. The paper is one which merits the perusal of all students of dynamics, and it may be specially recommended for study to certain classes of metaphysicians.

[1] Proceedings, Sessions 1883-87. Transactions, Vol. xxx. Part 4; Vol. xxxii. Parts 2, 3, 4; Vol. xxxiii. Parts 1, 2.

The fact that, in the kinetic theory of gases, Boltzmann's extension of Clerk Maxwell's theorem regarding the distribution of energy in a system of colliding spheres has led to results which are inconsistent with either the observed value of the ratio of the specific heats of a gas, or the complexity of the radiation spectra of gases, has induced Prof. Tait to investigate the question afresh from the very commencement. His results are contained in papers "On the Foundations of the Kinetic Theory of Gases," published in the Transactions. His great aim is the simplification of the mathematical treatment of the subject, and he has "purposely gone into very minute detail in order that no step taken, however slight, might have the chance of escaping criticism, or might have the appearance of an attempt to gloss over a difficulty." Abstracts of the papers have appeared in the *Philosophical Magazine*. The subjects treated in the first two papers are as follows: one set of equal spheres; mean free path among equal spheres; number of collisions per particle per second; Clerk Maxwell's theorem; rate of equalization of average energy per particle in two mixed systems; mean path in a mixture of two systems; pressure in a system of colliding particles; effect of external potential; pressure in a mixture of two sets of spheres; viscosity; thermal conductivity; diffusion. On the suggestion of Prof. Tait, Prof. W. Burnside applied a method used by the former in his first paper to the problem—Given a very great number of smooth elastic spheres, equal and like in all respects, whose centres of figure and centres of inertia do not coincide, and the sum of whose volumes is but a small fraction of the space in which they move, it is required to find the ultimate distribution of energy among the various degrees of freedom when by collisions the system has attained a "special state." Prof. Burnside's result is given in a paper "On the Partition of Energy between the Translatory and Rotational Motions of a Set of Non-Homogeneous Elastic Spheres." The various steps of the investigation are given in detail, and the assumptions are clearly specified. The conclusion is that the average energies of rotation of a sphere about each of the three principal axes are equal, and the whole average energy of rotation of a sphere is twice the average energy of translation.

Sir W. Thomson communicated sixteen papers to the Royal Society during the period under consideration. These include a remarkable series of investigations of various cases of fluid motion. Most of the papers have already appeared in the *Philosophical Magazine* or elsewhere; only four are given in detail in the Proceedings. These are "On a New Form of Portable Spring Balance for the Measurement of Terrestrial Gravity," "On the Front and Rear of a Free Procession of Waves in Deep Water," "On the Equilibrum of a Gas under its own Gravitation only," and "On Stability of Fluid Motion: Rectilinear Motion of Viscous Fluid between two Parallel Planes." From results obtained in the latter paper, Sir William considers it probable "that the steady motion is stable for any viscosity, however small; and that the practical unsteadiness pointed out by Stokes forty years ago, and so admirably investigated experimentally five or six years ago by Osborne Reynolds, is to be explained by limits of stability becoming narrower and narrower the smaller is the viscosity."

The following extract will give an idea of the scope of a paper by Lord Rayleigh, "On the Colours of Thin Plates":—"The theory set forth so completely in our treatises tells us indeed how the composition of the light reflected depends upon the thickness of the plate, but what will be its colour cannot, in most cases, be foretold without information of an entirely different kind, dealing with the chromatic relations of the spectral colours themselves. This part of the subject belongs to physiological optics, as depending upon the special properties of the eye. The first attempt to deal with it is due to Newton, who invented the chromatic diagram, but his representation of the spectrum is arbitrary, and but a rough approximation to the truth. It is to Maxwell that we owe the first systematic examination of the chromatic relations of the spectrum, and his results give the means of predicting the colour of any mixed light of known composition. Almost from the time of first reading Maxwell's splendid memoir, I have had the wish to undertake the task of calculation from his data the entire series of colours of thin plates, and of exhibiting them on Newton's diagram. The results are here presented, and it is hoped may interest many who feel the fascination of the subject, and will be pleased to see a more complete theory of this celebrated series of colours."

A little note by the (then) Astronomer-Royal for Scotland on Brewster's line Y in the infra-red of the solar spectrum is illustrated by an exceedingly instructive plate. The line Y is one of three discovered by Brewster in a portion of the spectrum usually invisible. As the Astronomer-Royal well remarks, "It was Brewster's eye that looked; so no wonder that he saw with it more than any of his predecessors, and most of his successors as well." Prof. Smyth proves that Y is a true solar line, being more distinct with a high than with a low sun. In the Royal (London) Society's publications the opposite had been asserted; and ultimately, though perhaps unintentionally, the line was altogether omitted from the spectrum—or, rather, it was misnamed Z. It is to be hoped that Prof. Smyth's paper will have the effect of removing all the ambiguity, and the consequent confusion, on this point. In this note, short as it is, we have evidence of the unlooked-for rewards which often await the scientific observer. While seeking for direct confirmation of M. Henri Becquerel's discovery that the line Y is due to sodium, Prof. Smyth discovered three new air-lines much farther in the infra-red than any previously discovered.

Prof. Piazzi-Smyth has also a paper on "Micrometrical Measures of Gaseous Spectra under High Dispersion." The paper is accompanied by maps of the spectra investigated on the scale of 40 feet to the visible spectrum. The prism arrangement gave a dispersion of 60° from A to H, and the magnifying powers of the telescope varied from 12 to 36 with a further magnifying power of 5 in the recording apparatus. There was thus in effect a possible dispersion of 9000°. The gases dealt with are CH, CO, H, O, and N. The peculiar arrangement of the leading lines (usually two) and train of linelets in each of the five bands of the CH spectrum is fully shown in the diagram and described in the text. The low-temperature (simple spark) spectrum of hydrogen is given, over 1600 lines being recorded. Prof. Smyth has found that three of the four principal oxygen lines are triple, and he has discovered three other such triplets. The remarkably methodical spacing of the lines in these triplets and of the triplets themselves is noted; but a more striking example of regular spacing is furnished by the lines in the green CO band. The map of this band is given on the scale of 120 feet to the visible spectrum. Prof. A. S. Herschel has made out two series of lines (single and double respectively) following the same law of arithmetical progression. The full interpretation of such "natural writing" may possibly never be obtained until we know the nature of molecules and atoms; but, on the other hand, a further investigation of spectra such as we have here may throw some light on molecular and atomic structure. So magnificent are the maps that accompany Prof. Smyth's paper, that one is rather inclined to speak of his contribution to the Transactions as a series of maps of spectra with explanatory text. But besides explanations the paper contains a full record of the experiments, and also tells a tale of high war—the combatants being the author on the one hand, and the London Royal Society, the London Royal Astronomical Society, *et hoc genus omne*, on the other. Vacuum-tubes containing nominally CO, but also

showing exceptionally strong CH bands, were "by a London maker." Prof. Smyth once admitted (though with great reserve), with "the English spectroscopists," that the CO spectrum might be that of pure carbon; he "begs now to apologize for that error;" and, long after he has come off victorious throughout the whole line, the rumble of distant thunder is heard in his "concluding notes."

Mr. John Aitken's investigations regarding the formation of small clear spaces in dusty air are already known to the scientific public, from the abstract published in NATURE, vol. xxix. p. 322 (January 1884). His paper on dew, a short abstract of which appeared in NATURE, vol. xxxiii. p. 256 (January 1886), is also contained in the Transactions. Some observations in addition to those indicated in NATURE are described. These refer to evaporation from extremely dry soils in Britain and France, and also, on the evidence of travellers, in Australia and South Africa. Additional evidence regarding the formation of the dewdrop is also given.

Mr. A. Crichton Mitchell has repeated Forbes's and Tait's experiments on the thermal conductivity of iron, copper, and German silver. His observations were conducted under improved experimental conditions, and the methods of calculation were different in some respects. These differences are pointed out and explained by Prof. Tait in an introduction to Mr. Mitchell's paper. On the whole, Mr. Mitchell's work confirms that of previous experimenters. One of the most important of his conclusions is that iron forms no exception to the rule that the thermal conductivity of ordinary metals increases with rise of temperature.

Prof. C. G. Knott, of Tokio University, Japan, gives a full experimental investigation of the thermo-electric peculiarities and the electrical resistance of hydrogenized palladium. In another paper he treats of the electrical resistance of nickel at high temperatures, and concludes from his results that "there is a strong presumption that the Thomson effect in metals has a close connection with the mutual relations of resistance and temperature."

From a series of observations made in atmospheric electricity at the top of Dodabetta, the highest hill in the Neilgherries, Prof. C. Michie Smith is led to the conclusion that on the edge of a dissolving mist the potential is lower than the normal, while in a condensing mist it is higher than the normal. He says: "If my results are confirmed by more extended observations, strong support will be given to the theory which looks on the condensation of a number of slightly charged particles into larger drops as the cause of the high potential indicated by disruptive discharges." In connection with this, results obtained by Mr. H. N. Dickson regarding the direction of earth-currents at Ben Nevis are worthy of note. When mist descends on the mountain, or rain (or snow) falls, a down current is observed in the telegraph cable when put to earth at both ends; but when the mist rises from the mountain-top the direction of the earth-current is upwards.

Von Helmholtz contributes an account of galvanic currents passing through a very thin stratum of an electrolyte.

Many of the facts brought out in a series of papers by Dr. H. R. Mill and others on various physical and chemical conditions of tidal estuaries should be of much use because of their evident bearing on the distribution of various forms of animal life, and on questions connected with meteorology.

A number of years ago Prof. Tait undertook the work of determining the pressure-errors of the *Challenger* thermometers. This investigtion gave rise to many others: such as the lowering of the maximum density-point of water by pressure; the variation of the compressibility of water with temperature, pressure, and amount of salt dissolved; and the question of the internal pressure in water. The various results obtained by Prof. Tait are contained in a series of notes scattered throughout the Proceedings.

The complete series of observations made at the Ben Nevis Observatory have been handed over to the Royal Society of Edinburgh for publication; and, from time to time, valuable meteorological information in connection with the Observatory is communicated to the Society and appears in their Proceedings. Dr. Murray remarks that "the refusal of assistance (to the Observatory) by the London Committees may be partly due to the fact that there are many claims on the funds which they administer, but it appears also to be very largely due to a want of proper knowledge of what has been done, and what may be reasonably expected to be done, by the Observatory, there being no Observatory in these islands that can compete with the Ben Nevis Observatory for the accuracy and intrinsic value of the hourly observations; and absolutely no pair of stations anywhere in the world that can be named alongside the Observatory and the station at Fort William as contributing data in furtherance of our knowledge of storms and the science of weather generally."

These volumes contain two or three papers in the department of engineering. Mr. A. C. Elliot gives a new formula for the pressure of earth against a retaining wall, which is an improvement on Rankine's formula. In a paper on "Cases of Instability in Open Structures," Dr. E. Sang discusses a class of theorems of which one previously enunciated by him—to the effect that any symmetric structure built on a rectangular basis, having no redundant parts, and depending on longitudinal strain alone, is necessarily unstable—was a particular example.

In the department of chemistry we note an elaborate paper by Prof. Dittmar and Mr. John McArthur, entitled "Critical Experiments on the Chloro-platinate Method for the Determination of Potassium, Rubidium, and Ammonium; and a Redetermination of the Atomic Weight of Platinum." The paper consists of five parts—two detailing experiments on the composition of chloro-platinate of potassium, a part on Finkener's and Tatlock's methods of potash determination, a part descriptive of experiments on chloro-platinate of rubidium, and another describing experiments on chloro-platinate of ammonium. The authors conclude that the atomic weight of platinum is very nearly 195·5.

Prof. Dittmar also gives, in conjuction with Mr. C. A. Fawsitt, a "Contribution to our Knowledge of the Physical Properties of Methyl-alcohol." The "vapour-tension" (why not *pressure* since it *is* pressure?) is investigated under varying conditions, and the specific gravity of aqueous methyl-alcohol for all the integral percentages is also tabulated at 0° C. and 15°·56 C.

Dr. John Waddell has determined the atomic weight of tungsten by an entirely new method, and has obtained results confirmatory of the commonly accepted value.

Comparatively few papers dealing with botanical questions were communicated to the Society during the period under consideration. A note "On the Structure of the Pitcher in the Seedling of *Nepenthes*, as compared with that in the Adult Plant," in which the late Prof. Alexander Dickson first called attention to the peculiar large marginal glands of *Nepenthes*, is of much interest. The Proceedings contain the fourth part of "Diagnoses Plantarum Novarum Phanerogamarum Socotrensium, &c.," by Prof. Bayley Balfour, and a note "On Degenerated Specimens of *Tulipa sylvestris*, by Mrs. A. B. Griffiths. Mr. John Rattray contributes a note on the marine plant *Ectocarpus*.

The number of papers in zoology and allied sciences is somewhat large.

In the summer of 1868, H.M.S. *Lightning* explored the region of the North Atlantic lying between the Hebrides and the Faroes. In 1869 H.M.S. *Porcupine* made three

cruises, the first off the north-west and the west coasts of Ireland, the second off the south and south-west coasts of Ireland, and the third off the north of Scotland as far as the Faroes. In 1870, the *Porcupine* dredged down the west coasts of France and Spain and in the neighbourhood of Gibraltar Strait, and explored the African coast of the Mediterranean as far east as Sicily. Prof. W. A. Herdman contributes to the Transactions the Report upon the *Tunicata* dredged during the cruises of H.M.SS. *Porcupine* and *Lightning* in the summers of 1868, 1869, and 1870. The Simple Ascidians alone are treated of. The Report on the *Pennatulidæ* dredged by the *Porcupine*, is by Prof. Milnes Marshall and Dr. G. H. Fowler. One new genus and one new variety were obtained. Dr. P. H. Carpenter writes "On the *Crinoidea* of the North Atlantic between Gibraltar and the Faroe Islands," and some notes are added by Prof. L. von Graff on the *Myzostomida*. The Report on the *Ophiuroidea* of the Faroe Channel, mainly collected by H.M.S. *Triton* in 1882, is drawn up by Mr. W. E. Hoyle. Mr. Hoyle also gives the second part (on the *Decapoda*) of a preliminary Report on the Cephalopoda collected by H.M.S. *Challenger*.

Mr. J. T. Cunningham (then of the Scottish Marine Station) writes on the "Eggs and Larvæ of Teleosteans;" on the "Reproductive Organs of *Bdellostoma*, and a Teleostean Ovum from the West Coast of Africa;" on *Stichocotyle nephropis*, a new Trematode, found as a parasite in the Norway lobster; and, along with Mr. Rupert Vallentin, on the "Luminous Organs of *Nyctiphanes norvegica*." Mr. George Brook discusses "The Formation of the Germinal Layers in Teleostei." Mr. Harvey Gibson gives a detailed account of the anatomy of *Patella vulgata*, no systematic account having been previously given, though separate accounts of various organs have appeared. Mr. Frank E. Beddard writes "On the Minute Structure of the Eye in certain *Cymothoidæ*;" "On the Structural Characters of certain new or little known Earthworms," five apparently new species and possibly a new genus being described; and "On the Reproductive Organs of the Genus *Eudrilus*." Mr. J. Arthur Thomson describes the structure of *Suberites domuncula*, a sponge found covering the outside of a sea-snail shell inhabited by a hermit-crab.

In geology some important papers appear. Dr. Traquair contributes the first part of a Report on fossil fishes collected in Eskdale and Liddesdale (*Ganoidei*). Mr. R. Kidston gives the first two parts of an account of the fossil flora of the Radstock series of the Somerset and Bristol coal-field (Upper Coal Measures). A note is appended on the fossil flora of the Farrington, New Rock, and Vobster series, and a table is given comparing the flora of the Radstock series with that of other coal-fields. Mr. Kidston also discusses the fructification of some ferns from the Carboniferous formation. Prof. Geikie writes on the geology and petrology of St. Abb's Head. The final Report of the Boulder Committee of the Society is contained in the Proceedings.

The plates in Vol. xxx. accompanying a paper by Dr. Traquair on fossil fishes are of great artistic merit. Indeed, the illustrations which are contained in the Proceedings and Transactions are probably unsurpassed by those published by any other similar Society.

Observations by Dr. H. B. Guppy, of H.M.S. *Lark*, on coral reefs and calcareous formations of the Solomon Group, appear in both publications. He is led to the conclusions: (1) that these upraised reef-masses, whether atoll, barrier-reef, or fringing-reef, were formed in a region of elevation; (2) that such upraised reefs are of moderate thickness, their virtual measurement not exceeding the limit of the depth of the reef-coral zone; (3) that these upraised reef-masses in the majority of islands rest on a partially consolidated deposit which possesses characters of the "volcanic muds" which were found during the *Challenger* expedition, to be at present forming around volcanic islands; (4) that this deposit envelops anciently submerged volcanic peaks. The bearing of the two latter conclusions on Dr. Murray's theory of the formation of coral islands is important.

We conclude with another quotation from the address already referred to:—"With respect to Scotland, the only grant for scientific purposes in aid of learned Societies is £300 annually to the Royal Society of Edinburgh, which is repaid to a department of the Government in the form of rent. One might well ask what Scotland had done that her learned Societies and scientific men should be treated so niggardly as compared with those in England and Ireland. It cannot be because she does no scientific work. It is sometimes said, indeed, that in literary matters Scotland, and especially Edinburgh, is a mere shadow of her former self; but in science this is not the case, and it is towards scientific matters that the great ploughshare of human thought and activity is, in this age, directed. I question if any country in the world, taking into consideration its size, can show a better record of scientific work, or a more excellent volume of scientific literature than Scotland, during the past ten or twenty years."

TIME.

TIME is one of the very numerous subjects which seem to be perfectly simple until we begin to think about them; then difficulties crop up in all directions, and afford a favourite battle-ground to philosophers.

Newton, avoiding metaphysical difficulties, gave an account of time which suffices for all the purposes of the mathematician and experimentalist. "Absolute, true, and mathematical time of itself and from its own nature flows equably and without regard to anything external, and, by another name, is called duration; relative, apparent, and common time is some sensible and external measure of duration by means of motion."

The word time is here used to express two distinct ideas, for the former of which it would be better to reserve the term duration. This double meaning of the word has caused much controversy between idealists and materialists, which is still far from arriving at any definite result. Thus, Whewell writes ("Hist. Ind. Sci.," 131):—"Time is not a notion obtained by experience." "Time is a necessary condition in the presentation of all occurrences to our minds. We cannot conceive this condition to be taken away. We can conceive time to go on while nothing happens in it, but we cannot conceive anything to happen while time does not go on."

It has always seemed to me that philosophers are rather hard on the intellect of their fellow-mortals in laying down so absolutely, as they are fond of doing, what can and what cannot be conceived by the mind, when they are in reality arguing from a single instance—their own. Many persons would be tempted to say that the idea of the fourth dimension of space is inconceivable, did not men of more powerful intellect assure them that their crude ideas on the subject are quite erroneous. I can myself find no impossibility in the conception of a universe composed of a homogeneous mixture of gases, to which the ordinary conception of time does not apply. If I err, I I at least do so in good company. Lucretius writes (i. 460):—

> "Tempus item per se non est, sed rebus ab ipsis
> Consequitur sensus."

And, in 1690, Locke gave the following luminous exposition of this difficult matter ("Hum. Und.," xiv.):—"A man having, from reflection on the succession and number of his own thoughts, got the notion or idea of duration, he can apply that notion to things which exist while

he does not think; as he that has got the idea of extension from bodies by his sight or touch can apply it to distances where no body is seen or felt." "Thus, a man when he is asleep, or when his mind is entirely occupied by one subject, has no notion of the passage of time." "This consideration of duration, as set out by certain periods and marked by certain measures or epochs, is that, I think, which most properly we call time."

According to Locke, then, duration is measured out, as it were, into time by changes, and as these changes are, so far as we know, due to motion, the ideas of time and motion are closely connected. These views have been further developed by Herbert Spencer ("First Principles," 163, 167, 171):—"The relation of sequence is given in every change of consciousness." "The abstract of all sequences is time. The abstract of all co-existences is space." "The conception of motion, as presented or represented in the developed consciousness, involves the conception of space, of time, of matter—a something that moves; a series of positions occupied in succession; and a group of co-existent positions united in thought with the successive ones." "These modes of cohesion, under which manifestations are invariably presented, and therefore invariably represented, we call, when contemplated apart, space and time; and when contemplated along with the manifestations themselves, matter and motion."

The abstract idea of duration without beginning or end is of the greatest value to the mathematician, but, so far as we know, it has no representative in Nature. It would, of course, be measured out by the equal spaces passed over by a body moving under the action of no forces, but no known body is in such a condition. As possible instruments for the accurate measurement of time, Thomson and Tait suggest a spring vibrating *in vacuo*, and Clerk Maxwell the period of vibration of light-waves of definite length. From this conception of duration or equable flow, Newton deduced his method of fluxions, which, owing to his delay in publishing the method, occasioned the lamentable controversy as to priority with Leibnitz. Though the manuscript of Newton's first paper on fluxions has been found with the date May 20, 1665, it was only communicated in a letter to Collins in 1672, used in some papers on motion read before the Royal Society in 1683, and printed in 1684. The method was first definitely published to the world in the "Principia," in 1687. According to Maclaurin: "In the doctrine of fluxions, magnitudes are conceived to be generated by motion, and the velocity of the generating motion is the fluxion of the magnitude." Suppose a movable point, starting from a fixed point, A, describes a line A B, of length x, Newton represented by $\dot{x}$ the velocity with which the line is described. Again, the velocity itself may not remain constant, but either increase, as when a stone falls, or decrease, as when a shot is fired. This change of velocity, now called acceleration, was expressed by $\ddot{x}$.

The conception of velocity is passing over a certain space in a certain time, as a mile in a minute or 88 feet in a second, and we may conceive both space and time to become infinitesimally small, so that the ratio of the one to the other becomes a fluxion. Acceleration is measured by the number of units of velocity gained or lost in the unit of time; thus, the acceleration due to gravity is a velocity of 32 feet per second gained or lost in a second.

The discussion of the connection between this conception of fluxions and the various methods of conceiving space as made up of infinitesimal portions, which were used more or less imperfectly by various mathematicians, until they were generalized and systematized by Leibnitz into the differential calculus (1675), would occupy too much space. A fluxion or differential, as was clearly pointed out by Newton, depends upon the philosophical conception of a limit, the foundation of so many of the higher branches of mathematics.

Important as are these theoretical questions deduced from the idea of duration, the practical questions of time and the means of measuring it with accuracy are far more so.

Since astronomers have been unable to find any truly equable motion by which to measure equal intervals of time, they make use of a fictitious sun, which apparently moves round the earth in the same period as the real sun does, alternately before and after it, but coinciding with it four times in the year—on April 15, June 14, August 31, and December 24.

The interval between two apparent passages of the fictitious sun over the meridian is a mean solar day, which is divided into 24 hours, 1440 minutes, or 86,400 seconds. The length of the tropical year, or the interval before the return of the sun to the same equinox, is 365·2422 mean solar days.

In the observatory, astronomers use as their unit the sidereal day, or the interval between two appearances of the same star on the meridian; owing to the apparent motion of the sun, there are 366·2422 sidereal days in the tropical year, or a mean solar day is equal to 1·0027379 sidereal days. About March 22 of each year, sidereal 0 hour coincides with mean noon, and for each day from that date the difference increases by 3 minutes 56 seconds. For purposes of calculation, astronomers make use of "the Julian period" of 7980 tropical years, of which 1889 is the 6602nd; and at mean noon on January 1, 2,411,004 mean solar days of the period had elapsed.

The oldest time-measurer, the sun-dial, dates from, at all events, 700 B.C. In its most simple form it consists of a style fixed parallel to the axis of the earth, and a graduated circle upon which the shadow falls. The clepsydra, or water-clock, in which time is measured by the equable flow of water, was introduced into Rome about 150 B.C.; and various methods of indicating the quantity of water which had flowed out by bells, hands, figures, &c., were subsequently added. A simple form of the instrument is still used in physical laboratories for measuring intervals of a few seconds. The replacement of water by sand furnished the hour-glass used by our ancestors for measuring out the eloquence of their preachers, as their more feeble descendants now use the three-minute glass for measuring the boiling of their eggs. A transit-instrument affords a ready means of correcting a clock; and mean-time signals are now sent from Greenwich to many places in England; hence in practice we individually measure only comparatively short intervals of time, correcting our private clocks and watches by public clocks regulated by time-signals.

Of all measures, those of time are most frequently and most accurately made. Public clocks are far more numerous than public standards of length or mass, and in 1880 the value of the clocks and watches imported amounted to £880,000. Few persons carry a foot-rule costing say 1*s*., but many a watch costing more than £2. Even among engineers but little attention is paid to lengths less than 1/64 of an inch; and few common balances indicate a difference of 1/100 of the load. But, according to Mr. Rigg (Cantor Lectures on Watch-making, 1881), a watch that does not vary more than half a second per diem, or 1/172800, is frequently met with, while an accuracy of two or three minutes per week, 3/10000, is attained even by cheap articles. It is no uncommon occurrence to meet with a chronometer which does not vary one-fifth of a second in twenty-four hours, or by about 1/432000 of the time measured out.

Almost all modern instruments for measuring time consist of three essential parts: (1) a motive-power, such as a falling weight, an uncoiling spring, an electric current; (2) a regulator, to render the motion steady, such as a pendulum, a balance-wheel, or a magnet; (3) some means

of indicating the space passed over, such as hands, bells, or marks on paper.

About the eleventh century the motive-power of a stream of water or sand was replaced by a falling weight; and in the early part of the sixteenth century, Peter Hele, of Nürnberg, substituted a coiled-up spring for the weight. As is often noticed when a foreign clock is wound up, the motive-power of such a spring varies very much as it uncoils. This difficulty was overcome in 1525 by the invention of the fusee, the increased leverage of which compensates for the decreased power of the partly uncoiled spring. In modern going-barrel watches reliance is placed on the careful adjustment of the regulating machinery; while in chronometers a very long spring is wound up so frequently that it never uncoils beyond a very small extent. In 1840, Wheatstone proposed a method of conveying the motion of a standard clock to several others by a current of electricity, and the electric current has since been used both as a motive power and as a regulator.

But little is known about the early methods of regulating clocks and watches, but, according to Shakespeare, the result does not seem to have been satisfactory, though some may consider his testimony invalidated by the accompanying libel ("Love's Labour Lost," iii. 1, 191). Biron speaks:

> "What I, I love, I sue, I seek a wife!
> A woman, that is like a German clock,
> Still a-repairing, ever out of frame,
> And never going aright, being a watch,
> But being watched, that it may still go right."

The use of clocks in observatories (1500), and for finding longitudes at sea (1530), caused a demand for better instruments which was only slowly met.

Galileo is said to have discovered the isochronism of the pendulum before about 1590, by observing a lamp swinging in the Cathedral at Pisa, but the discovery, though used by him, was not published until 1639, and it is doubtful if he applied it to clocks. In 1673, Huyghens proved the isochronism of the cycloidal pendulum, and showed that a pendulum could be caused to vibrate in a cycloid by making the upper portion of the suspending arrangement of steel springs or silk fibres, which wrap round cycloidal cheeks. The cycloidal cheeks are not found to answer in practice, but many makers use one or two parallel steel springs, which causes the bob to describe a curve which falls within the circle, and adds a positive and negative accelerating force at the commencement and end of each swing.

The time, t, of one swing of a simple circular pendulum of length l, at a place where the acceleration due to gravity is g, is—

$$t=\pi\left\{1+\left(\frac{1}{2}\right)^2\frac{\text{vers }\theta}{2}+\left(\frac{1\cdot 3}{2\cdot 4}\right)^2\left(\frac{\text{vers }\theta}{2}\right)^2+\&\text{c.}\right\}\sqrt{\frac{l}{g}};$$

where θ is half the angle through which the swing passes. When θ is very small, vers θ vanishes, and the swing is isochronous. If $\theta = 2°$, the error is about 1/13333, or two seconds in three days. If $\theta = 8°$, vers $\theta = 0\cdot 00973$, and the second and third terms become 0·00122 and 0·000003 respectively, or the time of oscillation is about 1/833 longer than it would be if the arc were indefinitely small.

Increase of temperature causes l to become longer, and therefore the clock to go more slowly. This cause of error is minimized by making the rod of some substance, such as varnished pine, which expands but little, or compensated for by some device, such as Graham's mercurial pendulum (1722), Harrison's gridiron pendulum (1725), or Baily's astronomical pendulum, in which expansion away from the axis of suspension is neutralized by an equal expansion towards it, so that the effective length of the pendulum remains unaltered.

The spring balance-wheel, which consists essentially of a heavy horizontal wheel, to which an oscillating motion is given by a long fine hair-spring, was invented by Hooke in 1660, and perfected by Huyghens in 1674. The difficulty of expansion is got over by dividing the wheel into two semicircles, each attached by one end only to the diameter, and made of two strips of metal of different coefficients of expansion, so that each curves inwards to compensate for the expansion of the radius which carries it.

Extremely short intervals of time have to be accurately measured in various scientific and practical researches, such as those connected with the science of astronomy and the art of gunnery. Many forms of the chronograph used for this purpose are extremely complicated, but the principle on which they all act is simple. A cylinder covered with paper is driven round by clockwork, at the rate, say, of a turn per minute, and a point connected with a pendulum beating half-seconds divides the circumference into 120 equal spaces. Suppose that by pressing a key an electric current causes a pen to press against the paper. So long as the key is down a line is traced, and the length of it, measured by the half-second pricks, determines how long the key has been down. Usually the cylinder is also caused to move along its axis, so as to throw the two circles of pricks and lines into spirals. It is said that 1/1000 of a second can be estimated by this method.

The need of accurate measures of time has had great effect upon, if it did not absolutely originate, the science of astronomy, and in many of the most important physical laws time is either directly or indirectly a most important factor. Thus, Sir William Thomson has found that, by a long-continued stress, the elastic resilience of a body may diminish, and has proposed for this curious fact the name of elastic fatigue; Harcourt and Esson and other chemists have investigated the circumstances which cause the rate at which certain chemical changes take place to vary; Berthelot and Dixon have measured the velocity of propagation of explosion waves; the time taken for sensation to pass through nerve-fibre and for other physiological phenomena has been carefully studied.

In 1830, Lyell, following up the work of Smith, Hutton, Murchison, and Sedgwick, showed that the history of the earth is continuous, and was governed by the same laws in the past as it is now, and hence that the rates at which changes are now going on are measures of the rates at which they have gone on in the past. Great doubt was thus thrown on the current view that the world has only existed for about 6000 years. For suppose chalk is now being formed at the bottom of the Atlantic at the rate of one-fifth of an inch per annum, and that the chalk formations in England, which are known to be more than 3500 feet thick, were formed in the same way at about the same rate; the time required for the mere formation of this series of beds would be, not 6000 years, but more nearly 3500 × 12 × 5, or 210,000 years! And we must reckon, in addition, the time required to form all the other beds below and above the chalk and to bring them all into their present positions and conditions.

Advanced geologists, then, convinced by the arguments of Lyell, postulated a world history of many millions of years, but their results were ignored or ridiculed by those who had not taken the trouble to investigate the proofs upon which the theory rested. In 1859, the publication of the "Origin of Species" brought this, among many other questions, prominently before the public. The admirable style and careful manner in which facts and theories, old and original, were shown by Darwin to point to the great law of evolution as opposed to the theory of special creations, threw what were previously the arcana of science open to all, and caused the acrimonious discussion of the duration, not only of each living or extinct type, but of the world itself. The fiercest

conflict raged about the age of man, and evidence was gradually accumulated, which proved that for many thousand years the human type has been practically the same as it now is: the question then forced itself forward, How long would it take a simple cell to develop through various forms to the anthropoid apes and man? The answer was given in figures higher even than those required by the geologist.

On the other hand, the question of the possible age of the world in its present condition has been attacked by Sir William Thomson from the side of mathematical physics, and his results have been recalculated and extended by Profs. Tait and Darwin. Arguments based upon (1) the internal heat of the earth, (2) the retardation of the rotation of the earth due to tidal friction, (3) the temperature of the sun, seem to show that the earth has not continued under present conditions for more than from ten to a hundred millions of years; while the theory of evolution probably requires at least three hundred millions of years for even a comparatively brief portion of geological history. The two results, each supported by strong evidence, are at present in contradiction to each other.

Political economists have for some years past been gradually realizing the immense importance of time in all their theories and calculations. The various causes which accelerate and retard the rate of growth are most important questions, not only for agriculturists, but for the whole population, who are dependent for their subsistence upon the reproduction of plants and animals. As Malthus pointed out in 1798, the population of a civilized country increases in geometrical ratio, while the food-supply can only be increased by importation, by taking inferior land into cultivation, or by improved methods of production. The gradual advance of civilization tends to quicken the rate of increase of population, while it decreases the three palliatives; hence, at some time, a limit must be reached at which population will increase faster than the means of subsistence. The results of this condition of affairs have been most ably discussed by Mill, and will possibly, before long, be exemplified in England.

To an individual, all duration beyond comparatively few years is of no importance, but, to a country or corporation, the difference between a hundred years and perpetuity may be very great. Two instances of this distinction have recently caused some discussion. The services of a general or lawyer may be amply rewarded by the grant of an annuity for a hundred years or for three lives; while the burden of a "perpetual pension" is felt long after the services for which it is granted are forgotten, and too often after all who have any real claim upon, or connection with, the original recipient have passed away.

The old fiction of English law, that all the land of the country belongs to the Government, and that the holders of the land are in reality not owners, but tenants, has recently been brought into prominence by Mr. George and his followers. The tenure of land varies almost infinitely in different countries, and even in diffent parts of the same country, but two simple examples may serve to render the point at issue clear. In the United States the land in the Territories, speaking generally, belongs to the Government, and has been, and to some extent is being, sold to capitalists in large lots at "prairie value."

Suppose 1000 acres worth £1 an acre are sold outright, they would fetch £1000, but the present value of a lease for a hundred years, interest being reckoned at 4 per cent., is £980. So far as the capitalist is concerned, for all practical purposes, the land is as much his own in the second case as in the first, since any change would take place in the time of a descendant whom he has never seen, and a fair compensation might be arranged for any unexhausted improvements. But, from the point of view of the Government, the case is very different: they would receive for the lease only £20 less than the selling price, and at the end of the century the land, with its "unearned increment," would revert to them in the same or better condition than it originally was, with the exception of minerals, for which special arrangements by royalty or otherwise must be made, and the conditions of the tenancy could then be altered to meet any change of circumstances. Where, as is generally the case in England, the land has long ago passed out of the possession of the community, considerations of public faith rightly overpower all considerations of expediency, but even in this case the absolute sale of "Crown lands" or "commons" seems to be suicidal. That things are not perfect is no reason for making them worse.

SYDNEY LUPTON.

NOTES.

WE print to-day an article on the proposal that English men of science and others should co-operate in the movement for the erection of a statue of Ohm in Munich. The Committee appointed by the meeting at the Royal Society to make the scheme known in England, and to collect subscriptions, consists of the following members:—Sir F. Abel, Prof. D. Atkinson, Mr. Vernon Boys, Mr. Conrad Cooke, Profs. Ewing, Fitzgerald, Fleming, G. Carey Foster, Mr. Glazebrook, Prof. D. E. Hughes, Mr. Norman Lockyer, Dr. Hugo Müller, Prof. John Perry, Mr. W. H. Preece, Lord Rayleigh, Profs. Reinold, Rücker, Stokes (President of the Royal Society), Mr. Swinburne, Sir William Thomson, and Prof. S. P. Thompson. Lord Rayleigh was elected President.

THE manuscript of the Royal Society Catalogue of Scientific Papers for the decade 1874–83 is now ready for the press, but Her Majesty's Government have informed the President and Council that it is not their intention to undertake, as in the case of previous decades, the printing and publication of the work.

THOSE who knew Dr. O. J. Broch, either when he was Professor of Mathematics at Christiania, or when he was Minister of the Board of Trade in Norway, or more recently, when he acted as Director of the International Bureau of Weights and Measures at Paris, and all who had any opportunity of intercourse with him either in social or official life, will hear of his death with deep regret. Dr. Broch died at Sèvres on the 5th inst., at the age of seventy-one. It has been the especial duty of the Bureau, over which Dr. Broch presided from its creation after the Metric Convention of 1875, to construct new standards of the metre and kilogramme for the different countries, including Great Britain, which were parties to that Convention. At the time of his death all these standards had been constructed, after much patient investigation, and were only awaiting final approval at Sèvres, before their delivery this year to the several contracting States. Dr. Broch's work remains to us, not only in those standards of exact measurement which, with the assistance of the men of science attached to the Bureau, he so well designed and verified, but also in the various scientific contributions by which he advanced our knowledge, particularly those published annually by the Comité International des Poids et Mesures; and in the mathematical papers issued by the Academy of Sciences at Paris (Elliptic Functions, *Comptes rendus*, 1864, &c.). Dr. Broch was a corresponding member of the Paris Academy; he was also a member of the Academies of Sciences of Berlin and Copenhagen, and a high officer of the Legion of Honour of France, and of the Order of St. Olaf of Norway.

THE death is announced of M. G. Meninghini, who had been Professor of Geology at Pisa from 1849. He died, on January 29, at the age of seventy-eight.

THE monument to be placed over the grave of the late General Prjevalsky on the shores of Lake Issik-kul has received the final approval of the Czar. It was designed by the traveller's companion and friend, M. Bilderling. The *Invalide Russe* gives the following description of it:—"The monument represents a picturesque rock 28 feet high, on the top of which is perched a large eagle, emblem of strength, intrepidity, and intelligence. The eagle grasps in its talons a map of Central Asia, the arena of the scientific exploits of the deceased, and in its beak an olive-branch, symbol of the peaceful scientific conquests which Russia owes to Prjevalsky. On one of the sides of the rock is a large bronze cross, beneath which is the inscription, 'Nicholas Mikhailovitch Prjevalsky, born 29th of March, 1839, died 20th of October, 1888.' In the interior of the rock is cut a spiral staircase crowned with an enlarged copy of the medal struck by the Academy of Sciences in 1887 in honour of Prjevalsky, and showing the original inscription, 'To the first explorer of Nature in Central Asia.'"

AT the last meeting of the Royal Swedish Geographical Society the *Vega* Gold Medal—the highest honour at the disposal of the Society—was conferred upon Dr. Nansen. There are only five other recipients, viz. Nordenskiöld, Palander, Stanley, Prjevalsky, and Junker.

THE eighth annual meeting of the Sanitary Assurance Association was held at 5 Argyll Place, W., on Monday, the President, Sir Joseph Fayrer, F.R.S., in the chair. After the reading of the Report, the following resolution was adopted: "That Dr. R. Farquharson, M.P., be ask to introduce the Sanitary Registration of Buildings Bill on the opening of Parliament, and to take the necessary steps to obtain as early a day as possible for the second reading; that Sir W. Guyer Hunter, M.D., M.P., Sir Henry Roscoe, M.P., F.R.S., and Dr. Cameron, M.P., be asked to again join Dr. Farquharson in introducing the Bill; and that as soon as the Bill is printed and in the hands of the members of the House of Commons, the President of the Local Government Board be asked to receive a deputation in support of the Bill."

THE Sanitary Institute has made arrangements for a series of lectures and demonstrations for sanitary officers, specially adapted for candidates preparing for the Institute's examination for Inspectors of Nuisances. The lectures will be delivered on Tuesdays and Fridays at 8 p.m., beginning with a lecture on the general history, principles, and methods of hygiene, to be given by Dr. Benjamin Ward Richardson, F.R.S., on March 5.

A MASTERLY paper on the scientific work of the German poet Adelbert von Chamisso was read by Emil du Bois-Reymond before the Berlin Academy of Sciences on June 28, 1888. This paper has now been issued separately. A most interesting account is given of Chamisso's voyage round the world in the *Rurik*—a voyage which in some respects resembled that of Darwin in the *Beagle*, fifteen years later. Among the many subjects which attracted Chamisso's attention during the voyage was the construction of coral islands. In connection with this question, curiously enough, he often receives credit for an observation with which he had in reality nothing to do. Darwin, for instance, was under the impression that it was Chamisso who had noticed that "the larger kinds of coral, which form rocks measuring several fathoms in thickness, prefer the most violent surf." M. du Bois-Reymond shows clearly that this observation was made, not by Chamisso, but by his companion in the *Rurik*, Dr. Eschscholtz.

AMONGST the memoirs recently published by the Société Philomathique of Paris in celebration of the hundredth anniversary of its foundation, is one by M. A. Milne-Edwards, describing a very singular new species of marsupial mammal of the genus *Dactylopsila* from New Guinea. *Dactylopsila palpator*, as it is proposed that this species shall be named, is remarkable for the enormous length of the fourth digit of the fore-limb, which surpasses in its proportions even that of the celebrated third finger of the Aye-aye, and is more than an inch longer than the two adjoining fingers. This new marsupial makes a second addition to the accurate catalogue of this order of mammals recently prepared by Mr. Oldfield Thomas, another important species (which will probably turn out to be the type of a new family) being the extraordinary fossorial form from South Australia, of which the discovery was announced in our issue of October 18, 1888 (see NATURE, vol. xxxviii. p. 588).

ON Sunday night, about 10.40, a shock of earthquake was felt in many parts of East Lancashire. At Great Harwood the vibration was so distinct that the occupants of a bedroom saw the wardrobe rocking and feared it would fall. "At Heapey, near Chorley, where there are many geological faults," says the *Times*, "a villa residence seemed to be struck three times, as if an attempt were being made to turn it round, and afterwards it oscillated to east and west five or six times, as if settling down after a violent shaking. But this effect was unique, and in most cases only a slight tremor was observed." At Bolton "there was first a heavy shock, and then for a few seconds a tremor. Doors were banged up; light articles danced in the houses; people were lifted in their beds, and one man in the out districts, who was sitting in front of a fire, was thrown into the grate, burning his hands and face. The electric bells at the fire brigade station were rung, and the central telephone office was besieged with inquiries. It was at first thought there had been a terrible colliery explosion." At Greenhaugh, near Kirkham, the shock was heralded by "a noise travelling in a westerly direction, which was followed by two or three oscillations."

AN earthquake occurred at Klagenfurt, on January 27, at 10.49 p.m., and on the same day a slight shock was felt at Ala, in the South Tyrol. At San José (Costa Rica), the National Capitol, the Cathedral, the President's palace, and many houses, were destroyed by the earthquake on December 29 and 30, 1888; and at Alejuela, several people were killed, and much damage was done.

A MANILA paper gives an account of an eruption of the Mayon volcano, in the Philippine Islands, on December 15, 1888. Vast columns of ashes were seen to ascend from the crater, and in a very short space of time the darkness became so intense that, though it was midday, lights had to be used in every house. The inhabitants of Legaspi, Camalig, Ligao, Libog, and other surrounding districts, were quite panic-stricken. At the time when the mail left, no loss of life had been reported. The lava, in vast streams, was then pouring down the mountain.

IN the Report of the Meteorological Council for the year ending March 31, 1888, recently published, the Council regret the loss they have sustained by the resignation of Prof. Stokes, consequent upon his election to Parliament. His place has been filled by the appointment of Dr. A. Buchan, who is well known by his various researches in meteorology. The work of the Office during the year in question is discussed under—(1) Ocean Meteorology. In this branch active intercourse is kept up with the Royal and Mercantile Navies, and an appendix shows that a large amount of valuable observations is being collected from all parts of the ocean. The investigations into the synchronous weather of the North Atlantic, the barometrical pressure charts for the Atlantic, Pacific, and Indian Oceans, and Part 5 of the contributions to Arctic meteorology have been completed, and noticed severally in our columns. Among the discussions now in hand may be mentioned those for the Red Sea, the Aden cyclone of June 1885, current charts for the principal oceans, and cyclone tracks in the Southern Indian Ocean for the years 1848–86 from materials supplied by Dr. Meldrum. It is intended that the discussion of the weather for the region lying between the

Cape of Good Hope and New Zealand shall also be taken up. (2) Weather Telegraphy. The work in this branch of the Office continues to increase. Forecasts are prepared three times a day, at 11 a.m., 3.30 p.m., and 8.30 p.m. A comparison of the results of the latter during the year shows an average success of 84 per cent. over the whole United Kingdom, being 3 per cent. more than for the previous year. Hay harvest forecasts were also issued, the results showing considerable success. Storm warning notices were issued to 146 coast stations. Under this head may be specially noticed a discussion of the severe storms which visited the British Isles between August 1, 1882, and September 3, 1883. From this investigation Mr. Scott concludes that it is extremely improbable that telegraphic reports from America can assist in the forecasting of the weather on our coasts, and this conclusion is supported by the actual results of the experiment made in dealing with the American reports during the year. (3) Land Meteorology. The records received from the Observatories and stations are classed under five heads; the methods employed are fully explained, and the Report shows that several important researches are being carried on in this branch.

A CORRESPONDENT writes to us from Adelaide:—"I am glad to be able to tell you that the drought, which has been so bad over nearly the whole of Australia during the past year, has at last broken up. We have had splendid rains right across the continent; the north-west monsoon, aided by a barometric trough across the interior, and a low-pressure area on the south coast, penetrating south of the tropics, and bringing a deluge of rain and heavy thunderstorms over the whole of South Australia, Victoria, and New South Wales."

IN consequence of the severity of the weather in Russia, wolves have made their appearance in East Prussia, where they have not been seen during the last six years.

THE hydrate of amidogen, or hydrazine, $N_2H_4 . H_2O$, has been prepared by Drs. Curtius and Jay, of the University of Erlangen. It may be remembered that a brief announcement of the isolation by Dr. Curtius of gaseous amidogen itself, $\begin{matrix} NH_2 \\ | \\ NH_2 \end{matrix}$, was made in NATURE (vol. xxxvi. p. 185) nearly two years ago. The free gas appears, however, to possess such an intense affinity for water, that its isolation in any quantity appears almost impracticable; for in all the reactions yet known in which it is liberated, water is also of necessity a secondary product, and combines with the amidogen at the moment of its liberation, forming this interesting hydrate, which is a liquid, and has been obtained pure in large quantities. Hydrazine hydrochloride, or hydrochloride of amidogen, as it is variously called, $\begin{matrix} NH_2 . HCl \\ | \\ NH_2 . HCl \end{matrix}$, a salt which may be obtained from its aqueous solution in fine regular octahedra, was distilled with caustic lime from a silver retort. The tube of the retort was inclined upwards for some distance and then bent into a U-shape, so as to prevent the possibility of any projection of particles from the contents of the retort. To the end of the silver U-tube was attached a horizontal tube, also of silver, containing fragments of quicklime; this in turn passed into a receiver of glass. The reaction between hydrazine hydrochloride and quicklime is exactly analogous to that so well known in the preparation of ammonia, amidogen water and calcium chloride being formed; but instead of obtaining the free hydride, its compound with water distils over. After the distillation had been in progress a few minutes, the horizontal tube was gently warmed, when liquid drops of the hydrate began to fall into the receiver. Barium oxide behaves just like lime, but by far the largest yield of the liquid is obtained by use of a strong solution of potash. Hydrazine hydrate is a fuming liquid, of very high refractive index, boiling unchanged at 119° C. Although boiling so near the boiling-point of water, it may be almost perfectly separated from that liquid by fractional distillation. It attacks glass energetically, and rapidly destroys cork or caoutchouc. It is strongly alkaline, as expected, tastes somewhat like ammonia, and leaves a burning sensation upon the tongue. It forms well-crystallized salts with most acids, which are found extremely poisonous, being fatal to the lower animals. It is probably the strongest reducing agent known. The most easily reducible metals are precipitated from solutions of their salts in the cold. Silver separates from cold strong solutions in fine compact crystalline masses; from very dilute solutions in the form of perfect mirrors of great beauty. On warming with a neutral solution of platinic chloride, metallic platinum separates, according to the degree of concentration, in silver-white particles or shining mirrors. In acid solutions it quantitatively reduces ferric to ferrous, cupric to cuprous, and platinic to platinous salts, with evolution of nitrogen gas. For instance, $N_2H_4 . 2HCl + 2PtCl_4 = N_2 + 6HCl + 2PtCl_2$. Finally, when dropped upon mercuric oxide, it violently explodes. From these facts it will be seen that hydrazine hydrate is one of the most remarkable liquids yet discovered, and appears likely to be of great use in chemical operations.

THE Haileybury Natural Science Society has published the first part of a list of the "Fauna and Flora of Haileybury." The volume is interleaved, and should be of considerable service to students of natural science in the neighbourhood.

MESSRS. ALLEN AND CO. have just issued a new edition of "Practical Microscopy," by George E. Davis. The work has been enlarged, and, as nearly as possible, brought down to the present time. The author has extended its scope, so as to include an account not only of English instruments, but of the apparatus in general use upon the Continent and in the United States of America.

MESSRS. LONGMANS, GREEN, AND CO. have published "The Student's Atlas," by the late Mr. R. A. Proctor. It consists of twelve circular maps, on a uniform projection and one scale, with two index maps, and is "intended as a *vade-mecum* for the student of history, travel, geography, geology, and political economy."

THE Royal University of Ireland has issued in a separate volume the examination papers of 1888. The volume forms a supplement to the University Calendar for 1889.

WE understand that the Trustees of the Australian Museum, Sydney, have decided to publish the manuscript and drawings relating to the life-histories of Australian Lepidoptera left by the late Alexander Walker Scott, and since acquired by them, and that the work of editing and revising this material has been intrusted to his daughter, Mrs. Edward Forde, and Mr. A. Sidney Olliff.

THE French Government, immediately after the recent outbreak of yellow fever at Jacksonville, despatched Dr. Paul Gibier, a French physician, to study the causes of the outbreak. Dr. Gibier now appeals to the American Government, on the ground that his own Government will not spend any more money on the task. He merely asks for the payment of incidental expenses, and for the moral support of the United States.

AT a recent meeting of the Scientific Society of Christiania, Prof. H. A. Getz exhibited the tusk of a mammoth found in Vaage, in Central Norway. This is the first discovery of remains of this animal in Norway.

THE Woman's Anthropological Society, Washington, has entered upon the fifth year of its existence; and, according to *Science*, it displays "undiminished enthusiasm and vigour." Mrs. Sybil A. Carter (wife of the Hawaiian Minister) and Miss Florence Spofford act respectively as President and Secretary.

We learn from *Science* that Dr. Thomas Featherstonhaugh, a grandson of the famous pioneer geologist, has just returned from a visit to Florida, and has brought back an interesting collection of aboriginal remains. He thoroughly examined a mound of damp sand on the shore of Lake Apopka, about the geographical centre of the State, and farther south than any previous researches of the kind. The mound was 50 feet in diameter and 14 feet high, and was covered with a dense growth of palmetto and other trees. It was found to be full of fragmentary bones and pottery, so numerous that Dr. Featherstonhaugh estimates that there could have been no less than four hundred bodies deposited there. A few Venetian beads near the top indicated intrusive burials, but below 4 feet there were no evidences of any intercourse with whites. Four shapely hatchets were recovered, also a charm-stone, and numerous specimens of decorated pottery. The whole find was presented to Major Powell, and by him turned over to the National Museum.

All hope has now been abandoned of saving Prof. J. Mainwaring Brown, who occupied the Chairs of English Language and Literature and of Political Economy in the University of Otago, New Zealand. He was one of an exploring party, being accompanied by Mr. White and Major Goring, which set out on an expedition to the neighbourhood of Lake Manapouri. One morning, Mr. Brown left his tent for a stroll in the bush, and, shortly after, a terrific storm of snow, wind, and hail burst over the district, and lasted without intermission for three days. His companions made every effort to find him, but without avail. Large search-parties were formed, but no tidings were obtained of the unfortunate gentleman. An enterprising newspaper proprietor in Otago despatched a special search-party at his own cost, and the Government have sent a steamer to Smith Sound, in the hope of obtaining some intelligence.

At the Royal Institution, Dr. Sidney Martin will, on Thursday next (February 21), begin a course of four lectures on the venom of serpents, and allied poisons, including those used in the Middle Ages; and Lord Rayleigh will, on Saturday (February 23), begin a course of eight lectures on experimental optics (polarization, fluorescence, wave-theory, &c.). Mr. Harold Crichton-Browne will give a discourse on Friday evening (February 22), entitled, "In the Heart of the Atlas."

A correspondent writes to point out that the planet Venus is now visible before sunset even in London. A few days ago he saw it at 4.30 p.m., thirty minutes before sunset, and as it was then very distinct he has no doubt that he could have seen it earlier if he had looked for it.

Mr. Harold P. Brown and Mr. George Westinghouse, Jun., have had a public discussion on the respective merits of alternating and continuous electrical currents. Mr. Brown, apparently not having satisfied Mr. Westinghouse, issues the following challenge, which we take from one of the electrical papers:—"I challenge Mr. Westinghouse to meet me in the presence of competent electrical experts, and take through his body the alternating current, while I take through mine a continuous current. The alternating current must not have less than 300 alternations per second (as recommended by the Medico-Legal Society). We will begin with 100 volts, and will gradually increase the pressure 50 volts at a time, I leading with each increase, each contact to be made for five seconds, until either one or the other has cried enough, and publicly admits his error. I will warn Mr. Westinghouse, however, that 160 volts alternating current for five seconds has proved fatal in my experiments, and that several men have been killed by the low-tension Jablochkoff alternating current." In other words, says Mr. Harold Brown, "I invite you to have a current passed through your body which I know (though you do not) will kill you, and I invite competent electrical experts to be present at the death which is sure to ensue." Mr. Brown and his experts will make preparations for what they all know to be an experiment highly dangerous to life, and which some of them believe must be fatal to Mr. Westinghouse, and if the latter is oolish enough to agree to his opponent's idiotic proposition, and the result is what Mr. Brown says is certain, then Mr. Brown and his experts will find themselves lodged in gaol awaiting their trial for murder, and being accessories to murder. If Mr. Brown knew a pistol was loaded which Mr. Westinghouse declared was not, and then induced the latter to fire it into his head to test which was right, Mr. Brown and the gallows would run the risk of being acquainted, while the experts who aided and abetted him would have an opportunity of trying the effects of some years' penal servitude. This fustian "challenge" does not make Mr. Brown any more accurate than he was before, but it must make every man of common-sense pretty certain that he can be an excessively foolish person, and that the chances are, when Mr. Brown is particularly positive about anything, he is wrong.

The additions to the Zoological Society's Gardens during the past week include a Tropical Squirrel (*Sciurus æstuans*) from Bolivia, presented by Mr. Peter Suarez; four Marbled Polecats (*Putorius sarmaticus*) from India, presented by Colonel Sir Oliver B. C. St. John, K.C.S.I., R.E.; eight Indian Gerbilles (*Gerbillus indicus* 2 ♂ 6 ♀) from India, presented by Dr. J. Gilbert; a Jackdaw (*Corvus monedula*), British, presented by Mr. Basil Carter; an Areolated Tortoise (*Homopus areolatus*), seven Tuberculated Tortoises (*Homopus femoralis*), two well-marked Tortoises (*Homopus signatus*), a Robben Island Snake (*Coronella phocarum*), two Infernal Snakes (*Boodon infernalis*), two Aurora Snakes (*Lamprophis aurora*), a Many-spotted Snake (*Coronella multimaculatus*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; an Adorned Ceratophrys (*Ceratophrys ornata*) from Buenos Ayres, presented by Captain Hairley.

OUR ASTRONOMICAL COLUMN.

New Minor Planet.—M. Charlois discovered a new minor planet, No. 284, on February 8, at the Nice Observatory. M. Charlois has named No. 277 Elvira.

Observations of Variable Stars.—Mr. Paul Yendall publishes in *Gould's Astronomical Journal* the results of his observations of a few variable stars in 1888. The observations compare as follows with the ephemerides given week by week in Nature:

Star.	Observed.		Calculated.
R Ursæ Majoris...	Oct. 22 *M*	...	Nov. 7 *M*
R Scuti	Aug. 6 *m*	...	Aug. 15 *m*
η Aquilæ	June 12·98 *M*	...	June 12·88 *M*
	July 25·76 *M*	...	July 25·96 *M*
	Sept. 6·50 *M*	...	Sept. 7·00 *M*
S Sagittæ	Sept. 27·9 *m*	...	Sept. 27·9 *m*
	Sept. 30·5 *M*	...	Sept. 30·9 *M*

Winnecke's Periodical Comet.—An exceedingly valuable memoir on the motion of this comet has recently been published by Dr. von Haerdtl, *Privatdocent* for Astronomy in the University of Innspruck. The most interesting point of this memoir, which was communicated to the Imperial Academy of Sciences of Vienna, lies in the evidence it supplies that an increase is necessary in the accepted value for the mass of Jupiter. After referring to the early history of the comet, its probable identity with Comet 1766 II., and with that discovered by Pons on February 6, 1808, and its rediscovery by the same observer on July 18, 1819, Dr. von Haerdtl commences the detailed treatment of the observations made during the four last periods when it was seen—viz. 1858, 1869, 1875, and 1886, discussing the individual observations, some 462 in all, with great thoroughness, and forming normal places and computing the resulting

elements for each of the four periods of observation. Then follow the computations of the perturbations exercised by the different planets from Venus to Uranus, Mercury having no appreciable disturbing effect, for the comet at perihelion does not come far within the orbit of the earth, and remains well without that of Venus, its perihelion distance being 0·831. The perturbations exercised by Jupiter, however, are most important, for the aphelion of the comet does not lie far outside the orbit of that planet, and the two tend to come into proximity every eleven years, their aphelion distances being, respectively, 5·57 and 5·20, and their periods 2076·79 and 4332·59 days, so that the comet was only 0·87 distant from the planet in December 1870, and eleven years later, in November 1881, was only half as far from it. These perturbations were computed for intervals of twenty days through the whole period covered by the observations, including thus five revolutions; and where it seemed desirable, for every ten or even every five days. The reciprocal of the mass assumed for Jupiter was $\frac{1}{m}$ = 1047·54, and with this value, so far from finding an acceleration of the mean motion of the comet, as with Encke's comet, a retardation was displayed—a retardation which, however, disappeared when a somewhat higher value viz. 1047·1752, was substituted. It appears that this latter value satisfies the observations not only of the comet in question, but also those of Faye's and Encke's. The value obtained by Dr. Schur from the four satellites of Jupiter does not greatly differ from that now found by Dr. von Haerdtl, and the latter considers that the simple mean of the two, $\frac{1}{m}$ = 1047·204, may be adopted as the nearest approach to the true mass of Jupiter, *i.e.* of the Jovian system, the satellites being included.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *FEBRUARY* 17–23.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 17

Sun rises, 7h. 11m.; souths, 12h. 14m. 12·0s.; sets, 17h. 17m.: right asc. on meridian, 22h. 4·9m.; decl. 11° 48′ S. Sidereal Time at Sunset, 3h. 8m.

Moon (at Last Quarter on February 23, 0h.) rises, 17h. 59m.*; souths, 1h. 12m.; sets, 8h. 10m.: right asc. on meridian, 11h. 0·9m.; decl. 10° 42′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	6 37	11 49	17 1	21 39·7	10 6 S.
Venus.....	8 22	15 4	21 46	0 55·6	7 42 N.
Mars......	8 9	14 11	20 13	0 1·6	0 26 S.
Jupiter....	4 24	8 19	12 14	18 8·8	23 6 S.
Saturn....	15 47	23 22	6 57*	9 14·9	17 11 N.
Uranus...	22 8*	3 32	8 56	13 21·4	7 53 S.
Neptune..	10 16	17 59	1 42*	3 50·9	18 26 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb h.
18 ... 4 ... Venus at greatest elongation from the Sun, 47° east.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.
U Cephei	0 52·5	81 17 N.	Feb. 17,	19 9 *m*
			,, 22,	18 49 *m*
R Ceti	2 20·4	0 41 S.	,, 17,	*M*
λ Tauri	3 54·6	12 11 N.	,, 17,	18 32 *m*
R Canis Majoris ...	7 14·5	16 11 S.	,, 17,	2 39 *m*
		and at intervals of		27 16
U Monocerotis ...	7 25·5	9 33 S.	Feb. 20,	*M*
S Canis Minoris ...	7 26·7	8 33 N.	,, 22,	*m*
S Cancri	8 37·6	19 26 N.	,, 20,	19 22 *m*
U Hydræ	10 32·1	12 48 S.	,, 22,	*M*
R Hydræ	13 23·7	22 42 S.	,, 17,	*M*
R Lyræ	18 52·0	43 48 N.	,, 18,	*m*
U Cygni	20 16·2	47 33 N.	,, 20,	*M*
X Cygni	20 39·0	35 11 N.	,, 18,	2 0 *M*
δ Cephei	22 25·0	57 51 N.	,, 19,	0 0 *M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
From Canes Venatici ...	181	34 N.	February 20. Very swift; white.
Near τ Herculis	238	46 N.	February 17.
,, ρ Herculis	260	36 N.	February 20. Swift.

GEOGRAPHICAL NOTES.

A PAPER of more than usual interest was read at Monday's meeting of the Royal Geographical Society, by the Rev. W. Spotswood Green, on his explorations in the glacier regions of the Selkirk Range, British Columbia, in the summer of 1888. This range is generally included in the Rocky Mountains, although, as Mr. Green showed, it is in many respects distinct from them. After crossing the Rockies by the Canadian Pacific Railway, and plunging into the valley of the Columbia River, the Selkirk Range lies before the traveller. It has been but little explored, and some of its glaciers were probably visited for the first time by Mr. Green. The Selkirk Range is entirely bounded by the great bend of the Columbia and its tributary, the Kootenie, and the drainage of all its glaciers finds its way into the Columbia in some part or other of its course. Under many difficulties, owing to the densely forest-clad nature of the ground, the want of guides and porters, the necessity of opening up new routes, and other causes, Mr. Green visited some of the higher parts of the range, and explored, and in some cases named, its previously unvisited glaciers. After crossing the Rockies proper, curiously ridged prairie hills have to be passed, and all the ranges between these and the Columbia have a smooth rounded outline, forming a strong contrast to the ranges on the other side of the watershed. These latter form a complexity of glacier-clad ranges, many peaks rising quite as high as those on the watershed. Among the higher ranges an immense number of small glaciers lie in the hollows, and two extensive snow-fields are to be found within the limits of Mr. Green's map. One of these, being the source of the best-known glacier in the whole region, on account of its being so clearly visible from the railway, Mr. Green has called the great Illecellewaet firn, after the river of which it is the true source. This ice-field, probably 500 feet thick, to the southward extends down into a valley as the Geikie Glacier, and to the eastward, having been joined by ice-streams coming from the Dawson Range, it pours into Beaver Creek Valley as the Deville Glacier. All these glaciers show evidence of shrinking. An immense moraine exists in the valley below the Illecellewaet Glacier. Some of the blocks of quartzite in the moraine are of huge dimensions, one being 50 feet long, 24 feet thick, and 33 feet high. Mr. Green set up some poles at a little distance from the end of the glacier, and found that after thirteen days the ice had melted a vertical foot over its whole surface, and the centre of the glacier had moved 20 feet. The Geikie Glacier, about 4 miles long and 1000 yards wide, is a much more interesting ice-stream. Sheltered from the sun's rays by high cliffs, it flows along a level valley, so that one can walk across its lower portion in various directions without trouble. As it descends from the firn, it is much broken; then its surface becomes level, but with numerous transverse crevasses. Flowing round a bend, longitudinal fissures are set up, crossing the others, and forming such a multitude of *séracs* that the surface presents an appearance more like some basaltic formation with the columns pulled asunder than anything else I can think of. This beautiful structure gives place to the frozen waves of a *mer de glace*, and the glacier terminates in longitudinal and slightly radiating depressions and crevasses. The level of perpetual snow in these mountains may be put down at 7000 feet, and the upper limit of the forest at 6000 feet. Red snow, caused by the presence of *Protococcus nivalis*, is of frequent occurrence. Like most of the rest of British Columbia, the Selkirks are covered with forests, all the trees attaining huge dimensions. These forests are being devastated by fires, often caused by sparks from the engines on the new railway. Beneath the living trees, thousands of prostrate trunks lie piled in every conceivable position, and in every stage of decay. Exploration and mountaineering under such circumstances are attended with enormous difficulties. Above the forest region, the slopes of the mountains are as profusely covered with flowers as the "Alp" region of the Swiss mountains; the most conspicuous plant being the *Castilleia miniata*. The heaps of boulders above the forest region form a refuge for a great

variety of small animals. With regard to the geology of the Selkirks, earlier than the Glacial formation, no rocks later than the Palæozoic seem to be met with in the central range. In the higher ranges, greenish quartzites and micaceous schists are the commonest rocks. The summit of Mount Bonney and the southern and south-western *arêtes* of Mount Sir Donald consist of a beautiful white, smooth quartzite, speckled in the former case with deep brown spots, "probably iron or manganese oxides." Associated with these harder rocks are a number of remarkable silky-looking schists (*phylites* of Prof. Bonney), the result of great squeezing in the movements which upheaved the ranges. Roughly speaking, then, the configuration of this district, with its complexity of valleys, is due to the disintegration and denudation of the softer schists and the permanence of the harder quartzites in mountain-ridges. With regard to age, the rocks range from true Archæan to late Palæozoic, possibly a little later. The presence of very old schists and gneisses would seem, then, to show that though the range called the Rockies, on the Canadian Pacific Railway route, is the water-parting, the Selkirks are geologically the true continuation of the Rocky Mountains of Montana, and the backbone of the continent.

The Russian Geographical Society has received the following news from Captain Grombchevski, who was sent out to explore the Khanate of Kunjut in the highlands between India and Afghanistan, and for a time was supposed to have been lost. After having left Marghelan in Russian Turkistan, M. Grombchevski crossed the highlands of Alai, and, *viâ* the Pamir lakes, Great Kara-kul, and Rang-kul, he reached the sources of the Amu-daria (the Murghab). Thence he proceeded to the Ak-baital River, and on August 16 he crossed the high ridge on the frontier of Afghanistan. On the southern slope of this ridge the Expedition was overtaken by a violent snow-storm, during which M. Grombchevski's Cossacks succeeded in getting hold of two inhabitants of Turm, from whom they learned that the Expedition was surrounded by Afghan troops, who had been sent out to take them prisoners. In consequence, M. Grombchevski, notwithstanding the snow-storm which was still raging, crossed the mountains again and returned to the Pamir, whence he immediately went across the Hindu-kush through a mountain pass which leads to Kunjut. The journey was so difficult that the Expedition lost one-half of its horses and part of its luggage. Circumstances did not permit M. Grombchevski to stay at Kunjut. He re-crossed the Hindu-kush, and entered East Turkistan at the sources of the Raskem-daria, one of the affluents of the Yarkand River. He followed its course, hoping to reach Karakorum, but was soon compelled to abandon his scheme, and only explored the nephrite mines on the banks of the river. After having surveyed part of the Raskem and Yarkand Rivers, the Expedition returned to Little Kara-kul Lake on the Pamir, and reached Kashgar on November 13. Three weeks later they were at Osh, bringing in a mass of interesting information and numbers of photographic views of the explored region.

The last volume of the *Izvestia* of the Caucasus branch of the Russian Geographical Society contains a variety of interesting short articles and notes. V. Massalsky's sketches of the regions of Kars and Batum are especially valuable to botanists. M. Konshin gives a most interesting geological and geographical sketch of the Transcaspian region; and two obituary articles (with portraits) devoted to Abich and Von Koschkul contain excellent reviews of their work in the Caucasus. The appendix contains a note on the study of the Caucasian languages, and various papers relating to Persia, Asia Minor, and Afghanistan. The most important of the latter is a report on the work done by the Russian Commissioners of the Afghan Boundary Commission, with a map of the region (13 miles to the inch) brought up to date in 1888. A short paper on the economic conditions of the Russian Transcaspian dominions, and a condensed translation from a "Guide to Armenia," by Bishop Srvandziantz, are also worthy of notice.

ELECTRICAL NOTES.

Hallwachs (*Ann. Wied.*, vol. xxxiv. p. 731, 1888) is continuing his researches on the connection between light and electricity. He has found that if the light of an arc lamp falls on clean plates of zinc, brass, and aluminium, they are always charged positively, the zinc to a potential of over 1 volt, the brass to 1 volt, and the aluminium to 0·5 volt. The plates become fatigued by constant illumination.

Sir William Thomson gave the Friday evening lecture on February 8 at the Royal Institution, on "Electrostatic Measurement," and described voltmeters and their functions; but the most interesting part of his discourse was his approving and eulogistic reference to Hertz's work, his own measurement of "v," which brings it very close to 3×10^{10} centimetres per second, and his long-deferred conversion to Maxwell's electromagnetic theory of light, which he thought had sprung from Maxwell's inner consciousness.

Himstedt (*Ann. Wied.*, vol. xxxv. p. 126), using a condenser, has determined the value of v to be $3{\cdot}0093 \times 10^{10}$.

Nahrwold (*Ann. Wied.*, vol. xxxv. p. 107) has shown that platinum rendered incandescent in a closed space is electrified negatively, the air being positive, but the same effect is not to be obtained with hydrogen, or any other pure gas.

Magnetic elements, Parc Saint-Maur, Paris:—

	January 1, 1889.	1888.
Declination	15° 47′·4	− 4′·7
Dip	65° 15′·7	− 1′·0
H	0·19508	+ 0·00028
V	0·42275	+ 0·0003
T	0·46559	+ 0·00039

E. G. Acheson in New York (*Electrical World*, January 19) has repeated many of Prof. Oliver Lodge's experiments on the "alternative" path in discharging Leyden jars, but has deduced from them different conclusions. He has avoided the errors due to charging which vitiated Prof. Lodge's early experiments. This is done by using one jar instead of two, and separating the charging system entirely from the discharging. He shows that the effects are due entirely to "extra currents" in the alternative wire dependent on the geometrical form of the current, and modified a little by the electro-magnetic inertia of iron. He has photographed the sparks, and obtains clear traces of oscillation when self-induction is present. His results have little or no bearing on the form of lightning protectors.

Wesendonck (*Ann. Wied.* vol. xxxv. p. 450) has made the curious observation that if in a long vacuum tube the distance between the electrodes be increased, the resistance is not affected. This does not agree with Varley's conclusions (Proc. R.S. vol. xix. p. 236, 1871), who showed that after the polarization of the electrodes is overcome gases obey Ohm's law.

Mebius (*Beiblatter der Physik*, vol. xii. p. 678, 1888) has tried to verify the statement that an electric current diminishes the coefficient of elasticity of metals, and he has come to the conclusion that it has no action on elasticity.

ON THE INTENSITY OF EARTHQUAKES, WITH APPROXIMATE CALCULATIONS OF THE ENERGY INVOLVED.[1]

As an exact science, seismology is in its infancy. Although great progress has been made during the past ten years, and especially in the development of instruments and methods for a more precise study of seismic phenomena, the results thus far have served rather to reveal the complicated nature of the problems involved; and while encouraging the seismologist to renewed effort, they warn him that his efforts are not to be light. The recent advances of the science have been, and properly, toward the study of the phenomena at hand, the nature and extent of the motion of the earth particle together with the rate at which the disturbance is propagated, in the expectation and hope that in time the location and character of the original cause may be revealed through these.

In the early growth of an exact science one of the obstacles met with is the absence of an exact nomenclature, and seismology furnishes no exception to this rule. Whenever it becomes desirable or necessary to incorporate the meaning of a word in a mathematical expression, it is imperative that the necessary restrictions be placed upon its use. It has long been customary to speak of the *intensity* of an earthquake without any special effort to give the word an exact meaning. Generally it is applied to the destructiveness of the disturbance on the earth's surface, and sometimes to the magnitude of the subter-

[1] By Prof. T. C. Mendenhall, President of the Rose Polytechnic Institute, Terre Haute, Indiana. (From the Proceedings of the American Association for the Advancement of Science, 1888.)

ranean cause of the same. But modern seismology proposes to measure the intensity of an earthquake and to express its value numerically. It is worth while, therefore, to inquire in what sense the term may be used with precision, and what may be accepted as its mathematical equivalent. Evidently it may mean, and in fact it has been made by different writers to mean, the measure of the surface destruction; the energy per unit area of wave-front of a single earthquake wave; the rate at which energy is transmitted across unit area of a plane parallel to the wave-front; and the total energy expended in the production of the original disturbance. The use of well-constructed seismographs has furnished us, within a few years, a good deal of fairly trustworthy information relating to certain elements of earthquake motion, notably the amplitude and period of vibration and the velocity of transmission, by means of which, and aided by a few not very violent assumptions, some of the above quantities may be calculated. They are not identical, numerically or otherwise, and it is manifestly improper to apply the word *intensity* to all of them.

An earthquake wave is generally assumed to be the result of an harmonic vibration. While this supposition is not strictly correct, it is probably not so far erroneous as to materially vitiate the results which follow.

If then—

a = maximum displacement,
t = periodic time,
v_1 = maximum velocity of particle,
V = velocity of wave transmission,
d = density of material through which transmission occurs,

the following are easily obtained:—

(1) Maximum velocity, $v_1 = \frac{2\pi a}{t}$.

(2) Maximum acceleration, $\frac{v_1^2}{a} = \frac{4\pi^2 a}{t^2}$.

(3) Energy of unit volume with velocity, $v_1 = \frac{1}{2}dv_1^2 = \frac{2\pi^2 a^2 d}{t^2}$.

(4) Energy of wave per unit area of wave-front $= \frac{2\pi^2 a^2 dV}{t^2}$.

(5) Energy per second across unit area of plane parallel to wave-front (rate of transmission) $= \frac{2\pi^2 a^2 dV}{t^2}$.

It is well known that Mallett and others of the earlier seismologists attempted to find a mathematical expression which should represent the so-called "intensity" of the shock, by means of the velocity of projection of loose bodies as determined by their range, and also through the dimensions of bodies which would be overturned by the shock. The maximum velocity of the earth might be ascertained by the first method with fair accuracy; the second method is nearly, if not quite, worthless in practice, and both are decidedly inferior in design and operation to the modern seismograph, which gives the principal elements of the motion directly.

In a paper by Profs. Milne and Gray, *Philosophical Magazine*, November 1881, the following occurs:—"The intensity of a shock is evidently best estimated from the maximum velocity of translation produced in a body during an earthquake. This is evidently the element according to which the destructive power is to be measured, it being proportional to the maximum kinetic energy of the bodies on the earth's surface relative to that surface during the shock." Now this statement is inconsistent with that which immediately follows, and with their mathematical expression, which is $I \propto \frac{A}{T^2}$, equivalent to the second expression given above. This inconsistency was doubtless quickly and first detected by the authors, and in a copy of the paper received from them I find interlinear corrections in the paragraph quoted above in virtue of which the words "rate of change of" are substituted for the word "maximum" where it first occurs, and "acceleration" for the words "kinetic energy," thus bringing it into agreement with the remainder of the discussion, and at the same time unquestionably better representing the opinion of the authors, who in all subsequent publications have used the maximum acceleration to represent the intensity as shown in the overturning, shattering, and projecting power of the shock.

The same expression, $\frac{v_1^2}{a}$, is used as a measure of intensity by Prof. Holden in his paper on "Earthquake Intensities in San Francisco" (*American Journal of Science*, vol. xxv. p. 427) where he defines it as "intensity of shock defined mechanically = destructive effect = the maximum acceleration due to the impulse." He asserts that "the researches of the Japanese seismologists have abundantly shown that the destruction of buildings, &c., is proportional to the acceleration produced by the earthquake shock itself, in a mass connected with the earth's surface." This statement is hardly justifiable, at least up to the present time. In the Report of the British Association for 1885, the Committee appointed by the Association for the purpose of investigating the earthquake phenomena of Japan, consisting of Messrs. Etheridge, Gray, and Milne, describe among other seismic experiments one which consisted in determining the quantity to be calculated from an earthquake diagram which would give a measure of the overturning or shattering power of a disturbance. The result of this investigation seemed to show that the acceleration, which by calculation from the dimensions of the columns was necessary for overturning, was something between the mean acceleration, represented by $\frac{4v_1}{t}$, and the maximum acceleration, $\frac{v_1^2}{a}$.

The actual destruction caused by an earthquake wave is undoubtedly a function of many variables, but it seems tolerably certain that maximum acceleration is the leading factor, and at the present time no better measure can be found. It appears to me, however, that it is unwise to apply the term "intensity" or "intensity of shock" to this quantity, which might be called the "destructiveness" of the wave, or perhaps its "destructivity," as indicating a little more clearly the power to destroy.

Dutton and Hayden, in their "Abstract of the Results of the Investigation of the Charleston Earthquake," presented to the National Academy of Sciences on April 19, 1887, define intensity as the "amount of energy per unit area of wave-front," but, in the subsequent discussion, use it almost continually as a measure of surface destruction. Upon the first definition they have based a very interesting and novel method for determining the depth of the focus; but in the application of the method to the Charleston earthquake they have used the word in its other and very different sense. A reference to the formulæ given above will show that one of these quantities is inversely as the square of the distance from the origin, as assumed by them in the development of their method, while the other, used in its application, is not so proportional, and this must be admitted to be fatal to their deductions.

In the discussion of a somewhat analogous case, Lord Rayleigh says ("Theory of Sound," vol. ii. p. 16), "The rate at which energy is transmitted across unit of area of a plane parallel to the front of a progressive wave may be regarded as the mechanical measure of the intensity of the radiation." The algebraic expression for this quality, as shown above, is, of course, similar to that of the quantity last considered, differing from it only in the power of "t" in the denominator. Both are very important expressions; neither is very closely related to "surface destruction," and the latter is unquestionably a suitable measure of the "intensity of an earthquake" in the most important sense.

It thus appears that at least four measures for earthquake intensities are and have been in use, which are expressed mathematically in terms of amplitude, period, velocity of transmission, and density of medium in formulæ (1) (2) (4) (5) above. To show more forcibly the necessity of placing some restrictions upon the use of the word, I have compared the "intensities" of two earthquakes, using each of the four expressions. The disturbances compared are those of May 6 and May 11, 1884, at Tokio, Japan, the observations being made by Prof. Milne (Trans. Seis. Soc. Japan, vol. x. p. 27). The same instrument, located in the same place, was used in both, and the interval of time between the two is so small as to forbid any important change in the conditions. That of May 6 is called "A," and that of May 11, "B." The results are as follows:—

$\frac{B}{A}$ ...	(1)	(2)	(4)	(5)
	1·1	1·7	0·9	1·3

from which it is evident that much depends on the measure of intensity adopted.

As stated at the beginning of this paper, the more recent

work of seismologists has been in the study of individual disturbances for the purpose of determining the principal elements of motion, amplitude, period, direction, and speed of transmission. In this study much has been learned. From the nature of the case we are almost absolutely restricted to an investigation of surface phenomena, and we are soon forced to admit that what goes on at the surface cannot accurately represent what is going on below. Among other reasons for this conclusion we have, notably, the greatly varying results obtained from the same disturbance at points comparatively very near to each other. The amplitude at one point may be two or three times that at another a few hundred feet away, and not only this, but the periodic times do not agree, and when the maximum acceleration is applied to the disturbance, its so-called intensity or destructiveness will vary greatly within a small area. As a matter of fact, it has long been known that such variations in destructive power do occur in nearly all earthquakes. Not only do the above elements vary, but the speed of transmission, when once the surface is reached, is undoubtedly not constant, although we have no reason to believe that it is not approximately so in the rocks through which it is, in the main, transmitted. Most of these irregularities are doubtless due to the non-elastic character of the materials lying near the surface and to their lack of homogeneity. In spite of their appearance in the phenomena of the surface, it is difficult, if not impossible, to believe that they exist in the rocks below. It is more reasonable to assume that during an earthquake the waves of transmission are, in the main, and until the surface is reached, somewhat regular in their form and approximately constant in certain of their elements. It may also be assumed that in amplitude and periodic time the subterranean wave, although doubtless much less than the surface-wave, cannot differ from it enormously, so that elements of motion obtained by seismometric observations upon the surface may be applied within certain limits to the investigation of the energy involved, the results being considered as rough approximations.

On these assumptions the following calculations have been made:—

Let A be the area of a portion of a wave-front, and l a length measured at right angles to A. Then formula (5) above, which shows the energy per second across unit area, multiplied by $\frac{\mathrm{A}l}{\mathrm{V}}$ will evidently express the energy required to generate the waves existing at any moment in the volume lA. That is

$$\frac{2\pi^2 a^2 d\mathrm{V}}{t^2} \cdot \frac{\mathrm{A}l}{\mathrm{V}}$$

$$= \frac{2\pi^2 a^2 d\mathrm{A}l}{t^2}$$

$$= \frac{2\pi^2 a^2}{t^2} \cdot m \; (m = \text{mass of volume } l\mathrm{A})$$

$$= \tfrac{1}{2} m v_1^2.$$

That is to say, the work consumed in generating waves of harmonic type is the same as would be required to give the maximum velocity to the whole mass through which the waves extend.[1] Sir William Thomson, who was probably the first to apply this principle, in his calculation of the mechanical value of a cubic mile of sunlight, concludes that in the case of a complex radiation this value is more likely to reach twice that of the above expression.

On the assumption that the maximum velocity of the particle is known, we may now apply this formula to the calculation of the energy involved in an earthquake. For this purpose I have selected, first, the Japanese earthquake of January 15, 1887, which disturbed over 30,000 square miles of territory, and the elements of which were well recorded on the Tokio seismographs. Assuming a mass of 150 pounds per cubic foot, and taking a cubic mile as the volume to be considered, I find that to put it in vibration required the expenditure of 2,500,000,000 foot-pounds of energy, and this might be called the "mechanical value of a cubic mile of earthquake." Assuming that an area of 100 miles square, with a mean depth of one mile, was thus in vibration at any one instant of time, which is not improbable considering the known rate of transmission and the long duration of the earthquake, the amount of energy thus represented would be 25×10^{12} foot-pounds. This energy might be generated by the fall, under the action of gravity, of a cube of rock 1000 feet on each edge, the mass of which would be 75,000,009 tons, through a vertical distance of about 166 feet.

[1] Lord Rayleigh, "Theory of Sound," vol. ii. p. 17. Sir William Thomson on "The Possible Density of the Luminous Medium."

It would be interesting to apply this method to the Charleston earthquake of August 31, 1886. Unfortunately no seismographic records were made, and the elements of motion are largely matters of conjecture. Messrs. Dutton and Hayden, in the report already referred to, express the opinion that in some localities the displacement must have been as much as 10 inches or 1 foot. This seems to me improbable, but it may be safe to say that over a considerable area it was as much as 1 inch. Nothing is known with certainty as to the period of the oscillations, but as it generally increases with the magnitude of the disturbance, it would probably not be grossly incorrect to call it two seconds. Assuming these magnitudes, I find the energy of a cubic mile of the Charleston earthquake, taken near enough to the epicentrum to be disturbed as above, to be equal to 24,000,000,000 foot-pounds. The speed of transmission of this disturbance has been pretty well determined, by Newcomb and Dutton, to be approximately three miles per second, so that a cubic mile would be disturbed in one-third of a second. To do this would require 130,000,000 horse-power. Assuming as before that an area about the epicentrum 100 miles square was thus disturbed, the energy involved would be 24×10^{13} foot-pounds, and the rate of its expenditure would be that of 1,300,000,000,000 horse-power.

All of these numbers can only be regarded as gross approximations. They probably indicate the order of magnitudes involved, and may be useful until more trustworthy data are furnished.

THE ROYAL HORTICULTURAL SOCIETY.

THE annual general meeting of the Royal Horticultural Society was held on Tuesday, February 12, at the offices, 117 Victoria Street, S.W. The Society is to be heartily congratulated on the great improvement which has taken place in its affairs since it quitted the Gardens at South Kensington this time last year. From the Report of the Council, and the speech of Sir Trevor Lawrence, Bart., M.P., President, in moving its adoption, we glean the following particulars. During the past year 657 Fellows have joined the Society, 81 have resigned, and 48 died, the total number of Fellows on the books being now 1636. The total income from all sources, independent of the "Donation" account (£1125 5*s.*), was £3617 8*s.* 6*d.*; the total expenditure, £3412 14*s.* 4*d.*, showing a surplus of £204 14*s.* 2*d.* On January 1, 1888, there was a debit balance of £1152, which has been cleared off by the transfer of of £755 7*s.* 6*d.* from the "Donation" account, and £396 12*s.* 6*d.* from current revenue. The total expenditure on the maintenance of the Society's Gardens at Chiswick was £1501 6*s.* 8*d.*, the receipts from the sale of produce, £737 7*s.* 6*d.*, brought up by minor items to £810 4*s.* 3*d.*, making the net cost of the Gardens to the Society £691 2*s.* 5*d.* The income for 1889 is estimated at £3000, and the expenditure at £2950. The President referred to the great value to the Society of the services of Mr. Dyer, F.R.S., Director of the Royal Gardens, Kew, and Mr. H. Veitch, who were retiring from the Council owing to the pressure of other engagements, and of Mr. Wilson, F.R.S., and Dr. Hogg, who were retiring after having served the Society well and faithfully during very many years. He also paid a well-deserved tribute to the energy, ability, judgment, and devotion to their duties, of the Honorary Secretary, the Rev. W. Wilks, and the Treasurer, Mr. D. Morris, Assistant-Director of the Royal Gardens, Kew. During the past year numerous very interesting exhibitions have been held in connection with the fortnightly meetings of the Society in the Drill Hall of the London Scottish Volunteers, James's Street, Buckingham Gate. A magnificent show was held on May 17 and 18, in the Gardens of the Inner Temple, by the permission of the Treasurer and Benchers, in which, for the first time in the history of such displays, attention was drawn to the excellent horticultural work being done by the market growers of the London district. A conference on apples and pears, held at Chiswick from October 16 to 20, attracted great attention, and the papers read and the discussions raised as to the circumstances and conditions requisite for the successful cultivation of these fruits in the British Isles were of great value. The Society propose to hold this year, in addition to a great show in the Temple Gardens on May 30 and 31, and its usual bi-monthly exhibi-

tions, a national rose conference and show on July 2 and 3, a great vegetable conference on September 24, 25, and 26, and a chrysanthemum centenary conference on November 5 and 6, —all at Chiswick. There will be at the bi-monthly meetings a short lecture and discussion on the plants exhibited, such as was in former years very popular under the guidance of Dr. Lindley. The Society will revive the publication of its periodical Journal and Proceedings, and carry on at Chiswick extensive trials of various classes of flowering plants, ferns, vegetables, and artificial manures.

Altogether it is gratifying to find that, the unfortunate and costly connection of the Society with South Kensington having been severed, there is a great revival of vigour and vitality and of interest in a Society which has been in existence for nearly a century, is the parent of a vast progeny of kindred institutions throughout the Empire, and has rendered to horticulture services the value of which it is impossible to over-estimate.

SOCIETIES AND ACADEMIES.

London.

Royal Society, January 31.—"On *Isoëtes lacustris*, Linn." By J. Bretland Farmer, B.A., F.L.S. Communicated by Prof. S. H. Vines, F.R.S.

Many points connected with the development of Isoëtes still remain to be cleared up, and this is especially true of the germination of the macrospore. This structure consists of a mass of protoplasm, which, in perfectly mature spores, is inclosed in a thick envelope, and this envelope is differentiated into six layers, of which the outermost belongs to the episporium, the three within this to the exosporium, and the two innermost to the endosporium. The protoplasm contains very large quantities both of oil and starch, and is provided with a large nucleus, in which are embedded certain bodies which appear yellowish-brown in preparations stained with hæmatoxylin, but as to whose nature it is not at present possible to speak definitely. During germination, hæmatoxylin fails to reveal the presence of a nucleus, and the cytoplasm at this period stains so rapidly and deeply that it is possible that the nuclear substance may be diffused through it. It is when the protoplasm stains thus deeply that the first indication of cell-formation is visible. Its mass is seen to be traversed by "cracks," which divide it into a few large isolated portions, and it is in the direction of these cracks that the cell-walls first appear. The splitting of the protoplasm is doubtless called forth by the methods necessarily used in embedding the spores, but the conclusion that may be drawn from its presence probably is, that the substance destined eventually to form the cell-wall, is present before the appearance of this structure, and is arranged in a plate-like manner, in such a way as to determine the direction of the cracks referred to. The cells thus formed probably owe their existence to a process similar to that obtaining in the endosperm of many plants, except that, at least so far, I have been unable to detect free nuclei in the first stages. Very soon after cell-formation has begun, these bodies are again very easily seen, especially in the upper part of the young prothallium. Division proceeds with great rapidity in this portion, and the rudiments of the archegonia are laid down. Their origin is similar to that in the Marattiaceæ; superficial cells divide at first once, periclinally, and of the two cells thus formed, the upper gives rise to the neck by further division into four stories, each story being divided crosswise in the usual way; the lower cell forms the central series consisting of oosphere, one neck, and one ventral-canal cell.

Division in the lower part of the prothallium takes place with extreme slowness, and here at least the appearance of free nuclei precedes that of the cell-walls. The cells which arise in this region are readily distinguishable, on account of their large size, from those which owe their origin to the primitive cells in the upper portion of the prothallium, but although further cell-division takes place throughout the spore, I am not in a position at present to state exactly how it occurs, or in what relation it stands with regard to the nucleus in these later stages.

Mineralogical Society, January 22.—Mr. R. H. Scott, F.R.S., President, in the chair.—The following papers were read:—On connellite from a new locality, by G. T. Prior.—On pseudomorphs of hæmatite after iron pyrites, by R. H. Solly.—On crystals of percylite, caracolite, and an oxychloride of lead (daviesite), from Sierra Gorda, Bolivia, by L. Fletcher.—On the distribution and origin of the hydro-carbons of Ross and Cromarty, by Hugh Miller.

Edinburgh.

Royal Society, January 21.—Prof. Sir Douglas Maclagan, Vice-President, in the chair.—Mr. J. Arthur Thomson discussed the history and theory of heredity.—Prof. Haycraft communicated a note, written by himself in conjunction with Dr. E. W. Carlier, on the conversion, by means of friction, of ciliated into stratified squamous epithelium. Specimens in illustration were shown under the microscope.—Prof. Tait read a paper on the virial equation, as applied to the kinetic theory of gases, especially as regards the form of isothermals in the neighbourhood of the critical point. The curves obtained from the new formula, with suitable values of the constants, represent with accuracy the isothermals of carbonic acid gas as obtained experimentally by Andrews. But the chief point of interest in the paper is connected with the question, What portion of the whole kinetic energy of a substance is, in each of its molecular states (*i.e.* as gas, vapour, liquid, and solid), to be regarded as proportional to the absolute temperature? Prof. Tait gave reasons for believing that it is incorrect to assume, with Van der Waals and Clausius, that the *whole* kinetic energy determines the absolute temperature. The part which is directly due to inter-molecular forces is at least mainly incommunicable to other bodies, and can thus have little to do with the temperature.—A note, by Mr. R. T. Omond, on a remarkable fog-bow seen from Ben Nevis on December 4, 1888, was communicated.—Mr. George Brook described a new type of dimorphism found in certain *Antipatharia*.—Prof. Crum Brown communicated a paper, by Dr. A. B. Griffiths, on a primitive kidney, or the beginning of a renal system.

Sydney.

Royal Society of New South Wales, October 3, 1888.—Sir Alfred Roberts, President, in the chair.—The following papers were read:—Considerations of photographic expressions and arrangements, by Baron Ferd. von Mueller, K.C.M.G., F.R.S.—Census of the fauna of the older Tertiary of Australia, by Prof. Ralph Tate.—Notes on the storm of September 21, 1888, by H. C. Russell, F.R.S. Mr. Russell pointed out that, although such cyclonic disturbances seldom visited the coast of New South Wales, experience had proved that they were not unknown, and another such storm of greater dimensions would probably cause considerable havoc in the city of Sydney if the unstable class of buildings so much in vogue were adhered to.—Some New South Wales tan substances, Part 5, by J. H. Maiden.

November 7.—Mr. H. C. Russell, F.R.S., Vice-President, in the chair.—The following papers were read:—Results of observations of Comets I. and II. 1888, at Windsor, N.S.W., by John Tebbutt.—Desert sandstone, by the Rev. J. E. Tenison Woods.—On a new self-recording thermometer; and notes on the thunderstorm of October 26, 1888, by H. C. Russell, F.R.S. Three meteorites were exhibited: the first, weighing 35½ pounds, found near Hay, was shown by the Chairman; the second, weighing 137 pounds, found at Thunda, in Queensland, had been received by Prof. Liversidge that afternoon. A preliminary note upon the qualitative analysis of a portion of this meteorite was read before the Society in 1886, which showed that it consists essentially of iron and nickel with a little cobalt, sulphur, and phosphorus. The third was exhibited by Mr. C. S. Wilkinson: it weighed 12½ pounds, and was composed chiefly of iron, of an irregular pear-shape, and was discovered firmly embedded in slate rock on the highest peak of a mountain near the junction of the Burrowa and Lachlan Rivers.

December 5.—Sir Alfred Roberts, President, in the chair.—The Chairman announced that the Council had awarded the Clarke Memorial Medal for 1889 to R. L. J. Ellery, F.R.S., Government Astronomer of Victoria.—The following paper was read:—The Latin verb *jubere*—a linguistic study, by Dr. John Fraser.—Prof. Liversidge exhibited and described a large collection of New South Wales minerals.

Paris.

Academy of Sciences, February 4.—M. Des Cloizeaux, President, in the chair.—On the loss of nitrogen during the decay of organic substances, by M. Th. Schlœsing. The fact that, during decomposition, nitrified organic matter liberates

free nitrogen, was demonstrated for the first time by M. Reiset in 1856, and confirmed by the subsequent experiments of Messrs. Lawes and Gilbert, and others. Here, M. Schlœsing describes the process by which he has endeavoured to determine the quantity of nitrogen dissipated during decomposition under the *natural* conditions of temperature and the general environment. The results of these researches will be communicated in a future paper.—The Buitenzorg Botanic Garden and Laboratory, by Dr. Treub. From this interesting description of the Buitenzorg institution near Batavia, Java, it appears that it comprises three distinct branches: (1) the Botanic Garden, properly so called, where are cultivated from 8000 to 9000 tropical and sub-tropical plants; (2) the Tjibodas Garden, situated at an altitude of 1500 metres in one of the hilliest districts of the Preang residency; (3) the Experimental Garden, in the Tjikeumeuh quarter of Buitenzorg, where are grown all the economic plants of the tropical zone. The first comprises a museum, an herbarium, a large library, a phytochemical laboratory, a photographic *atelier*, and a laboratory for botanic research. This last was established four years ago for the purpose of enabling botanists from Europe to study tropical vegetation on the spot, and is thus somewhat analogous to the Zoological Station at Naples. Buitenzorg is supported by an annual grant of £6000 from the Dutch East Indian Government.—Observations of Barnard's Comet, 1888 *e*, made with the west equatorial of the Paris Observatory, by M. D. Eginitis. These observations, covering the period from December 7 to January 8, give the positions of the comparison stars, and the apparent positions of the comet.—Observations of the new planet discovered on January 28, at the Observatory of Nice, by M. Charlois. The observations were taken on January 28 and 29, when the planet was of the thirteenth magnitude.—On the personal equation in astronomical calculations, by M. J. J. Landerer. The object of this communication is to show that within somewhat wide limits the personal equation depends on an effect of diplopy which may be easily measured.—Euler's problem on the equation $ds^2 = dz^2 + dy^2$, by M. G. Kœnigs. In this note the problem in question is extended to the case of any surface.—On homography in mechanics, by M. Appell. It is shown that the method of transforming figures by central projection, which plays such an important part in geometry, may also be employed in mechanics. These remarks may be extended to the movement of a point in space, and even to the movement of several points, on the condition in the latter case of making a general homographic transformation, which shall simultaneously contain the co-ordinates of all the points.—On the compressibility of mercury and the elasticity of glass, by M. E. H. Amagat. In his communication of October 15, 1888, the author gave the results of his studies on the elasticity of the crystal in the cylindrical piezometers of this substance charged with mercury. By simultaneous inward and outward pressure of these cylinders, he obtains the coefficient of apparent compressibility, and ultimately that of the absolute compressibility of the liquid metal. The whole series of experiments are now repeated, and the results here tabulated, showing for mercury a mean general coefficient of 0·000003918 under pressures not exceding 50 atmospheres. Although slightly higher, this may be regarded as in accordance with the coefficient 0·0000036, obtained by Prof. Tait, who worked up to a pressure of 450 atmospheres, and who considered his result as somewhat too low, even according to the method adopted by him ("Voyage of the *Challenger*," Part 4).—On the heat of formation of the bichromate of aniline, by MM. Ch. Girard and L. L'Hote. In a previous note (*Comptes rendus*, June 13, 1887) the authors showed that the bichromate of aniline might be easily prepared by causing the bichromate of potassa to react on the hydrochlorate of aniline under conditions there specified. In order to complete these researches they here study the thermic conditions of the formation of this salt.—Alcoholic combinations of the glycolalcoholate of soda, by M. de Forcrand. The author finds that the monatomic alcohols combine with the glycolalcoh olate of soda, forming with it crystallized compounds analogous to the alcoholic glycerinates of soda. He has prepared and analyzed the following products:—

$$C_4H_5NaO_4,\ C_2H_4O_2,$$
$$C_4H_5NaO_4,\ C_4H_6O_2,$$
$$C_4H_5NaO_4,\ C_6H_8O_2.$$

—On the quantitative analysis of organic nitrogen by the Kjeldahl method, by MM. E. Aubin and Alla. In reply to M. L'Hôte's objections to this method, experiments are here described showing that it is both trustworthy and accurate, yielding results fully equal, if not superior, to those of MM. Will and Warrentrapp. During the process the organic matter is completely transformed, and in the end all the nitrogen appears under an ammoniacal form; the sulphuric liquids obtained are always limpid and colourless, nor is there any loss of ammonia during the operations.—On the cockroaches of the Carboniferous age, by M. Charles Brongniart. Mr. S. H. Scudder, author of the best monograph on these Palæozoic forms, divides them into the two families of Blattinariæ and Mylacridæ, the former common to both hemispheres, the latter, as he supposed, confined to the United States coal-fields. But it is here shown that this is a mistake, and that the Mylacridæ are as common as the Blattinariæ in the Commentry formations, France.—M. de Malarce contributes a paper on the extension of the metrical system, on the development of uniform monetary systems and of the credit system (cheques, bills, &c.), throughout the civilized world.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Scientific Papers of the late Thomas Andrews, M.D., F.R.S., with a Memoir by P. G. Tait and A. Crum Brown (Macmillan).—The Physician as Naturalist: W. T. Gairdner (MacLehose, Glasgow).—Report on the Proceedings of the U.S. Expedition to Lady Franklin Bay, Grinnell Land, vol. i.: A. W. Greely (Washington).—Popular Lectures and Addresses, vol. i., Constitution of Matter: Sir Wm. Thomson (Macmillan).—The Philosophy of Mysticism, 2 vols.: C. du Prel, translated by C. C. Massey (Redway).—Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs, Erster Band, Protozoa, 53, 54, 55, Liefg.: Dr. O. Bütschli (Williams and Norgate).—Bird Life of the Borders: A. Chapman (Gurney and Jackson).—Report on the Cost and Efficiency of the Heating and Ventilation of Schools: T. Carnelley (Dundee).—Yield Tables for the Scotch Pine: W. Weise; converted into English measure and arranged by Dr. W. Schlich (Allen).—Volkaner i det Nordöstlige Island: Th. Thoroddsen (Stockholm).—Journal of the Chemical Society, February (Gurney and Jackson).—Observaciones Magnéticas y Meteorológicas del Real Colegio de Belen en la Habana, 4°. Trim. 1886 (Habana).—Bulletin de l'Académie Royale des Sciences de Belgique, No. 12 (Bruxelles).—Annalen der Physik und Chemie, 1889, No. 2 (Leipzig, Barth).—Beiblätter zu den Physik und Chemie, 1889, No. 1 (Leipzig, Barth).

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THURSDAY, FEBRUARY 21, 1889.

THE ILLUSTRATED OPTICAL MANUAL.

The Illustrated Optical Manual. By Sir T. Longmore, C.B., F.R.C.S. Fourth Edition. Pp. 239. (London: Longmans, Green, and Co., 1888.)

IN the present edition this manual, which was originally intended for army medical officers, has been considerably enlarged, with a view to its suitability as a text-book for civil as well as for military surgeons. Civil surgeons, however, are abundantly supplied with excellent works on ophthalmology; and for this reason (to say nothing of the fragmentary nature of Sir T. Longmore's Manual) we scarcely think that it will ever enjoy any large vogue amongst that class. The author treats of the detection and correction of refractive errors, and of optical defects generally: surely it would have been better to produce at once a complete manual of ophthalmology than to treat the subject in this piecemeal fashion. For how can any military surgeon be certain of his diagnosis in a case unless he has a thorough knowledge, not only of errors of refraction, but of eye diseases as well?

The opening chapter, on optical principles, is very good; the description of refraction, &c., being well and clearly done. Speaking of the composition of spectacle lenses, the author points out that the only real advantage possessed by quartz lenses ("pebbles") over ordinary crown glass ones is that they are harder, and, therefore, are not so easily scratched. By experiment, he has ascertained, contrary to the common supposition, that there is no difference in weight between pebbles and crown glass lenses of corresponding powers. The tourmaline forceps, for determining whether a pebble lens is, or (as is often the case) is not, cut exactly perpendicular to the axis of the crystal, is figured and explained.

We notice some peculiar statements in the chapters on myopia, hypermetropia, and astigmatism. For example, it is stated that the myopic eye "usually presents some peculiar characters indicative of its condition. It is prominent, or even appears to protrude; the pupil is usually contracted." At times, no doubt, cases of high myopia are seen in which the eyes do appear to be unduly prominent; but it is a sign of no value, and one which is more often absent than present. The pupil, if it shows any change at all, tends, in myopes, not to contraction, but to dilatation. In estimating refraction by the direct method, the examiner is recommended to place himself 18 inches or more from the patient's eye. This is an extraordinary statement, our impression heretofore being that it was impossible to be too close to the patient's eye. In examining the fundus oculi by the direct method of ophthalmoscopic examination, the examiner, it is stated, will have little or no tendency to bring his accommodation into play. This, again, is quite contrary to the experience of most persons; in fact, one of the greatest troubles experienced by tyros is that they almost invariably call their accommodation into play when it should be quiescent, and many adepts are unable, even after years of practice, completely to relax their ciliary muscles. The refraction, as tested by the direct method, is enjoined to be taken at the optic disk. Although this is the usual plan, a caution might well have been given that the refraction at the blind spot (optic disk) and at the yellow spot (the point of the most accurate vision) may differ appreciably. Again, in practising retinoscopy, it is recommended that the patient's eye be turned inwards. By this means, doubtless, a more easily seen reflex is obtainable with a small pupil, but this great drawback attaches to the method, that the refraction of some part of the fundus, other than the yellow spot, is estimated.

Sir Thomas Longmore gives currency to the views of his colleague, Dr. Macdonald, on the subject of normal astigmatism. Briefly, they are as follows. Every eye has a greater curvature in its vertical than in its horizontal meridian, but the difference in curvature is so slight that it does not interfere with visual acuteness. The meridian of greater curvature is not exactly vertical, but intersects the vertical meridian at an angle of about 15°. When a vertical line is held within the near point of an eye it is blurred; but when the left eye alone is used, the left side of the line is less blurred than the right side; when the right eye alone is used, the right side of the line is seen more distinctly than its left side. The effect of this is that a line held within the near point is seen more distinctly when both eyes are used than when either eye by itself is used. Hence, in this view, normal astigmatism becomes an important aid to the perfection of binocular vision of near objects.

The regulations as to the visual examination of recruits are given in detail. The vision of regular army recruits is ascertained by their ability to count test-dots, one-fifth of an inch in diameter, held 10 feet from the eye: this is equivalent to a bull's eye target, 3 feet in diameter, at 600 yards. Sir Thomas remarks that the standard is very low, since an eye with normal vision should be able to count the dots, not at 10 feet, but at 43 feet. At present, therefore, recruits may be accepted who possess less than one-fourth normal vision.

There is no regulation in the English army allowing ametropic soldiers to wear correcting glasses. Sir Thomas Longmore states that the only disadvantage accompanying such permission would be the difficulty of replacing broken lenses when their wearers were on foreign service. The German soldiers, in 1870-71, wore spectacles; and, since then, their use has been sanctioned in the French army. Perhaps when the British mind is emancipated from the thraldom of the red coat, it may give its soldiers fairer play with respect to the use of spectacles. Some day a regimental optician may accompany soldiers on foreign service.

Malingering, or feigned blindness, is stated to be rare in our army, although sufficiently common in the armies of those countries where conscription is in force. If we are to take the two illustrative cases as samples, the British Tommy Atkins does not appear to be so good at deception as he is at fighting. One of the cases is reminiscent of a half-forgotten anecdote. A soldier complained that his sight was very defective. The surgeons were unable to prove that he was shamming. One day he was suddenly ordered by a sergeant to pick up a pin from the floor, where it had been placed at some distance from him; and, taken unawares, he did so. As the text somewhat

naïvely says, "He was sent back to the depot, and made no further complaint of weak sight." The author is not complimentary to his *confrères* when he says that many so-called malingerers are in reality found to be suffering from eye disease which their surgeons have been unable to detect.

The manual is profusely illustrated. Many of the illustrations are excellent, but some are of a fearful and wonderful nature. The frontispiece, representing a section of the anterior hemisphere of the globe of the eye, is, in its way, a masterpiece: the knob-like protuberance on the iris, the wormy ciliary processes, and the aggressively patent posterior aqueous chamber (which is now known to be non-existent as a space) complete a picture which belongs rather to the domain of comedy than to the province of reality.

GENERAL ASTRONOMY.

General Astronomy. By Prof. C. A. Young, Ph.D., LL.D. (Boston, U.S.A., and London: Ginn and Co., 1888.)

PROF. YOUNG is so well known in this country through his researches in solar physics, that he needs no introduction to our readers. The title of his latest work is very comprehensive, but we may at once say that it is not more so than the book itself. Every branch of the subject is touched upon more or less, although no particular branch receives very extended treatment. It is essentially a book for students, "and is meant to supply that amount of information upon the subject which may fairly be expected of every liberally educated person." For an intelligent reading of the book, only the most elementary knowledge of mathematics is necessary, but, as pointed out in the preface, the mental discipline and maturity which usually attend the later years of College life are assumed. The student is warned at the outset that astronomy, notwithstanding its important practical applications, is in the main a subject of intellectual pleasure rather than an economic one.

The general arrangement of subjects is as good as it well can be. Definitions and general considerations come first, then the various instruments are described, and these are followed by the corrections to be applied to instrumental measurements. Succeeding chapters deal with the various phenomena and problems presented by the earth, moon, sun, planets, comets, meteors, nebulæ, and stars. It is not necessary that we should refer in detail to those parts of the book dealing with the "old astronomy." Still, it may be mentioned that the treatment of Kepler's laws and the theory of lunar perturbations is especially good. We may also recommend Art. 253 to the notice of landscape painters who may be in doubt as to the representation of the moon with scientific accuracy. It would be hard to find a better account of this matter than that given by Prof. Young, and it has the great advantage of brevity.

It is in the various departments of astronomical physics that most debatable points arise, and any criticism is naturally directed there. With regard to sun-spots, Secchi's theory, with slight modifications, as previously published in Prof. Young's well-known book on the sun, is regarded as the most probable one. The theory that they are due to falls of cooled materials, however, scarcely receives justice. The objection raised is that it is not in accordance with the distribution of spots in latitude, but Mr. Lockyer has fully explained ("Chemistry of the Sun") how these spot zones may be accounted for by this theory. Obviously, if spots were produced by the fall of meteorites into the sun from any direction whatsoever, and there were no system of regulation, they would be formed all over the surface, and without espect to period. But just such a regulator as is required is to be found in the solar surroundings, and in the system of solar currents, of the existence of which evidence is constantly accumulating. Moreover, by far the greater part of spot-producing material is probably "home-made," consisting of the cool condensed products of the vapours which have been driven out from below. In the face of these explanations, it is difficult to understand why Prof. Young should have dismissed the "downrush" hypothesis with so few words.

Some people find it difficult to believe that a light body like a comet can partake of motion in an orbit just as well as a planet, but these doubters are reminded (p. 407) that a feather falls as freely as a stone in a vacuum, and that this condition holds for space.

The theory that the luminosity of comets is due to collisions between the meteorites of which they are composed appears to find little favour with Prof. Young. He remarks (p. 418) that, "although the *absolute* velocity of the comet is extremely great, the *relative* velocities of its constituent masses, with reference to each other, must be very slight—far too small apparently to account for any considerable rise in temperature or evolution of light in that way." It must not be forgotten, however, that the meteorites must have some relative velocities, owing to the differential attraction of the sun upon the swarm, and that the disturbance thus set up would be increased as the distance from the sun diminished. The number of collisions would therefore vary exactly as the brightness of comets is known to vary, and this argues strongly in favour of the collision theory of luminosity. Prof. Young attempts to get over the difficulty by suggesting (p. 418) that a gas *in the mass* may become sensibly luminous at a much lower temperature than is generally supposed, but this, we fear, is scarcely reconcilable with the kinetic theory of gases.

There is an excellent short account of the zodiacal light on p. 347, but no reference is made to its connection with the aurora, as regards its spectroscopic phenomena and its periodicity. This relation furnishes further important evidence of its meteoritic nature.

Prof. Young has evidently very little sympathy with Mr. Lockyer's new meteoric hypothesis. Only scattered references to it are made, and very little criticism is vouchsafed. The little criticism that there is seems to be based on an imperfect acquaintance with the papers already published. Thus, the collision theory of variable stars is objected to (p. 484) on the ground that it is not consistent with the irregularities in the periods, but Prof. Young is evidently under the impression that this theory limits itself to the case of a single cometic swarm revolving in an orbit round a central swarm, whereas this case was put forward as the simplest possible.

In the chapter on the classification of stars according to their spectra, both Vogel's and Secchi's classifications are given. Now, Prof. Young admits that these are based on the "doubtful assumption" that stars like Sirius and Vega are the hottest, and he further remarks that it is possible for a red star to be younger than a white one. It scarcely seems consistent, therefore, to omit the new classification suggested by Mr. Lockyer, which is the only one that takes into account the probability of there being bodies with increasing as well as with decreasing temperatures. The latter classification is certainly a very new one, but other parts of the book show that Prof. Young must have been acquainted with it.

One remark of Prof. Young's is worth quoting. After stating that there are two Observatories established solely for the study of solar physics (Potsdam and Meudon), he remarks with characteristic straightforwardness that "There ought to be one in this country (America)."

We know of no other book which is so comprehensive and at the same time so well adapted for the use of those who aim at something more than a mere smattering of astronomical knowledge. For the benefit of those whose time may be too limited to take up everything in the book, those parts which may be most conveniently omitted are printed in small type. The language is clear, and to simplify matters there are over two hundred excellent illustrations. Further, as might be expected from the fact that Prof. Young *teaches* astronomy, the book is not diluted with irrelevant matter. A. F.

AN INDEX-CATALOGUE.

The Index-Catalogue to the Library of the Surgeon-General's Office, United States Army. Vol. IX. Medicine (Popular)—Nywelt. Pp. 1054. (Washington: Government Printing Press, 1888.)

THE progress of this *magnum opus* seems irresistible. Year by year the volumes reach us with a regularity that implies strength, and a completeness that indicates a more than mechanical accuracy of work. It still remains, so far as we know, unique among printed catalogues in classifying under subject-headings, such as Mercury, Milk, Neuralgia, &c., not only the books, but also the whole of the signed medical articles in the 3500 periodicals which form the medical press of the world, from Pekin to Paris, from Newfoundland to Uruguay. The newspaper articles are still, as they have always been, collected under the subject-title only, and not under the name of the writer also; for, if the latter cross-cataloguing had been adopted, we should have had more than 300,000 cross-entries, which would have necessitated already two more volumes at least as large as the present; but those articles or essays which the authors have thought it worth while to reprint all come under their names as pamphlets, and this is no inconsiderable number.

This volume includes some names which are embarrassingly popular among medical writers. It needs a clear head to deal with a catalogue of the works of 206 authors of the name of Meyer; but when the librarian comes to Müller, he finds 343 different authors awaiting him with much more voluminous works, and he must be thankful for the great variety of Christian names, and that not more than seven besides the great physiologist contented themselves with the plain Johannes. When we notice that the librarian of the most complete professional library in England has not to do with more than eighteen and thirty-seven authors with these surnames respectively, we can form some rough comparison of their relative completeness; and the student must become aware of what a debt he owes to his Transatlantic *confrère*, who has undertaken and carried through the task of collecting and cataloguing the works of the 494 other medical authors of the same names.

In the first forty-six pages of this volume, the immense collection of facts which had been so well grouped in Vol. VIII. under Medicine, is concluded; and the two longest articles left us are those on the Nervous System, and—perhaps not unnaturally—on New York. That on the Nerves and Nervous System (103 pp.) is one of the most valuable to the student, as such a very large proportion of that rapidly-growing part of medical knowledge is embodied not in books so much as in journalistic and pamphlet literature. The strength of the historic feeling still affecting Mr. John S. Billings and his fellow-workers is shown in the great collection of sixteenth and seventeenth century literature that is to be found under such a heading as Medicine (Popular), and many others. It is very rare nowadays to find in a newly-formed collection, dating from about a quarter of a century ago, any such tendency to accumulate the materials for those great works on the history of disease which we are leaving our successors to write. That is a point towards which the strong modern development of historic research, the earnest inquiry into the origin of species of disease is gradually leading us, but even the great work of Hirsch has left us much to learn and teach, a great field for genius in tracing correctly the broad generalizations in the evolution of the morbid tendencies of men. We are a little surprised to notice the complete absence of the works of Conyers Middleton, which contributed considerably to the understanding of Roman medicine; but we have been much more surprised at the almost unimpeachable way the Index-Catalogue has stood firm in our tests on minute points of very trifling general interest.

Now that these first nine volumes have covered the ground as far as the end of the letter N, it is not unlikely that the work may be finished in five more volumes, and that at Christmas 1893 the enthusiastic student of medicine may be able to possess himself of a work not much smaller than the latest edition of the "Encyclopædia Britannica," containing a catalogue of some 150,000 medical authors, and the titles of about 600,000 of their books, pamphlets, and articles that have been got together in a generation, mainly by the untiring energy of Mr. John S. Billings. A. T. MYERS.

OUR BOOK SHELF.

The Anatomy of Megascolides australis (the Giant Earthworm of Gippsland). By W. B. Spencer. Transactions of the Royal Society of Victoria, Vol. I. Part 1, pp. 1–60, 6 Plates. (Melbourne: Stillwell and Co., 1888.)

THE Royal Society of Victoria, which has hitherto issued only an *octavo* volume of Proceedings each year, will in future publish also Transactions in *quarto*. The present memoir is the first part of this new series; the

style in which it is printed and the excellent plates seem to promise that the Transactions will be quite on a level with any journal published in Europe. The illustrations are, indeed, unnecessarily large; but this cannot be pointed to as a fault—at least by those who are not responsible for the cost.

Prof. Spencer's paper is of considerable interest, particularly that section which refers to the nephridia. *Megascolides*, like *Perichæta* (as was first pointed out by Beddard, not by Perrier, as Prof. Spencer asserts), possesses a ramifying network of nephridial tubes which are continuous from segment to segment, and which open on to the exterior by numerous pores; connected with these there are—in the posterior segments of the body—a pair of large nephridial tubes in each segment, which open internally by a funnel. It is from these latter that the single pair of nephridia per segment of *Lumbricus*, &c., are to be derived; the network of minute tubules, which represents the excretory system of the flatworms, has disappeared in such forms as *Lumbricus*.

Prof. Spencer discusses the much-vexed question of the homologies of the sexual ducts, and concludes that they are not derived from nephridia.

Other points of interest cannot be touched upon in this short notice.

Lectures on Geography, delivered before the University of Cambridge, during the Lent Term, 1888. By Lieut.-General Strachey, R.E., C.S.I., President of the Royal Geographical Society. (London: Macmillan and Co., 1888.)

THESE lectures are published opportunely at a time when it is most desirable that the now almost general effort to further geographical education should be properly directed. They form a short course introductory to the work of the Lectureship on Geography now established in Cambridge, and in them General Strachey describes the aspects of the subject which he considers most suitable for the instruction of students at the University. He thus gives a complete summary of the aims and matter of scientific geography—of geography as a natural science related to other natural sciences, much as mathematics is to physical science. He assumes that students, before going to University, have acquired a general knowledge of geography; and, in passing, he points out that the primary object of the school teaching of geography is to impart an accurate knowledge of the main topographical features of the entire earth, all trivial details being omitted, and suitable instruction being given in the physical, economical, and historical characteristics of important places.

As material for the higher or University teaching of geography, the author practically claims the various branches of science which in recent years have been assembled under the term "physiography"; but he is most successful in showing that the science is not a mere patchwork, but a connected whole; and he sees no reason for abandoning the well-known name "geography." Certainly from many points of view the introduction of the new term has retarded the spread of a knowledge of the science.

An excellent epitome of the growth of our knowledge of the astronomical relations of the earth, and a short account of the methods of projection and orography, prepare the way for the history of geographical discovery. This department is reviewed in a manner at once interesting and philosophical, indicating clearly the close connection between the progress of discovery and the political movements of the world. The influence of the form and movements of the earth on terrestrial phenomena, terrestrial magnetism, our knowledge of the interior of the globe, and the relation of geology to geography, are in turn shortly discussed. The sections on land, sea, and air, and on the history of life and of man, indicate the results of recent investigation, and suggest many points which may well receive much attention from students of geography.

The lectures are written throughout in an agreeable and simple style, and will prove valuable to general readers as an elementary epitome of scientific geography.

F. GRANT OGILVIE.

A Text-book of Elementary Metallurgy for the Use of Students. By Arthur H. Hiorns. (London: Macmillan and Co., 1888.)

WE recently had occasion to notice a useful little work on practical metallurgy by Mr. Hiorns. He has now endeavoured to write a purely elementary treatise on theoretical metallurgy, adapted to the capacity of beginners. The attempt can scarcely be considered successful. In 172 pages printed in large type he deals with the whole of the wide field of metallurgy. This necessitates a very fragmentary treatment. And besides this, errors are so frequent as to render the book quite unsuited for beginners. The following examples may be cited:—The barrel method of amalgamation is stated (p. 90) to be carried on at Freiberg, where it was discontinued twenty-four years ago. One of the seven methods of producing steel is stated (p. 74) to be "by melting raw steel in crucibles." The Coppée coke-oven is described (p. 40) as being of the Appolt type. The coke-oven described (p. 42) as the Simon-Carvès is in reality a Carvès oven. The author appears to be ignorant of the existence of the principality of Catalonia, for the Catalan process is said (p. 54) to be carried on at "Catalan in the Pyrenees."

Altogether, the book compares very unfavourably with the author's work on assaying, and appears to have been hastily written. An illustration of the want of care displayed is afforded by the table of the specific gravities of eighteen metals (p. 11), in which in nine cases the figures differ from those given in the author's companion volume. With a little care, the author could have avoided such statements as—"An analogous compound, 'Boghead' of Scotland, which is a bituminous schist, is richer in bitumen than ordinary coal." Again, manganese, the author states (p. 74), "prevents the separation of carbon in the form of graphite, which is the opposite of silicon." The appendix of examination questions, covering 65 pages, appears to indicate that Mr. Hiorns's intention has been to write a cram-book for the elementary stage of the Science and Art Department's examination in metallurgy. It is, however, doubtful whether a student who made such blunders as occur in this book, would satisfy his examiners.

B. H. B.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Weismann's Theory of Variation.

ACCORDING to Weismann ("Die Bedeutung der sexuellen Fortpflanzung für die Selektions-Theorie," Jena, 1886), heredity does not consist in the parent having the power to reproduce offspring in its own likeness, but in the property of the germ (ovum or spermatozoon) in each generation to develop into an individual of a certain invariable type. He starts from the fact that in development the germinal cells are separate from the beginning, are portions separated off from the original fertilized ovum. He distinguishes between actual and virtual differences. Different individuals developed from successive remnants of a given *Keimplasma* may show actual differences; but these are due to the action of conditions affecting the particular individual during its development and life: these differences are not inherited, cannot possibly be transmitted to the offspring, because

the germ-cells in this altered individual, which were originally continuous with the germ-cell from which the individual itself developed, remain entirely unaffected by the action of the conditions on the body, and when they begin to develop have exactly the same properties as the germ-cell in the generation preceding.

Heredity, then, according to Weismann, is simply the property possessed by a germ to develop into exactly the same type in each successive generation. He says: "Ich stelle mir vor, dass die Vererbung darauf beruht, dass von der wirksamen Substanz des Keimes, dem Keimplasma, stets ein Minimum unverändert bleibt," &c.

As the action of conditions can give rise to no hereditary individual differences, these must be due to some other cause. This cause, Weismann says, is to be found in sexual, or, as Haeckel calls it, amphigonous, reproduction. Sexual reproduction consists in the fusion of two complementary germ-cells or of their nuclei: each of these germ-cells has a specific molecular structure, on which depend the hereditary tendencies of the organism whence the germ-cell is derived. Thus, in fertilization, two hereditary tendencies are mingled, and thus the offspring does not resemble exactly either of its parents, but combines the characters of each together.

In order that there shall be no ambiguity about his argument, Weismann precisely states what, according to his view, and, as he believes, in actual fact, occurs in monogonous reproduction, *i.e.* in parthenogenesis, where there is only one parent instead of two. If, in a species reproducing parthenogenetically, all the individuals were perfectly similar, all the descendants throughout any number of generations would continue similar, leaving aside evanescent differences due to conditions, and which are not hereditary. In such a case no selection, Weismann says, would be possible, and therefore no evolution in any direction.

"Processes of selection in the proper sense of the word, those which produce new characters by the gradual increase of characters already present, are not possible in species which reproduce asexually."

"If it were once proved that a species reproducing itself solely by parthenogenesis had been transformed into a new species, thereby it would be proved at the same time that other causes of modification exist than processes of selection, for by selection the new species could not have been formed."

But with sexual reproduction it is quite otherwise. Weismann points out that no two individuals of different generations could ever be similar where reproduction is sexual, and even the individuals of one family, born of the same two parents, would not be similar, because the various tendencies in the parents are present in different intensities at different times, though he gives no reason for this assumption.

According to Weismann, the individual hereditary differences so produced are the basis on which selection acts; and these differences thus explained, summed up or combined in different ways by selection, give a complete and satisfactory explanation of all organic evolution.

Now, let us examine this theory a little. For the sake of simplicity we will in most cases consider the effect of the supposed processes on one organ. In the first place, what ground is there for assuming that *Vermischung* would ever cause an increased development in the offspring of an organ possessed by the parents? Heredity, as understood by Weismann, is nothing more than the property in the germ-cell of developing into an individual like that from which it was derived. If each parent possessed a given organ in the same degree of development, a degree unaffected by external conditions, then both the ovum and the spermatozoon will, on this view of heredity, have the property of developing into an individual with the same organ developed to the same degree. When the two properties are combined by fertilization, the fertilized ovum ought to have the property of developing this character with still greater certainty, but why should it have the property of developing the character to a higher degree than that reached in either parent? By *Vermischung*, in its literal sense, the union of the two hereditary powers cannot have this effect. If by *Vermischung* it is meant that the offspring is intermediate between the two parents, then the mean of two equal characters is the same character again. And if this is what Weismann means by *Vermischung*, then a character developed to a certain degree in one parent, and not at all in the other, would in the offspring be developed to exactly half the degree in which it existed in the one. And so on. But it is obvious that in this way no increase of any character could ever occur.

But of course *Vermischung* may mean something else. It may mean that the hereditary powers of ovum and spermatozoon are added together, that the result of copulation between the germ-cells is not the mean, but the sum, of the properties of both. In this case, evolution would be extremely rapid, for each child would be equal to both its parents rolled into one. If each parent, say, among cattle, had horns equally well developed, the offspring would have horns twice as big. And it is obvious that in this way no *decrease* could ever occur, for if one parent had an organ developed and the other had no trace of it, the offspring would have it in the same degree as the one.

Now, it seems to me that, if *Vermischung* does not mean either of these things, there is only one other meaning it can have, and that is, that the hereditary powers of the copulating germ-cells reinforce one another to some extent, but not to such an extent that the result is equal to their sum. If this be the meaning, then there can never be any decrease in a character once formed. For, if every individual of a species possesses a certain organ, let us take the hind-legs in a mammal, then if two individuals which have these organs less developed than any other individuals in the species, copulate, the offspring resulting must have hind-legs better developed than either of them. Thus the whale could never have been evolved.

It follows, therefore, that, on Weismann's theory of variation, evolution is impossible. And as acquired characters are not inherited, no other theory of variation can be discovered. Therefore evolution is impossible altogether: the extremes meet, and the Darwinian principle overstrained goes rather to prove the fixity of species than their plasticity.

J. T. CUNNINGHAM.

Mr. Howorth on the Variation of Colour in Birds.

ALLOW me to assure Mr. Howorth that I have no theory to maintain. I simply called attention (*supra*, p. 318) to an overlooked hypothesis, propounded long ago, and, so far as I know, still unrefuted. Neither have I any wish to argue the question. Indeed, controversy about it is happily almost impossible, since he admits the chief fact of which I reminded him to be what he now terms (*supra*, p. 365) "an elementary postulate"—an expression far stronger than I should venture to use; but had he before shown any disposition to recognize it, my remarks had not been written. On the contrary, he implied (*supra*, p. 294) that it was a recent discovery, as it certainly appears to have been to him. I trust he will excuse me for having pointed out its want of novelty, just as he seems to excuse Prof. Geikie for pointing out the antiquity of his views as to the former climate of Siberia; and at the same time I have to ask Mr. Howorth's pardon for demurring to some of the assertions in his last communication, especially that as to the avifauna of Siberia having been "worked out from end to end." I dare not hope to see the day when this shall be done; but then I am not of a sanguine temperament.

I take this occasion to mention that in line 3 of the second paragraph of my former letter (p. 318) the word "Russian" was omitted before "explorers and naturalists." Of course it will be understood to cover Poles, as well as all those foreigners who were employed by the Russian Government.

ALFRED NEWTON.

Magdalene College, Cambridge, February 16.

Currents and Coral Reefs.

MAY I be allowed space to call attention to a remarkable fact relating to the growth of coral reefs, which has apparently (as far as I can ascertain) had no explanation, and which might assist materially in the elucidation of some problems relating to ocean currents about which—although the broad facts are known—a great deal of doubt exists? It might also give us some idea of the flow of submarine currents, the direction of which it is very difficult to determine.

It will be observed that in all coral formations there are in some places remarkable extensions of them from the land, which is not accountable for by supposing the depths only to be shallower in those directions, and the only alternative we have then is that the food supply must come chiefly from that direction, and this supply could only be kept up by currents striking the reef at these points. To give an instance of this, I might

refer to the Bermudas as affording a good example of this action: there, will be seen a great extension of the reefs on the north-west side, and reefs are forming on the south-west—the Challenger and Argus Banks; and it should be noticed that these are on the edge of the Gulf Stream, and also that there are considerable eddy currents here which would cause a constant supply of food to be brought to these parts of the islands; whereas the conditions round the other parts of the islands are not so favourable, and consequently there is no extension of the reefs.

I have been much puzzled by the curious formations of coral reefs in the harbours of Suakim and Massaua and also round the island of Key West; and I have been unable to account for the peculiarities in the shape of the fringing reefs except by an hypothesis such as the above, in these cases the tidal currents taking the place of the ordinary currents.

DAVID WILSON-BARKER.

The Earthquake in Lancashire.

ON Sunday, February 10, at 10.40 p.m., there was felt here a shock as of a heavy falling body, which caused the windows to rattle loudly. Two or three seconds later a second thud-like shock was felt of somewhat greater intensity than the first. This was followed by gentle but distinct tremors, lasting, perhaps, twenty or thirty seconds more. The weather was calm at the time; the heavy snow-fall had just ceased; barometer rising after the considerable depression which had occurred during the day. The sounds appeared to come from the north-east, as if a heavy body had fallen outside a window having that aspect. Several other persons name the same quarter as that whence the sounds seemed to proceed, and in one instance, in a room having several aspects, there was a distinct statement that the north windows were the first to shake, then those in the south-west, thus indicating a possible line of movement. In most cases no direction was noted. Persons down-stairs thought something had fallen above, those in the upper stories rushed down to see what had happened below. Others, again, felt surrounded by the unwonted movement. A heavy slip of snow from the roof seemed to occur, as first thought, to most, then a colliery explosion—there are two coal-beds near—and finally an earthquake.

The chief physical effects observed here, beside the general vibration, were a violent shaking of windows, the rattling of glass and crockery, such as bed-room ware. Suspended gas-shades and pictures on walls swung as if with the wind. The doors of rooms and cupboards opened and shut, one or two ornaments fell from their brackets, and the floor is described as "rising." Most of those who slept were awakened, and seemed to suffer more alarm than those who had not retired. The movements had apparently been more severely felt by them, and they describe their beds as "rocking," and themselves being almost thrown out. None of our telephones were affected. Substantial buildings seemed less affected than those less solidly built. The various observers quoted were all in a good position to note what occurred.

T. R. H. CLUNN.

The County Asylum, Prestwich.

Can Animals count?

UNDER this heading, Sir John Lubbock, in his recent interesting book on "The Senses of Animals," gives several instances of apparent counting in the case of insects. He says:—

"One species of Eumenes supplies its young with five victims, one ten, another fifteen, and one even as many as twenty-four. The number is said to be constant in each species. How, then, does the insect know when her task is fulfilled? Not by the cell being filled, for, if some be removed, she does not replace them. . . . In the genus Eumenes, the males are smaller than the females. . . . In some mysterious manner the mother knows whether the egg will produce a male or a female grub, and apportions the quantity of food accordingly. She does not change the species or size of her prey; but if the egg is male, she supplies five, if female, ten, victims. Does she count? Certainly this seems very like a commencement of arithmetic."

Now, it seems to me that this can be explained in a far simpler and more probable manner than by supposing that insects have any power of counting. I think we may safely consider—

(1) That a certain *average amount* of food is required in each case.

(2) That a certain *average time* is required by the insect to collect this food.

(3) That the eggs of the insect follow one another at a *certain rate*, over which she has little or no control.

(4) That the eggs which are to produce *males*, being smaller, take less time to form, and follow at shorter intervals, than do those which are to produce *females*.

Now take the case of the Eumenes which provides ten victims. She makes the cell, and goes on adding caterpillars until the egg comes to maturity and is laid, and the cell finished off. She then repeats the process, laying the egg when it comes to maturity, as before; the interval between the laying of one egg and the next being long enough, on the average, to provide ten victims, or in case the egg is to produce a male, the smaller interval only allows of five being provided.

There is thus no need to credit the insect with any power of *counting*, or of knowing beforehand anything about the sex of the eggs. It is merely another instance of the perfect way in which, by the process of evolution, means are adapted to ends.

The same explanation applies to the cases mentioned on pp. 254–56 of "The Senses of Animals." The bee laboured to fill the cell (in which a hole had been made so that the honey ran out again), until, "when she had brought the usual complement of honey, she laid her egg, and gravely sealed up the empty cell: . . . in some mysterious manner the bee feels when she has provided as much honey as her ancestors had done before her, and regards her work as accomplished."

I should suggest that the bee merely goes on bringing honey until the *egg is ready*. She then starts another cell, and goes through the same routine until the next egg is ready, and so on; the average amount of honey collected being proportional to the interval between the laying of one egg and the next. According to the theory of evolution, this interval is just sufficient for enough food to be provided for the use of the grub.

G. A. FREEMAN.

St. Olave's Grammar School, Southwark.

Weight and Mass.

MR. ANDREW GRAY need only turn to p. 355 of the current number of NATURE to find an example of an engineer's dynamical terminology, and it would tend to some useful result in this interminable discussion if he would point out in what manner the language of the engineer must be modified to suit the requirements of the *precisionist*. We find on p. 355 "the working pressure is 175 pounds per square inch," "the total weight of the engine in working order is 37 tons," "probably having about 30 tons useful weight for adhesion," and so on.

Let Mr. Andrew Gray point out what he considers the errors of the engineers. Ought the engineer to say, "the working pressure is 175 pounds weight (or pound weights?) per square inch," "the total mass of the engine is 37 tons," "probably having about 30 tons useful (? mass or weight) for adhesion."

On former occasions in this controversy I have attempted to elicit definite expressions of opinion on the terminology of similar definite actual dynamical problems, but hitherto without success.

A. G. GREENHILL.

Woolwich, February 12.

Detonating Meteor.

MR. MAXWELL HALL's letter on this subject is of considerable interest. When the great meteor-shower of November 11–15 was traced to the orbit of the comet of 1866, it was natural to suppose that the fine fire-balls which occur about this period belong to the same series. Plainly, however, the Jamaica fire-ball recorded by Mr. Hall had a southern radiant far distant from that of the well-known Leonid shower. The same thing was noticed in the case of the shower of aërolites which fell in France on the 13th of November, 1835, the motion being south-east to north-west; and in many other instances in which fire-balls or aërolites were observed within this period the phenomena seem to agree best with a southern radiant, though the descriptions are not as full as we could desire. It thus appears highly probable that almost concident in time with the well-known Leonid shower there is another shower, rich in fire-balls and aërolites, proceeding from a southern radiant. I hope it will be watched from southern stations in future.

W. H. S. MONCK.

Dublin, February 15.

Ice Planed.

PERHAPS it will interest some of those who are investigating the structure of different kinds of ice to know how blocks of it may, with ease and certainty, be shaped into bars and plates of any required dimensions. Some time ago I had occasion to prepare some specimens for experimental purposes. At first my success in working the ice into the required form was not very great, for it cracked in all directions under the action of a saw or chisel. After trying many devices, I at last resorted to a joiner's plane; a tool which may have been tried for the purpose before, though I do not remember having seen its use suggested. With it ice may be planed with greater ease than wood, the shavings coming away in powder, and leaving the ice with a clean, bright, sound surface. R. M. DEELEY.

39 Caversham Road, Kentish Town, N.W., February 16.

REPETITION OF HERTZ'S EXPERIMENTS, AND DETERMINATION OF THE DIRECTION OF THE VIBRATION OF LIGHT.

SINCE last October, Prof. Fitzgerald and I have been repeating some of Prof. Hertz's experiments, as occasion allowed; and it may not be without interest at the present time to give a short account of our work.

The first experiment tried was the interference of direct electro-magnetic radiation with that reflected from a metallic sheet. This experiment is analogous to that known in optics as "Lloyd's experiment."

The radiation was produced by disturbances caused in the surrounding space by electrical oscillations in a conductor. It was arranged in this wise. Two thin brass plates, about 40 centimetres square, were suspended by silk threads at about 60 centimetres apart, so as to be in the same plane. Each plate carried a stiff wire furnished at the end with a brass knob. The knobs were about 3

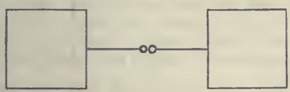


FIG. 1.

millimetres apart, so that on electrifying one plate a spark could easily pass to the other. This spark, as is well known, consists not simply of a transference of half the electricity of the first plate to the second—though this, which is the final state, is all that is observable by ordinary experimental methods—but the whole charge passes across to the second plate, then returns, and so on, pendulum-fashion, the moving part of the charge becoming less each time, till finally brought to rest, the energy set free at sparking being converted partly into heat in the wire and air break, partly into radiation into space, or in terms of action at a distance in inducing currents in other bodies.

The time taken by the charge to pass over to the second plate and to return, is a definite thing for a given sized arrangement, and depends on the connection between them. If C be the capacity of the plates, and I the self-induction of the connection, the time of each complete oscillation equals $2\pi\sqrt{CI}$. The time in the case of the particular arrangement used is, speaking roughly, about the 1/30,000,000 of a second.

If there be conductors in the neighbourhood of this "vibrator," currents will as usual be induced in each on every passage of the charge between the plates, each passage serving simply as a primary current.

Now, speaking briefly, the whole object of the experiment is to find out if these induced currents take place simultaneously in conductors situated at various distances from the primary current, and if not, to determine the delay. In order to do this we must, in the first place, be possessed of some means of even ascertaining that these currents occur, all ordinary methods being inadequate for detecting currents lasting only for such exceedingly short periods as these do. By devising how to determine the existence of these currents, Hertz made the experiment possible.

His method depends on the principle of resonance, previously suggested by Fitzgerald, and his current-observing apparatus is simply a conductor, generally a wire bent into an unclosed circle, which is of such a length that if a current be induced in it by a passage of a charge across the "vibrator" the return current or rush back of the electricity thus produced in the ends of the wire occurs simultaneously with the next impulse, due to the passage back across the "vibrator."

In this way the current in the "resonator" increases every time, so that at last the end charges, which are always of opposite sign, grow to be so great that sparks will actually occur if the ends of the wire are brought near together. Thus, Hertz surmounted the difficulty previously experienced by Fitzgerald when proposing electro-magnetic interference experiments.

The time of vibration in this circle is, as before, $2\pi\sqrt{CI}$, but on account of difficulties in calculating these quantities themselves, the length of the wire is most readily found by trial. To suit the "vibrator" we used, it was about 210 centimetres of wire No. 17. The ends of the wire were furnished with small brass knobs, which could be adjusted, as to distance between them, by a screw arrangement, the whole being mounted on a cross of wood for convenience in carrying about.

At first sight the simplest "resonator" to adopt would seem to be two more plates arranged similarly to the "vibrator," but it will be seen on consideration that it would not do, because no break for seeing the sparking could be put between the plates, for, if it were, the first induced current would be too feeble to jump the break, so that the reinforcement stage could never begin.[1]

The charging of the "vibrator" was effected by connecting the terminals of an induction-coil with the plates. In this way a continuous shower of sparks could be obtained in the resonating circle.

The circle in the interference experiment was held in the horizontal plane containing the axis of the "vibrator," the ends of the circle of wire being in such a position that a line joining the knobs was at right angles to the "vibrator." In this position only the magnetic part of

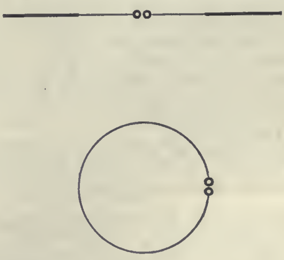


FIG. 2.

the disturbance could affect the circle, the "magnetic lines of force," which are concentric circles about the axis of the "vibrator," passing through the "resonator" circles.

When the knobs of the circle are brought round through 90°, so as to be parallel to the "vibrator," the electric part of the disturbance comes into play, the electric lines of force being, on the whole, parallel to the axis of the "vibrator." The electric action alone can cause a forced vibration in the knobs, even when the connecting wire is removed, if placed fairly close to the "vibrator."

[1] However, two pairs arranged in line, the pairs connected by a wire, could probably be got to spark between the centre plates.

Again, if the knobs be kept in this position, but the circle be turned through 90°, so that its plane is vertical, only the electric part can act, the magnetic lines of force just grazing the circle. In this way the disturbance can be analyzed into its magnetic and electric constituents.

Lastly, if the knobs be in the first position, while the circle is vertical, there will be no action.

To exhibit these alone forms an interesting set of experiments. It also makes a very simple and beautiful experiments to take a wire twice as long and fix it instead of the first, but with two turns instead of one; no sparking is then found to occur. This is, of course, quite opposed to all ordinary notions, double the number of turns being always expected to give double the electromotive force. In this way the reality of the resonance is easily shown.

Interference Experiment.—The sparking, of course, becomes less intense as the resonator is carried away from the "vibrator," but by screwing the knobs nearer together it was possible to get sparks at 6 and 7 metres away. On bringing a large sheet of metal (3 metres square, consisting of sheet zinc) immediately behind the "resonator," when in sparking position, the sparking increased in brightness, and allowed the knobs to be taken further apart without the sparking ceasing; but when the sheet was placed at about 2·5 metres further back, the sparking ceased, and could not be obtained again by screwing up the knobs. On the other hand, when the sheet was placed at double this distance (about 5 metres), the sparking was slightly greater than without the sheet.

Now these *three* observations can only be explained by the interference and reinforcement of a direct action of the "vibrator" with one reflected from the metallic sheet, and in addition by the supposition that the action spreads out from the vibrator at a finite velocity. According to this explanation, in the first position the reflected part combines with the direct and reinforces its effects. In the second position—that of no sparking—the reflected effect, in going to the sheet and returning, has taken half the time of a complete vibration of the "vibrator," and so is in the phase opposite to the incident wave, and consequently interferes with it.

If it were possible to tell the direction of the current in a "resonator" at any moment, then, by employing two of them, and placing one just so much beyond the other that the currents induced in them were always in opposite directions, we would obtain directly the half-way length. Now, by reflection we virtually are put in possession of two "resonators," which we are enabled to place at this distance apart, although unable to tell more than whether there be a current or not.

The distance from the position of interference to the sheet is a quarter of the wave-length, being half the distance between these simultaneous positions of opposite effects.

In the third position the reflected wave meets the effect of the next current but one in the "vibrator" after the current it itself emanated from, and since these two currents are in the same direction their effects reinforce each other in the "resonator." This occurs at half the wave-length from the sheet.

The first two observations alone could be explained by action at a distance, by supposing the currents induced in the metallic sheet to oppose the direct action in the "resonator" everywhere, and by also supposing that, in the immediate neighbourhood of the sheet, the direct action is overmastered by that from the sheet, while at 2·5 metres away the two just neutralize each other.

On this explanation, at all distances further the direct action should be opposed by that from the sheet, so that the fact of being increased at 5 metres upsets this explanation. Again, behind the sheet, evidently on this supposition, the two actions should combine so as to increase the sparking, but instead of this the sparking was found to cease on placing the sheet in front of the "resonator."

In performing these experiments the "resonator" circle was always placed in the position in which only the magnetic part of the disturbance had effect. Hertz has also used the other positions of the resonating circle, whereby he has observed the existence of an electric disturbance coincident with the magnetic one, the two together forming the complete electro-magnetic wave.

Ordinary masonry walls were found to be transparent to radiation of this wave-length—that is, of about 10 metres—and some visitors to the opening meeting of the Dublin University Experimental Society, last November, were much astonished by seeing the sparking of the resonating circle out in the College Park, while the vibrator was in the laboratory.

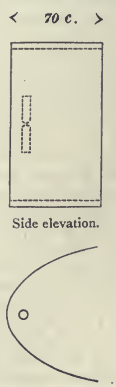
Attempts were first made last December to obtain reflection from the surface of a non-conductor, with the hope of deciding by direct experiment whether the magnetic or electric disturbance was in the plane of polarization; that is, to find out whether the "axis of the vibrator" should be at right angles to the plane of reflection or in it, when at the polarizing angle, for obtaining a reflected radiation. It is to be observed that in these radiations the electric vibration is parallel to the "axis of the vibrator" while the magnetic is perpendicular to it, and that they are consequently polarized in the same sense as light is said to be polarized.

Two large glass doors were taken down and used for this purpose, but without success; and until lately, when reflection from a wall was tried, the experiment seemed unlikely to be successful.

In working with the glass plate, the resonator circle was first placed so that the "vibrator" had no effect on it. Then the glass plate was carried into position for reflection, but without result, though even the reflection from the attendants moving it was amply sufficient to be easily detected.

To obviate a difficulty arising from the fact that the wave was divergent, we decided to try Hertz's cylindrical parabolic mirrors, for concentrating the radiation. Two of these were made with sheets of zinc nailed to wooden frames, cut to the parabolic shape required.

In the "focal line" (which was made 12·5 centimetres from the vertex) of one of these, a "vibrator" was



Side elevation.

Fig. 3.—Plan.

placed, consisting of two brass cylinders in line, each about 12 centimetres long and 3 centimetres in diameter, rounded at the sparking ends.

In order that the "resonator" wire may lie in the "focal line" of the receiving mirror, it has to be straight;

this necessitates having two of them. They each consist of a thick wire 50 centimetres long, lying in the "focal line," and of a thin wire, 15 centimetres long, attached to one end at right angles, and which passes out to the back of the mirror through a hole in the zinc, where the sparking can be viewed, without obstructing the radiation in front. The total length of each "resonator" is about two wave-lengths, the wave-length being about 33 centimetres, so that it may be that there are two vibrating segments in each of these "resonators."

With this apparatus it is possible to deal with definite angles of incidence. No effect was obtained with glass plates using these mirrors, whether the "vibrator" was perpendicular to the plane of reflection or in it. But with a wall 3 feet thick reflection was obtained, when the "vibrator" was perpendicular to the plane of reflection; but none, at least at the polarizing angle,[1] when turned through 90° so as to be in it.

This decides the point in question, the magnetic disturbance being found to be in the plane of polarization, the electric at right angles. Why the glass did not reflect was probably due to its thinness, the reflection from the front interfering with that from the back, this latter losing half a wave-length in reflection at a surface between a dense and a rare medium; and, as Mr. Joly pointed out, is in that case like the black spot in Newton's rings, or more exactly so, the black seen in very thin soap-bubbles. Hertz has pointed out several important things to be guarded against in making these

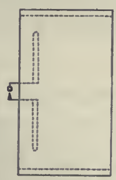


Fig. 4.

experiments. Ultra-violet light, for example, falling on the "vibrator," prevents it working properly, the sparking in the resonator ceasing or becoming poor. Also, the knobs of the "vibrator" must be cleaned, of burnt metal, and polished every quarter of an hour at least, to prevent a like result.

Both these effects probably arise, as suggested by Mr. Fitzgerald, from a sort of initial brush discharging (either ultra-violet light or points being capable of doing this), which prevents the discharging impulse being sufficiently sudden to start the oscillation in the "vibrator." For, to start a vibration, the time of impulse must be short compared with the time of oscillation. These precautions, therefore, become especially needful when working with small-sized "vibrators." Possibly, charging the "vibrator" very suddenly, after the manner of one of Dr. Lodge's anti-lightning-rod experiments, would save the irksome necessity of repeatedly cleaning the knobs of the "vibrator."

Several important problems seem to be quite within reach of solution by means of these Hertzian waves, such as dispersion. Thus, it could be tried whether placing between the reflector and the "resonator" conducting bodies of nearly the same period of vibration as the waves used would necessitate the position of the "resonator" being changed so as to retain complete interference. Or again, whether interspersing throughout the mass of a large Hertzian pitch-prism conductor with nearly the same period would alter the angle of refraction. In some such way as this, anomalous dispersion, with its particular case of ordinary dispersion, may yet be successfully imitated.

[1] Slight reflection was obtained at an incidence of 70°.

The determining the rate of propagation through a a large tile, or sheet of sandstone, could be easily made by means of the interference experiment, by placing it between the screen and the "resonator."

Fred. T. Trouton.

THE SCHOOL OF FORESTRY AT DEHRA DOON, INDIA.

Last year we gave an account of the newly-established School of Forestry at Cooper's Hill, the first of the kind in the United Kingdom, and explained what kind of instruction was there given, and how it was suited to the training of officers for the Indian Forest Department. We now propose to say something of its brother in India—an elder brother, indeed, by some eight years—the School at Dehra Doon, in the North-Western Provinces, now engaged in the education of those who may, not inaptly, be called the non-commissioned officers of the Department. The Dehra Doon is a long valley, which lies at the foot of that portion of the Himalaya which stretches between the great rivers Jumna and Ganges. It is shut off from the great Gangetic plain by a range of hills called the "Siwaliks," known well to all students of palæontological geology as the range in which were found the wonderful series of bones of extinct mammals described by Messrs. Falconer and Cautley. The valley itself lies about 2000 feet above the level of the sea, possesses a beautiful climate free from the blasts of the hot winds which, in April to June, sweep over the plains to the south of it; and is further known historically as having been the site of the first experiments made by the Indian Government in growing the tea-plant, experiments which proved its suitability to India, and made the Doon the fatherland of the great Indian tea industry—an industry which has gradually increased to such an extent that the exports of tea from India and Ceylon now very nearly rival in amount those from the Chinese Empire. Centrally situated in this beautiful valley, among plantations of tea, forests of sál-wood, and groves where the deodar of the Himalaya may be seen alongside of the mango, typical of the Indian plains, and feathery bamboos raise their heads from an undergrowth in which wild or semi-wild roses thrive with luxuriance, lies the town of Dehra Doon, the head-quarters of a Deputy-Commissioner, of the offices of the great Trigonometrical Survey of India, of a regiment of Ghoorka troops, and of the body-guard of the Viceroy. It is rather a straggling town, like most similar Indian stations; but, centrally situated and surrounded by gardens, is found the Forest School, of which we wish to convey some idea to our readers. The School was first started, in 1878, by the exertions of the then Inspector-General of Forests, now Sir Dietrich Brandis, K.C.I.E., and the first Director was Lieut.-Colonel F. Bailey, of the Royal Engineers.

At present the Director is Mr. W. R. Fisher, B.A. of Cambridge University, who is assisted by a Professor of Forestry, Mr. E. E. Fernandez, and a Professor of Geology and Chemistry, Dr. H. Warth. Mr. Fisher himself lectures on forest botany, while other officers, attached to the School for the management of the adjacent forests, teach mathematics, forest law, forest entomology, and surveying, the teaching of the last-named subject being especially fostered by the presence, in the same building, of the office of the Forest Survey, which is now engaged in the preparation of careful detailed maps of the great forest estate which Government possesses in India, and which bids fair to become, not only by its agricultural and climatic effects, but by its financial success, one of the most valuable of the revenue-yielding departments of the Empire.

Attached to the School is a well-equipped museum, containing a magnificent collection of accurately-named

Indian woods; an herbarium, a chemical laboratory, and a meteorological observatory; while the forests of three districts are attached to the School as a training-ground, in which the young students may learn, by personal and actual experience, the conduct of forest operations in the field. The students are usually selected in the different provinces by the Conservators of Forests, and are generally young officers who have seen already some preliminary service. Several have been deputed by the chief native States, such as Mysore and Baroda, and this shows the spread that an enlightened forest policy is making in the country. There are, besides, a number of independent students, who study in the hope of obtaining appointments if successful, either in the British territory or in the native States.

Two courses of study are carried on at the School, the higher in English, leading up to the ranger's certificate, which qualifies the students who succeed in obtaining it for the appointment as "Forest Ranger," on salaries rising from Rs. 600 to Rs. 3000 yearly; the lower, in Hindustani, leading to the forester's certificate, which qualifies the holder for appointments of from Rs. 240 to Rs. 480 per annum. The ranger's course lasts twenty-one months, of which eight are spent in theoretical instruction, and the rest in practical work in the field. The subjects taught are forestry, botany, the elements of zoology, chemistry, physics, geology, mathematics, and surveying, with elementary engineering, such as road-making and the construction of forest export works, and forest law. The forester's course lasts sixteen months, four in theoretical study, and the rest in the field, and the subjects taught are elementary forestry and botany, mathematics, surveying and plan-drawing, and departmental procedure.

The students wear a neat uniform of *kharki*, drill with a turban or helmet, and they are regularly exercised in drill, most of the European and Eurasian students, however, preferring to join the Dehra Doon Corps of Mounted Infantry. When on tour in the forests on practical instruction, each has a small tent, with furniture of a camp-table, chair, and bedstead, and some of them amuse themselves occasionally in sport, one student last year distinguishing himself by carrying off the first prize for shooting in the province.

The forests attached to the School Circle consist of those of the Dehra Doon, Saharanpore, and Jaunsar Forest Divisions. The two former contain chiefly forests of the sál tree (*Shorea robusta*), the chief gregarious tree of India, and the most valuable timber, for building purposes, after teak. They occupy respectively the northern and southern slopes of the Siwalik Range, and are carefully managed as training forests. The Dehra Doon forests are now being worked under a working plan prepared by Mr. Fernandez, the Professor of Forestry. These forests had, till some twenty years ago, been very badly treated, so that at present the older portion of the stock consists chiefly of trees which are crooked and unsound, the good and sound ones having previously been all cut out to provide sleepers for the East Indian, and Sind, Punjab, and Delhi Railways. The present working plan provides for a temporary rotation of twenty years, during which (1) all the old, unsound, and crooked sál trees which can be cut without letting in too much light are removed; and (2) all trees of the less valuable kinds that are not required for shade are cut away. These operations have now been carried on for a few years with the most beneficial results, for the ground is being rapidly covered with good and straight saplings and coppice shoots of sál. The forest operations, the selection of the trees to be cut, and their marking and enumeration, are all done by the students themselves, so that in this way they obtain a valuable amount of practical experience.

The forests of Jaunsar lie on the hills of the outer Himalaya at an elevation of some 5000 to 10,000 feet, and consist chiefly of coniferous trees. The deodar cedar (*Cedrus Deodara*) is, of course, the most valuable of these; then come the pines, the "kail" (*Pinus excelsa*), which so often accompanies the deodar, and the "chir" (*Pinus longifolia*), which forms gregarious forest at the lower elevations. The silver and spruce firs (*Abies Webbiana* and *Smithiana*) also occur, as well as oaks (*Quercus incana*, *dilatata*, and *semicarpifolia*) and other temperate trees. These forests are also carefully treated under working plans, and in them the students of the School learn the management of coniferous forests, the extraction of timber by roads and slides, the planting of blanks in the forest, and the measures necessary for protection against fire and frost.

At the end of their course, and on obtaining their certificates, the students return to the provinces from which they were sent, qualified to carry out ordinary forest works in their own country; and some of them have already obtained promotion into the higher staff of the Department as the reward of their good work, industry, and energy.

The Forest School at Dehra Doon may thus be said to be doing an excellent work, a work which cannot fail to have the best possible effect in the country, and to show the truth of Sir Edwin Arnold's saying that "the Forest Conservancy carried out by the British 'Râj' is one of the greatest benefits to the peninsula."

Soon, perhaps, the extension of forest work will necessitate the establishment of other or branch establishments in Madras, Burmah, and elsewhere; but it is to Dehra Doon that all will look up as the pioneer of scientific forest teaching for the natives of our great dependency.

THE GIANT EARTHWORM OF GIPPSLAND.

THE recently-issued first part of the Transactions of the Royal Society of Victoria contains an elaborate essay (of which we have something to say elsewhere to-day) by Mr. Baldwin Spencer, the newly-appointed Professor of Zoology in the University of Melbourne, on the anatomy of the Giant Earthworm of Gippsland, the largest earthworm yet known. This worm, of which some examples attain to the extraordinary length of six feet, was first described by Prof. McCoy in 1879, and named *Megascolides australis*. It belongs to a peculiar Australian group, of which five species are now known. Mr. Spencer gives us the following general account of its habits:—

Of all the species of *Megascolides* yet known, this one seems to be the largest, and is apparently confined to Gippsland; it is, when found at all, somewhat abundant, and lives principally on the sloping sides of creeks. At times it is found beneath fallen logs, and may be turned out of the ground by the plough.

When first seeking it, we were somewhat puzzled by some of those who were evidently well acquainted with the worm assuring us that the entrance to its burrow was indicated by a distinct "casting"; whilst others, evidently equally well acquainted with the animal, were quite as positive in asserting that it never produced any "casting." Whilst searching, we found what I believe to be the explanation of the contradictory statements, and soon discovered that the surest test of the presence of the worm underground was a very distinct gurgling sound, made by the animal retreating in its burrow when the ground was stamped upon by the foot. When once heard, this gurgling sound is unmistakable, and we at once learnt to regard it as a sure sign of the worm's presence.

The worm very frequently lives in ground riddled by the holes of the land-crab, as it is popularly called; this animal has a small circular burrow leading down to a

chamber hollowed out underneath containing a pool of water, and through these chambers the worms' burrows frequently pass. The "crab" almost invariably has a large conical "casting" at the entrance to its hole, and may raise this to a height of even a foot and more; but the true worm-burrow never, so far as yet observed, has any "casting" at its entrance, and all trace of this is wanting where the crab-holes are absent. The very frequent association of the "crab" and worm leads to the idea that the latter forms a cast; but one of the most noticeable features of the ground, which is at times riddled with worm-burrows only, is the entire absence of "castings." What the worm does with the immense quantity of earth which it passes through its body I cannot at present say, and it must also be noticed that only on very rare occasions can any trace be detected of leaves dragged down into the burrows.

It is no easy matter to extract the worm without injury, owing to its length, the coiling of the burrow, the rapidity of movement which it possesses when underground, and its power of distending either the anterior or posterior ends of the body, or both.

Directly the burrow is laid bare, the worm is seen gliding rapidly away, often producing the curious gurgling sound as it passes through the slimy fluid always

Megascolides australis. Drawing of McCoy's figure ("Prodr. Zool. Vict.," i. pl. vii.).

present in a burrow containing the living animal. Sooner than allow itself to be drawn out, it fixes, if held in the middle, both ends of its body by swelling them out till they are tightly jammed against the sides of the burrow; under these circumstances, pulling merely results in tearing the body. The worm has been described as brittle, but this term is most inapplicable, as its body is very soft, and capable of a great amount of extension before tearing. Its curious smell, when living, resembling somewhat that of creosote, has been already observed by Prof. McCoy, and, when dead, it is worse than ever, and very strong and characteristic; the body, in decaying, passes into an oily fluid, which, we were assured by one or two old natives of the district, is very good for rheumatism. Fowls refuse to touch the worm, living or dead.

When held in the hand, the worm, in contracting its body, throws out jets of a milky fluid from its dorsal pores to a height of several inches; if the burrow be examined carefully, its sides are seen to be very smooth, and coated over with a fluid exactly similar to that ejected from the pores. Whatever be the primary function of the fluid when within the body-cavity, there can be no doubt that it has the important and perhaps secondary function, when it has passed out of the body, of making the burrow walls smooth, moist, and slippery, and of thus enabling the animal to glide along with ease and speed.

The worm in its burrow moves rapidly by swelling up its anterior or posterior end, as the case may be, and then, using this as a fixed point, in doing which the setæ perhaps help, though to a minor extent, it strongly contracts the rest of its body. In the next movement, the end free in the first instance will be swollen out and used as a fixed point, from which expansion forwards can take place. These changes of motion follow each other so rapidly, that in the burrows the appearance of continuous gliding is given. Outside the burrow, when the whole body-surface is not in contact with the earth, the worm makes no attempt whatever to move, lying passively on the ground. Anyone who merely sees the beast removed from its burrow imagines it to be of a very sluggish temperament, and can form no idea of its active and rapid movements when underground.

So far as locomotion is concerned, its setæ seem to be of little or no use to it. The perichæte worms, on the contrary, when taken from the burrow, move along on the ground with remarkable speed, certainly using their setæ as aids to progression.

The burrows of the large worm measure $\frac{3}{4}$–1 inch in diameter; and in disused ones are often found (1) casts of the worms, or rather, what are probably the earthy contents of the alimentary canal, with clear indications marked upon them of the segments of the body; and (2) more rarely cocoons. The latter measure $1\frac{1}{2}$–2 inches in length, vary from light yellow to dark brown in colour, according to their age, and contain only one embryo each, which I have at present only been able to obtain in a somewhat highly developed state.

The cocoon itself is somewhat thin, and made of a tough leathery material, with a very distinct stalk-like process at each end; it contains a milky fluid, closely similar to that found in the body-cavity of the worm.

It is interesting to note the fact that at the present time we know of three especially large kinds of earthworms; that, of these, one comes from South Africa, another from the southern parts of India and Ceylon, and the third from the south of Australia. We know as yet little about the distribution of earthworms, but the same laws which governed the distribution of other animals must also have governed theirs, and it is just possible that these great earthworms may be the lingering relics of a once widely-spread race of larger earthworms, whose representatives at the present day are only found, as occurs with other forms of life, in the southern parts of the large land-masses of the earth's surface. Possibly careful search will reveal the existence of a large earthworm in the southern parts of South America.

NOTES.

In reply to his letter in our columns on the 7th inst. (p. 341), Mr. Sclater has received applications from several unexceptionable candidates for the post of Naturalist to the Pilcomayo Expedition. Out of these, Captain Page has agreed to select Mr. Graham Kerr, of the University of Edinburgh, to accompany him. Mr. Kerr is most highly recommended by Prof. Balfour, Prof. Geikie, and Prof. Ewart as in every way suited for the work. He will leave England for Buenos Ayres about the beginning of June.

It has now been definitely arranged that the steamer *Hvidbjörnen* shall leave Copenhagen on March 15 for Greenland, in order to bring back the members of the Nansen Expedition. The vessel will, however, not be back in Copenhagen until the beginning of June. After a few days' sojourn in that city, Dr. Nansen will proceed direct to Bergen, and prepare a work on his expedition and its scientific results.

In the presence of a distinguished company of men of science, the King of Sweden recently opened the sealed papers containing the names of the two successful competitors for the mathematical prizes offered by him five years ago. The successful competitors were found to be Prof. H. Poincaré, of the Faculté des Sciences, Paris, who receives £160, and M. Paul Appert, Professor in the same Faculty, who receives a gold medal valued at £40. The papers, with reports by Profs. Weierstrass and Hermite, will be published in the *Acta Mathematica*. Twelve papers were sent in for the competition.

We regret to have to record the death, at the early age of twenty-four, of a biologist of great promise, Mr. Richard Spalding Wray, B.Sc. Lond. The son of the Rev. William Wray, a Nonconforming minister in Yorkshire, he early developed a strong taste for natural history pursuits, which led him to become a student at the Normal School of Science at South Kensington, where he eagerly followed the teaching of Prof. Huxley and Mr. Howes. When, at the close of the year 1884, the present Director of the Natural History Museum was seeking some one to assist him in the formation of an elementary series of biological preparations to be placed in the great hall of the Museum, as an introduction to the study of the subjects more fully developed in the special galleries, he fortunately became acquainted with Mr. Wray, who entered with enthusiasm into the project, and soon showed that he possessed every natural capacity requisite for such a work. A neat-handed, skilful dissector, a good mechanician, an excellent draftsman, he displayed great taste and ingenuity in carrying out and often improving upon every suggestion made to him by the Director. While he was engaged in the formation of a series of preparations to illustrate the arrangement of the bones and feathers of the wings of birds, the very insufficient state of the knowledge upon the subject as recorded in ornithological works became apparent, and Mr. Wray made some valuable original observations, which were embodied in a paper "On some Points in the Morphology of the Wings of Birds," published in the Proceedings of the Zoological Society for 1887. This and two minor papers on kindred subjects were all that he was able to communicate to the world, for, unhappily, his powers were greatly diminished by long-continued ill-health, which finally developed into pulmonary phthisis, to which he succumbed on the 12th of this month. He has, however, left a lasting memorial of his patience, ability, and knowledge in the preparations which enrich the Museum; and his simple, modest, unaffected character, and the genuine earnestness with which he entered into the performance of every duty, will not be easily forgotten by those who had the pleasure and advantage of being in any way associated with him.

The Hunterian Oration was delivered on Thursday last, in the theatre of the Royal College of Surgeons, by Mr. Henry Power, senior ophthalmic surgeon to St. Bartholomew's Hospital. Speaking of Hunter's career as a student, Mr. Power pointed out that he took six or seven years to learn anatomy and surgery, whereas in the present day a medical student has only four years to acquire a knowledge of many more subjects. Mr. Power urged that another year is necessary, that it is, in fact, taken by the best students, and that it should be compulsory on all. This, he thought, could be easily obtained if every student were obliged to pass a thoroughly practical examination in chemistry, physics, and elementary biology, before being permitted to register.

In a letter addressed to MM. Henry, of the Paris Observatory, and printed in *La Nature* (February 16), Mr. J. A. Brashear says that the Photographic Society of San Francisco obtained 167 negatives of the recent solar eclipse, the majority of them being very successful. Mr. Brashear himself was able to do good work at Winnemucca, Nevada.

The Naples Correspondent of the *Times*, writing on the 14th inst., says:—"On the 12th inst. a perpendicular shock of earthquake was felt here, lasting about four seconds. It was stronger at the Observatory of Vesuvius, and in the towns at the foot. After the lapse of a minute another shock, the return shock, was felt at the Observatory. 'Meanwhile,' says a reporter from the spot, 'small streams of lava continue to run down on the eastern side, and at the time when we are writing the seismograph is less animated.'"

The Paris Correspondent of the *Daily News*, telegraphing on Tuesday, February 19, says:—"The district of Pont de Beauvoisin, in the Department of the Isère, was disturbed yesterday by a shock of earthquake which lasted about three seconds. Many houses were violently shaken. Field labourers were very much frightened. A good many villages suffered from it, but no lives appear to have been lost."

At the meeting of the French Meteorological Society on January 15, the President, M. Renou, on taking that office for the third time, delivered an address on the progress of meteorology since the establishment of the Society in 1853. He referred to the great improvement that had taken place in the construction and use of the various instruments, and to the progress made in weather prediction, and stated that in order to further improve the system more frequent and direct intercourse by telegraph between the various central offices was necessary, and the extension of telegraphic communication rather than refinement in observations. He also alluded to the importance of automatic or hourly observations at selected stations, and to the differences existing in thermometric exposure, the screens employed in different countries being far from uniform. (That used in France is open to objection, being liable to the influence of radiation.) M. Angot communicated the results of his discussion on the diurnal variation of the barometer, deduced from above fifty stations spread over the surface of the globe, and based on means varying from five to twenty years and upwards. The whole of the values will be published in the Annals of the French Central Meteorological Office.

Mr. H. C. Russell, the Government Astronomer of New South Wales, has published his results of rain and river observations for 1887, and of the meteorogical observations for 1886. The form of publication is the same as before (see Nature, vol. xxxvi. pp. 546 and 566), but the amount of valuable materials dealt with is continually increasing. The rain and river stations for which monthly and annual observations are given amount to 866.

In their fifth Annual Report on the Museum of General and Local Archæology, the Antiquarian Committee of the University of Cambridge call attention to a discovery of unusual interest made in Cambridge at the beginning of 1888. A field was being levelled at the back of St. John's College, when the workmen cut into a Saxon burying-ground. For several days no notice was taken of it; and during that time a number of skeletons and urns (the workmen said several hundreds) had been found and destroyed. As soon, however, as the discovery was made known, steps were taken, with the co-operation of the authorities of St. John's College and Christ's College, the owners and lessees of the land, to have the ground thoroughly examined, under proper supervision, in the interest of the Museum. The excavations occupied more than six weeks, during which time they were never left unwatched. At least thirty skeletons, one hundred urns, and a large quantity of ornaments were discovered. The entire "find" has been placed in the Museum, and forms a most valuable addition to the local Saxon collections.

In an interesting paper on the Eskimo of Hudson's Strait, reprinted from the Proceedings of the Canadian Institute, Mr. F. F. Payne says that, as a rule, the Eskimo deserve to be called

keen observers of Nature. When he was making collections of birds, insects, and plants, he found the natives of great assistance. "If an insect was shown them," he says, "they could usually take me where more of the same species might be found. On the approach of summer they watched with interest its signs, and often would bring to me insects which they believed were the first of the season."

MR. PAYNE found that the Eskimo of Hudson's Strait were much given to the habit of offering food and other things to spirits. By the graves of many of their dead were found scraps of food, tobacco, powder, shot, and other articles, and at first it was supposed that these were offered only to those who had died. To Mr. Payne's surprise, however, a number of like articles were found upon a beacon he had built in the shape of a man. When two cannons, found upon the shore near Cape Prince of Wales, that had undoubtedly been left by some of the early explorers, were made to stand on end, a quantity of bullets, shot, and other rubbish rolled out. On inquiry as to how these things had got there, Mr. Payne was informed they had been given as "an offering to the spirits."

THE extent to which variation may prevail among butterflies is well illustrated in some facts set forth in *The Entomologist* for February, by Mr. W. W. Smith, of Ashburton, New Zealand. The special subject of Mr. Smith's remarks is *Argyrophinga antipodum*, one of the few endemic species of New Zealand Rhopalocera. This butterfly in its season is generally numerous, and owing to its slow and somewhat laboured flight is easily captured. Among the specimens Mr. Smith has taken are some remarkable varieties, exhibiting all the phases or stages of variation to which a single species could be subject. Many individuals of both sexes differ considerably from each other in their ground colouring, the colours of the neuration, and in the number of ocelli. The typical colour of the male is given by Mr. A. G. Butler as "dark greyish brown, paler at base"; in Mr. Smith's specimens every shade of brown is developed, while some are partially melanic forms. Among the females there is also considerable distinction, the general colouring varying from whitish yellow to rich dark orange. In a typical specimen the hind wings are crossed with three small ocelli, the centre one in the male being slightly the largest; among them are several having a broad blackish band crossing the wing from the inner to the outer margin, and inclosing four distinct ocelli. Others have the three ocelli much enlarged and coalescent, while a number possess only two or one ocelli, and in one specimen (a male) they are obsolete on all the wings.

A NEW acid of tin has been obtained by Prof. W. Spring, consisting of two molecules of a higher oxide, SnO_3, combined with one of water. The new acid, $H_2Sn_2O_7$, or $2SnO_3 . H_2O$, is consequently analogous to disulphuric acid, $H_2S_2O_7$, and dichromic acid, $H_2Cr_2O_7$. The method by which Dr. Spring has prepared it is of peculiar interest. A saturated solution of about ten grammes of stannous chloride, in water containing sufficient hydrochloric acid to prevent decomposition into the oxychloride, was treated at the ordinary temperature with excess of peroxide of barium. The latter substance was obtained pure in the form of the hydrate, $BaO_2 . 6H_2O$, by precipitating clear baryta-water with oxygenated water. After the addition of the peroxide the liquid became very thick, and lost most of its limpidity, indicating a change resulting in the production of a colloidal substance. Neither by allowing to stand nor by filtration could any clearing of the solution be effected. But upon subjecting it to dialysis, chloride of barium diffused through the membrane, and was eventually entirely removed by changing the water in which the dialyzer floated every day during a period approaching three months. The contents of the dialyzer were now evaporated as low as possible upon a water-bath; when the evaporation was sufficiently advanced the contents of the dish became converted into a white opalescent jelly, and this eventually dried up into a white solid mass of the new acid. The analyses were most thoroughly carried out, the tin, water, and oxygen each being determined directly, and the numbers obtained are, within the usual limits of error, those required for the $H_2Sn_2O_7$. The oxygen was estimated by passing a current of pure hydrogen over a weighed quantity of the substance contained in a porcelain boat heated to redness in a combustion tube. The water obtained was absorbed by calcium chloride, and weighed, and after deducting the water contained in the substance, the oxygen present in the oxide was readily calculated. As a control, the residue of reduced tin was also weighed. These analyses prove beyond doubt that the tin is here present in the form of trioxide, and that at 100° C. one molecule of water remains combined with it. From certain secondary phenomena Dr. Spring is of opinion that the reaction really takes place in two stages; an oxychloride of tin being first formed by direct addition of oxygen to stannous chloride, $SnCl_2 + BaO_2 = SnOCl_2 + BaO$. This stannic oxychloride appears, then, to react with a further molecule of peroxide of barium with production of barium chloride and trioxide of tin, or hyperstannic anhydride, as Dr. Spring terms the new oxide:

$$SnOCl_2 + BaO_2 = BaCl_2 + SnO_3.$$

The baryta obtained as by-product in the first stage is of course dissolved by the hydrochloric acid present, and the barium is thus entirely removed as chloride upon dialysis.

MESSRS. TRÜBNER have in the press a work on ethics, by Mr. S. Alexander, Fellow of Lincoln College, Oxford, entitled "Moral Order and Progress: an Analysis of Ethical Conceptions." It will be in three books: Book I., Preliminary, dealing with conduct and character; Book II., Statical—Moral Order; Book III., Dynamical—Moral Growth and Progress. The work ought to be interesting to students of science, as the author's conclusions, if sound, will tend to confirm the theory of evolution by showing that the characteristic differences of moral action are such as might be expected if that theory were true. In Book III. he aims at proving that moral ideals follow, in their origin and development, the same law as natural species.

THE Clarendon Press has issued a fourth edition of the second volume of Prof. Minchin's "Treatise on Statics, with Applications to Physics." It is to a very great extent a reprint of the previous edition, but Prof. Minchin explains that it treats much more fully of conical angles; contains new articles on line- and surface-integrals and magnetic shells; and presents an improvement in the method of treating some questions of strain and stress, for which the author is indebted to Prof. Williamson.

THE *Annuaire*, for 1889, of the Royal Observatory of Brussels, by F. Folie, has been published. This is the fifty-sixth annual issue, and the work fully maintains the high standard of excellence attained in previous numbers.

THE first number of a popular scientific periodical—*Der Stein der Weisen*—has just been issued by H. Hartleben, Vienna. It will be published once a fortnight. The editor is A. von Schweiger-Lerchenfeld. If we may judge from the present number, the new periodical is likely to be a decided success. The articles are well written, and there are many illustrations.

MR. S. H. WINTLE contributes to the *Victorian Farmers' Gazette* an account of a mineral substance found in the slightly decayed heart of a beech-tree, *Fagus Cunninghami*, cut down, and split up for firewood, at Gladstone, Mount Camera, Tasmania. A mass of the substance, about one pound in weight, was sent to Mr. Wintle for examination. Analysis proved it to be oxalate of potassium—the "salts of lemon" of commerce. "Potash, as potash," says Mr. Wintle, "enters largely into the

composition of vegetable matter. It is to be found in the ashes of all timber and plants in association with lime and soda; but, in this case we find the potash, or more correctly speaking, the oxide of the metal potassium, is in chemical combination with oxalic acid; thus forming a compound salt, highly crystallized. The paradoxical feature of the phenomenon is presented by the query—Where did the oxalic acid come from to combine with the oxide of potassium? Analysis has failed to detect oxalic acid in the wood or leaves of the *Fagus*. There are certain well-known plants which furnish oxalic acid. Notably, the *Oxalis acetocella*, from which it used to be extracted before the great advances of chemistry enabled man to be independent of that plant as its source. Combined with lime it has been found in some lichens, while the roots of rhubarb and bistort contain it in small quantity in combination with potash; but this is the first instance, it seems, of this organic combination being found in a solid, compact, crystallized form, especially in the heart of a tree."

THE fiery sunsets which revealed the existence of the Krakatão dust in the atmosphere were also noticed by Prjevalsky in November and December 1883, as he was crossing the Gobi and the Northern Ala-shan. He describes them as follows in his "Fourth Journey to Central Asia":—"After a bright day, which is here the usual state of the weather during the winter, light cirrus and cirro-stratus clouds appeared in the west, just before sunset, or immediately after. Probably they were floating all day long in the upper strata of the atmosphere, but became visible when the sun went beneath the horizon. Immediately after that, the whole of the western part of the sky became lighted by a bright cream light, which soon acquired a violet colour in the upper parts, with stripes of shadows. At the same time the shadows of the night rose in the east, dark lilac in the lower parts, and violet in the upper parts. The violet colour vanished by and by in the west, and a segment of bright orange colour appeared close by the horizon, on a cream background. Sometimes it acquired a light red colour, but sometimes it became bright red or even blood-red. In the meantime the lilac colour disappeared in the east, and all the sky became of a gray-lilac colour. Amidst the changing colours in the west, Venus glowed like a diamond descending beneath the horizon at the time when the twilight, which lasted for about one hour and a half, came to an end. During nearly all that time the glow in the west was casting shadows, and all objects in the desert appeared in a fantastic light. The sunrise was accompanied by the same phenomena, but in a reverse order: sometimes the morning twilight began with the appearance of a blood-red colour. At full moon the phenomena were less striking, and in the atmosphere of Northern Ala-shan, which is charged with dust, we saw them less often than in the Central and Northern Gobi."

A HUGE Greenland whale, 90 feet in length, after having been seen in various parts of the Cattegat, lately went ashore in the Sound, and was killed. During the previous twenty years a whale had not been seen in these waters. The skeleton is to be forwarded to the Copenhagen Museum.

PROF. O. TORELL AND DR. TRYBOM have petitioned the Swedish Government for funds sufficient to enable them to continue their researches on the sea fisheries of Sweden, and to establish a biological station on the west coast.

LAST summer Dr. Th. Thoroddsen effected some further explorations in the interior of Iceland, visiting parts hitherto untraversed. It is said that in Norse times, in the district west of Hecla, by the River Thjorsaa, a numerous population inhabited a fertile valley, which was laid waste in 1343 by a terrible volcanic eruption of the Raudukambar Mountain. Dr. Thoroddsen now reports that this mountain is not a volcano at all, and that in historical times no volcanic eruption has devastated this valley; but he is of opinion that the colony in question was destroyed through an eruption of Hecla in the middle of the fourteenth century. Dr. Thoroddsen afterwards explored the desert land south of the Hofsjökul, particularly a mountain range, called Kjerlingarfjöll, close to the south of it. These mountains had never been explored before, and Dr. Thoroddsen found a country which he describes as very remarkable. It was known in the low lands that there were some valleys with hot springs, steam having been seen from a distance, but the springs had never been visited. Dr. Thoroddsen found grand sulphur springs in great numbers, which, he states, far excel the well-known ones at Myvain and Krisuvik. There are also numbers of large boiling mud pools—blue, red, yellow, and green in colour—whilst steam penetrates everywhere through fissures in the earth with terrific noise. One steam jet, 6 to 9 feet in height, kept up such a continual roar that it was impossible to hear the loudest shouts for a long distance. Several subterranean cavities were also found containing boiling clay pools, and around one of them the earth trembled far and wide, whilst far down below in the earth a noise was heard like that which might proceed from a gigantic butter-churn. The valleys in which these springs and mud pools are found are surrounded by extensive and deep snow-fields with innumerable fissures, through which the roar of the steam far below the snow can be heard in some places, whilst in others the steam escapes through them. The ground in these valleys is so hot that only with the greatest care is it possible to tread on the thin crust of clay covering the boiling mud below.

THE additions to the Zoological Society's Gardens during the past week include a Ring-tailed Coati (*Nasua rufa* ♀) from Brazil, presented by Mr. N. T. Williams; three Herring Gulls (*Larus argentatus*), British, presented by Mr. L. V. Harcourt; six Moorish Geckos (*Tarentola mauritanica*) from the south of France, presented by Masters F. and O. Warburg; a Thigh-striped Wallaby (*Halmaturus thetidis* ♀) from New South Wales, deposited; a White-throated Capuchin (*Cebus hypoleucus*), a —— Capuchin (*Cebus* sp. inc.) from Central America, purchased; an Eland (*Oreas canna* ♀), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE MULTIPLE STAR ζ CANCRI.—This remarkable stellar system has justly attracted much attention ever since Sir W. Herschel discovered in 1781 that it was really composed of three, not two stars, the principal star being itself a close double. But the interest with which the system was regarded was greatly increased by the remarkable paper which Prof. O. Struve produced upon the subject, and communicated to the Paris Academy of Sciences in 1874 (*Comptes rendus*, vol. lxxix. p. 1463), and in which he pointed out a noteworthy inequality in the motion of the distant companion C, having a period of about twenty years. The question was again taken up by Prof. Hugo Seeliger in 1881, in a paper entitled "Ueber die Bewegungverhältnisse in dem dreifachen Sternsystem ζ Cancri," and presented to the Vienna Academy of Sciences on May 5 of that year. Prof. Seeliger has continued his discussion of the observations of the star, and has recently published a further paper on the subject, which appears in the Memoirs of the Royal Bavarian Academy of Sciences, Munich, under the title "Fortgesetzte Untersuchungun ueber das mehrfache Sternsystem ζ Cancri." The result of his further labours has been in effect to confirm the results he had obtained in his earlier work, and those which Prof. Struve had brought out in 1874.

The three stars A, B, and C, have the magnitudes respectively 5·0, 5·7, and 5·3. The proper motion of the system amounts in a century to + 10″·6 in R. A., and to − 11″ in Decl. The close pair, A and B, first separated by Herschel, have a motion round one another in about sixty years, their apparent distance from each other varying from about 0″·6 to 1″·1; whilst C, the more distant companion, has moved through about 55° of position-angle round the other two since Herschel's

observation in 1781, its distance never very greatly varying from $5\frac{1}{2}''$. The motion of A and B round their common centre of gravity does not appear to be disturbed to any appreciable extent by the influence of C, which is so placed as not to affect their apparent relative motions, even though a very considerable mass be assigned to it, and as a fact Prof. Seeliger finds, for the most probable value of the mass of C, $\frac{m'}{1+m} = 2{\cdot}386$, where 1, m, and m' are the masses of A, B, and C respectively. But there is a periodical retrogression of C itself which is most easily accounted for by supposing the presence of a close companion, one hitherto undetected, and therefore either entirely dark, or but faintly luminous. The distance of this companion is probably only a few tenths of a second, the distance of C from the point, S_2, round which it appears to revolve, and which may be reasonably assumed to be the centre of gravity of itself and of D, the as yet undiscovered fourth member of the family, being only about one-fifth of a second.

The entire group therefore may be considered as a double-double, the following being the definitive elements derived for the two pairs:—

	For A and B.		For C about S_2.
T ...	1868·112	... ...	1860·127
☊ ...	109°·735	1850·0	71°·958
λ ...	80·190	1850·0	109·677
i ...	11·135	1850·0	17·352
e ...		... ...	0·1106
a ...	0″·853	... ...	0″·217
ϕ ...	22·450	... ...	
n ...	− 6·0898	.. ...	− 20·460

For the motion of S_2 round the optical centre of A and B:—

$$\rho_0 = 5''{\cdot}438.$$
$$p_0 = 145^\circ{\cdot}46 - 0^\circ{\cdot}513\,(t - 1850{\cdot}2).$$

The concluding portion of this valuable contribution to the study of a most interesting case in stellar physics is devoted to the consideration of personal errors in the observations, and a plate is added giving a graphical representation of the apparent motion of C, and bringing out in a striking manner the evidence the observations afford of the looping of the curve.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 FEBRUARY 24—MARCH 2.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 24

Sun rises, 6h. 57m.; souths, 12h. 13m. 21·5s.; sets, 17h. 30m.: right asc. on meridian, 22h. 31·6m.; decl. 9° 16′ S. Sidereal Time at Sunset, 3h. 49m.

Moon (New on March 1, 22h.) rises, 2h. 59m.; souths, 7h. 13m.; sets, 11h. 23m.: right asc. on meridian, 17h. 30·3m.; decl. 20° 54′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian h. m.	Declination on meridian ° ′
Mercury..	6 5 ...	11 2 ...	15 59 ...	21 19·9 ...	12 49 S.
Venus ...	8 2 ...	15 2 ...	22 2 ...	1 20·3 ...	11 1 N.
Mars ...	7 49 ...	14 2 ...	20 15 ...	0 21·1 ...	1 46 N.
Jupiter ...	4 1 ...	7 56 ...	11 51 ...	18 13·8 ...	23 5 S.
Saturn ...	15 17 ...	22 53 ...	6 29* ...	9 12·8 ...	17 21 N.
Uranus ...	21 40* ...	3 4 ...	8 28 ...	13 20·8 ...	7 49 S.
Neptune..	9 49 ..	17 32 ...	1 15* ...	3 51·1 ...	18 27 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb.	h.	
25 ...	1 ...	Jupiter in conjunction with and 1° 11′ south of the Moon.
27 ...	5 ...	Mercury stationary.
28 ...	2 ...	Mercury in conjunction with and 4° 18′ north of the Moon.

Meteor-Showers.

	R.A.	Decl.	
Near β Trianguli	30° ...	35° N.	
,, δ Virginis	192 ...	2 N.	
,, α Serpentis	235 ...	10 N. ...	Swift; streaks.
	280 ...	17 S. ...	Very swift.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5 ...	81 17 N. ...	Feb. 27,	18 28	m
T Arietis	2 42·1 ...	17 3 N. ...	,, 27,		M
Algol	3 1·0 ...	40 32 N. ...	,, 28,	5 52	m
R Geminorum ...	7 0·7 ...	22 53 N. ...	,, 26,		m
R Canis Majoris ...	7 14·5 ...	16 11 N. ...	,, 25,	1 30	m
		and at intervals of		27 16	
T Geminorum ...	7 42·6 ...	24 1 N. ...	Mar. 1,		M
U Virginis	12 45·5 ...	6 10 N. ...	,, 2,		M
U Coronæ	15 13·7 ...	32 3 N. ...	Feb. 25,	5 8	m
X Cygni	20 39·0 ...	35 11 N. ...	,, 28,	2 0	m
T Vulpeculæ ...	20 46·8 ...	27 50 N. ...	Mar. 2,	0 0	m
Y Cygni	20 47·6 ...	34 14 N. ...	Feb. 24,	17 40	m
			,, 27,	17 40	m
δ Cephei	22 25·0 ...	57 51 N. ...	,, 28,	3 0	m

M signifies maximum; m minimum.

GEOGRAPHICAL NOTES.

AN Antarctic Expedition is being again talked of. A New Zealand colonist (of Norwegian origin) has come to Europe for the purpose of taking out a contingent of his countrymen accustomed to fishing. His object is to endeavour to organize an Antarctic whale fishery. He hopes to equip two steamers with which to explore the region generally, and, if possible, he will leave a contingent of men on Victoria Land, or some other suitable point, for a whole year. One or more scientific men will be taken, so that if the proposed expedition be carried out we may expect some important results.

DR. HUGO ZÖLLER (sent out by the *Kölnische Zeitung*) has been doing some original exploring work in German New Guinea. He made an excursion for a considerable distance into the interior. In November last he ascended the Finisterre Mountains to a height of 9000 feet. Some of the peaks in this and the Bismarck Ranges rise to a height of over 10,000 feet.

CAPTAIN PAGE, who recently read a paper on the Gran Chaco at the Royal Geographical Society, proceeds shortly to the Argentine Republic for the purpose of thoroughly exploring the Pilcomayo. He will probably be accompanied by a naturalist.

THE French are endeavouring to raise the funds for a Congo railway which will pass entirely through French territory, in opposition to the scheme for a railway from Vivi to Stanley Pool, for which a survey has recently been made by Belgian engineers. The French railway would run from Brazzaville, on the north side of Stanley Pool, to the River Kwilu, 100 kilometres. Steps, it is stated, will be taken to render the Kwilu navigable, and so establish direct communication between the Congo and the Atlantic.

IN a long article in the new number of the *Mouvement Géographique*, the question of the origin and course of the Lomami, one of the great southern tributaries of the Congo, is discussed. The conclusion is that it is the same river which Cameron crossed far to the south, and which has been crossed at various points further northwards. It enters the Congo some distance below Stanley Falls. Its course is probably about 1000 miles in length.

DR. OSCAR BAUMANN contributes to the February number of *Petermann's Metteilungen* a short monograph (with map) on the district of Usambara, in East Africa. The monograph ought to be specially interesting to geologists.

THE February number of the *Scottish Geographical Magazine* contains several very useful articles. Colonel Cadell, Chief Commissioner of the Andaman Islands, gives a highly interesting account of the group, and especially of its people, who, he maintains, have been very much maligned from the days of Marco Polo downwards. The people are fast dying out. Dr. Guppy sends a preliminary note on the geological structure of the Sindang-Barang district on the south coast of Java. Dr. Guppy sums up the structure of this part of Java as follows: a basis of massive volcanic rocks overlain by submarine tuffs and volcanic muds as far as twelve miles from the coast, and by older and allied tuffs farther inland. The upheaval in post-Tertiary times has been very great, and can only be measured by several thousands of feet. Mr. S. P. Ford gives a brief *résumé* of our knowledge of the geography of the Transvaal; and Mr. W. A. Taylor supplies a real want in his account of the Philpпine Islands, compiled from various recent sources.

NOTES ON METEORITES.[1]

VIII.

THERE can be little doubt that it is to the varying conditions produced by the outflows in both directions along the radius vector, to which reference was made in the last article, that the various appearances put on by the axis of comets' tails are due. Thus, in Coggia's comet, to take an instance, the perihelion passage of which took place on August 27, on June 10 the axis was brighter than the rest of the tail, but by July 10 the bright axis was replaced by one of marvellous blackness, which was one of the features of the comet at that time, and this dark axis expanded as perihelion was approached.

The tail is always curved, but if the earth lie in the plane of the orbit the curvature cannot be seen.

FIG. 27.—Great comet of 1861, seen on June 30, when the earth was in the plane of the orbit.

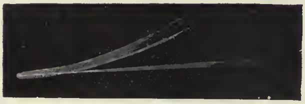


FIG. 28.—Same comet seen on June 15.

The accompanying woodcuts will explain how the solar repulsion produces this curvature, and how the curvature will depend upon the velocity due to repulsion.

FIG. 29.—Slight repulsion ; great curvature.

Fig. 29, which I owe to M. Faye,[2] represents the successive positions of a series of molecules emitted by the nucleus of a comet so as to constitute the axis of the tail. A density is imagined such that the repulsive force exactly counterbalances the solar attraction : thus their motion, solely due to the tangential velocity of the comet, takes place in a straight line. To again simplify matters, this rate is supposed constant, as if the orbit were a circle.

On the first day, the comet being at C^1, a molecule m^1 is detached and subsequently follows the line m^1 m^1 m^1. On the second day, a molecule m^2, likewise leaves the nucleus at C^2, and subsequently describes the tangent $m^2\, m^2\, m^2$. Similarly, on the third day, for a molecule m^3 ; and so on. If we join by a continuous line the series of positions occupied at the same time, the fifth day, by all these molecules, m^5, m^4, m^3, m^2, m^1, we shall have the curvilinear axis of the tail ; this will be, in this particular case, the involute of a circle. This construction accounts for the three laws which have been ascertained as the result of observation : (1) the tail, at its origin, is sensibly opposed to the sun, S ; (2) the tail is curved backwards on its path ; (3) the axis of the tail is a plane curve situated in the plane of the orbit.

If the density of these molecules were still smaller, the repulsive force would prevail over the solar attraction, and the molecules would describe no longer straight lines, but sections of an hyperbola whose convexity would be turned towards their common focus, S (see Fig. 30).

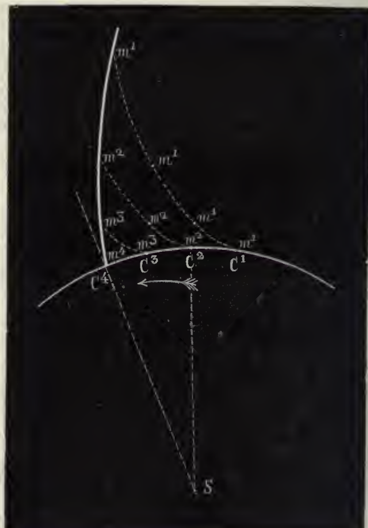


FIG. 30.—Here the velocity is greater and the tail is straighter.

The series of points m^1, m^2, m^3, m^4, emitted at C^1, C^2, C^3, C^4, by the comet, gives a curve like the former one, but with a curvature much less pronounced and nearer to the radius vector.

Now the single tail we have been considering will depend upon the repulsive action upon molecules of similar density, and that very small.

But suppose there are in consequence of collisions among the members of the swarm, several gases given off which can retain their gaseous form, and suppose they are of different densities. Then it is evident that a winnowing process will be set up, and that the molecules of smallest density will be repelled with the highest velocity ; and given these varying densities, we must get more tails than one—one, in fact, for each representative density.

M. Bredichin, of the Moscow Observatory, has in fact shown that there are three distinct types of tails. In the first class. the tails are long and straight, and the repellent energy of the sun upon the small particles is about twelve times as great as the energy of his gravitational attraction. The particles therefore leave the nucleus with a high velocity, generally about fourteen or fifteen thousand feet per second. The greater this velocity in relation to the rate of travel of the comet, the straighter of course will be the tail, because the particles forming it do not lag behind. In the second type, the energies of the

[1] Continued from p. 236.
[2] "Forms of Comets," NATURE, vol. x. p. 268.

attraction and repulsion balance each other, or nearly so, and the tails of this class are plumy and gently curved. In this case the particles which go to form the tail leave the head with a velocity of about 3000 feet per second. Tails of the third type are short and strongly bent, the repellent energy being only about one-fifth of the attractive energy of the sun, and the velocity of the particles leaving the head is only about 1000 feet per second.

Many comets exhibit tails of more than one type, and it was conjectured long ago that such tails were composed of different kinds of matter.

Bredichin went further, and defined the composition of the different kinds of tails which he had classified, by referring to the molecular weights of the materials which would give the relative values of the repulsive and attractive forces necessary for tails of the different types. He thus found that the long straight tails of the first type would be probably formed by hydrogen, since this substance, on account of its exceeding lightness, would be little influenced by gravity, while at the same time strongly influenced by the solar repulsion. The second type of tails he considered to be made of hydrocarbons, since hydrocarbons have a molecular weight such that the repellent and attractive forces of the sun upon their particles may be nearly equal. Iron, on the other hand, would be more subject to the action of gravity, on account of its greater weight, and was therefore taken as adapted to tails of the third type.

There is nothing extravagant in these suppositions, for we know that all the substances in question do exist in comets, and it is evident that much is to be learnt from a continuation of the inquiry, but at the outset we can see that iron vapour cannot in space remain as vapour to form a tail.

We know that the short-period comets get less brilliant with every approach to perihelion, and that some do not even throw out a tail, and we can easily ascribe both these results to the fact that after several such appulses the vapours liable to be driven out of the meteorites by temperature get less and less.

If this be so, we may regard a comet with many tails as one which for the first time undergoes perihelion conditions.

If it be conceded that the tails of comets are in part composed of hydrogen and compounds of carbon with gases such as oxygen, an explanation seems to be suggested of many recorded phenomena, while at the same time it seems more probable that the repulsive force would act continuously upon permanent gases rather than on condensable vapours, such as iron vapour, to take an instance.

Suppose that the sun has been formed by the coming together of meteors, whether brought by comets or not, it is obvious that with equal temperatures *of the sun* the repellent action would be the same on the permanent gases given off by the meteorites, whether in large or small groups. In the larger groups there would be possibly more collisions, and therefore greater possibilities of higher temperature *of the meteorites*.

This action would surround the sun, as it were, by a cordon, inside which, to take instances, neither hydrogen nor oxycarbon-compounds could enter. Hence we should have a sun without hydrogen, carbon, or oxygen.

But while, as demanded on this view, the quantity of carbon and oxygen is extremely small, even if the latter exists at all, the quantity of hydrogen is enormous.

This difference can, however, be accounted for by the idea which has been suggested on several other grounds, that the hydrogen which plays such an important part in the sun's economy and in the economy of all stars hotter than the sun is really produced locally by the dissociation of the vapours of the chemical elements which form the sun and the meteoritic constituents which still reach it in the shape probably of iron and silicates.

We know perfectly well (from Dunér's work chiefly, in stars of the class III.*b*) that when the sun gets cooler its atmosphere will consist almost exclusively of carbon compounds; and indeed one of the last scenes in the drama of world-formation seems to be the gradual approach of the "cordon" to which I have referred, as the radiant energy of the star is diminished, thereby enabling all the permanent gases in the system to gradually approach the primary; and it is not impossible that the great differences of density of the interior and exterior planets may be connected with this state of things.

Before passing on, it is well to recur to the question, Why should not *vapours* be also repelled from the cometary nucleus and its envelopes?

No doubt they are; but it is straining the facts to suppose that they would not be condensed by the cold of space before they had been repelled any great distance; the enormous lengths of some comets' tails would seem to negative any such possibility.

Some of these lengths may be given in miles:—

Comet	1843 (I.)	198,800,000 miles
,,	1680	149,000,000 ,,
,,	1847 (I.)	130,800,000 ,,
,,	1811 (I.)	109,400,000 ,,
,,	1860 (III.)... ...	21,700,000 ,,

With regard to the rate at which the tails are thrown out it may be stated that, in the case of Donati's comet, between September 23 and October 10 the tail had increased from 15,000,000 to 55,000,000 miles, or, speaking roughly, the tail had increased by 2,000,000 miles a day.

If we are justified in considering that the materials of the comet thus repelled to form the tail are non-condensable gases, such as the hydrogen and the carbon compounds which are actually found in meteorites, we have in this fact probably the *vera causa* of the so-called occlusion of these gases by meteorites. That is, one set of meteorites—a comet—may be giving off these gases, while other meteorites, which have never been members of such a large swarm, may occupy regions of space swept over by the gases repelled from the comet.

But if it be agreed that it is not probable that, say, the vapours of iron and magnesium could retain their vaporous condition so long as the hydrogen and the carbon compounds—there can be no doubt that they start on the common journey in consequence of the repulsive action outside the track of the comet—then we shall expect to find condensed particles of iron, nickel-iron, and magnesium or their compounds; and here again we have a *vera causa* for the chondrites which enter so largely into the composition of meteorites.

The tail of a comet being thus formed at the expense of the materials composing the head, the materials removed from the head can never be returned to it because of its insufficient gravitational power over them, and moreover they can no longer traverse the same orbit as the comet to which they originally belonged, because they have already been turned out of that course by the forces attending the development of the tail. The small tail-forming meteoric bodies thus become distributed throughout the space occupied by our system, and give no further trace of their existence, unless they happen to break into our atmosphere and appear as shooting-stars.

Comets must thus degenerate, so far at all events as their easily volatilized constituents are concerned, with each perihelion passage, but as the majority of them only approach the sun at long intervals of time they do not suffer much in this way. Some of the short-period comets get less and less brilliant at each successive perihelion passage, and others are then observed entirely without tails, all the available tail-forming material having been used up and dispersed into space.

It is a fact well worthy of consideration that on many occasions pulsations exactly resembling those observed in auroræ have been observed in comets' tails.

This subject is thus referred to in Guillemin's book on comets:—

"Kepler is the first observer who has made mention of the changes. 'Those,' he says, 'who have observed with some degree of attention the comet of 1607 (an apparition of Halley's comet) will bear witness that the tail, short at first, became long in the twinkling of an eye.' Several astronomers—Kepler, Wendelinus, and Snell—saw, in the comet of 1618, jets of light, coruscations, and marked undulations. According to Father Cysatus, the tail appeared as if agitated by the wind; the rays of the coma seemed to dart forth from the head and instantly return again. Similar movements were observed by Hevelius in the tails of the comets of 1652 and 1661; and Pingré, describing the observations of the comet of 1769, made at sea, between August 27 and September 16, by La Nux, Fleurien, and himself, thus describes the phenomenon of which he was a witness:—'I believe that I very distinctly saw, especially on September 4, undulations in the tail similar to those which may be seen in aurora borealis.' The stars which I had seen decidedly included within the tail were shortly after sensibly distant from it.

"M. Liais has given the following account of the observations made by him of the great comet of 1860:—'On the evening of July 5, whilst I was observing the comet at sea, I saw a rather

intense light from time to time arise in those portions of the tail that were furthest from the nucleus. Sometimes instantaneous, and appearing upon a small extension of the extremity of the tail, which then became more visible, the fugitive gleams reminded me of the pulsations of the aurora borealis. At other times they were less fleeting, and their propagation in rapid succession could be followed for some seconds in the direction of the nucleus near the extremity of the tail. These appearances then resembled the progressive undulations of the aurora borealis, but even in this case they were only visible in the last third of the length of the tail. The gleams in question were similar to those that I remember to have seen in the tail of the great comet of 1843, and which were observed by very many astronomers.'"

The American observers of Donati's comet in 1868 described a number of brighter bands "like auroral streamers" crossing the tail and diverging from a point between the nucleus and the sun.[1]

This point is one well worthy of subsequent inquiry. I have brought together evidence to show that in the aurora one of the chief factors in the production of the spectrum is meteoric dust.

If this be conceded, we have meteoric dust in all probability very slowly falling through our atmosphere at a height at which its pressure is very low, the luminosity both of the dust and the atmosphere being produced by electricity. Whether the electricity is produced by the movement is a matter on which at present we are quite ignorant, but if it be eventually shown that all auroræ follow well-recognized star-showers a certain amount of plausibility will be accorded to the notion.

However this may be, we must in the case of the aurora regard the permanent gases in the air as a constant, and the dust as the variable.

But if we wish to assimilate these displays with comets' tails, we must in the latter case consider meteorites in space as the constant, and the permanent gases repelled from the comet as the variable.

Prof. Tait, assuming that the head of a comet is a swarm of meteorites or stones, varying in size from a marble to boulders 20 or 30 feet in diameter, has shown that all the various cometary phenomena may be explained. His researches have not yet been printed *in extenso*, but the following general statement gives a summary of the results of his calculations which appeared in *Good Words* some time ago.

Firstly, with regard to the masses of the comets. The total mass of a comet cannot be very great, for, as we have seen, no measurable disturbance of planetary orbits has been known to be produced, and this small mass is just as likely to be due to scattered solid masses as to one continuous gaseous mass, and indeed we know that this is so. In the case of comets of but small masses, the component meteorites would be small and far apart. Then with regard to the transparency of the comet, it is calculated that a meteorite 25 feet in diameter at a distance of half a million miles from us could not totally eclipse a star of the same size as our sun, even if it were at such a distance as to be barely visible to the naked eye. Again, if some of the meteorites were large enough to eclipse the stars behind the comet, the eclipse would be of very brief duration, and we should see the star as if nothing had happened. In order for the comet to reduce the light of a star seen through it by one-tenth, it would require to be 300 miles thick, supposing the stones to be 1 inch cube and 20 feet apart.

While the swarm which builds up the comet is coursing round the sun as a whole, the individual members will themselves gravitate towards each other; and if we suppose the whole mass to be 1/1000 that of the earth, and the meteorites to be uniformly distributed in a sphere 20,000 miles in diameter, those coming from the outside to the centre of the group would have a velocity of about 500 feet per second. The stones colliding will generate heat, and some gas will be evolved; some members of the mass will be quickened, while other constituents of the mass will be retarded in their motion, and in this way we have a probably sufficient explanation of the various forms which the telescope has revealed to us. And then finally Prof. Tait goes on to show that the result of these collisions would be such a smashing up of the constituents of the swarm that much finely-attenuated material would be left behind, sufficient to reflect sunlight, and to give rise to the phenomena of the tail.

[1] Webb, p. 197.

If in the imaginary swarm the mass of each stone be 100 pounds, and its velocity, due to attraction, be 500 feet per second, the heat resulting from the impact of two of them would be quite sufficient to volatilize a portion, and to make the outsides of the stones white-hot. Stones of this weight would be about 10 inches cube, and in the swarm considered there would therefore be about 136,000,000,000,000,000,000 of them. At the rate of one collision per second, there being about 31,436,000 seconds in a year, there would be a possibility of one collision per second for 2,150,000,000,000 years. There would therefore be material for such collisions for a period of over two million years even at the extravagant rate of one million per second, and on the assumption that no stone comes into collision with another more than once.

The whole mass being 1/1000 that of the earth, and the space occupied being 250 times that occupied by the earth, the stones in question being 10 inches cube will only occupy about 1/8000 of the space through which they are distributed; the average distance apart would be about 17 feet. The swarm would reflect about half as much sunlight as a slab of the same material in the same place, but it would probably be too opaque to transmit starlight. By making the stones larger, and thus increasing the distances between them, the luminosity would be retained, while at the same time the swarm would be sufficiently transparent. It thus seems to suit the hypothesis better if we regard the separate stones to be greater than 10 inches cube.

J. NORMAN LOCKYER.

(*To be continued.*)

THE FORCES OF ELECTRIC OSCILLATIONS TREATED ACCORDING TO MAXWELL'S THEORY. BY DR. H. HERTZ.[1]

I.

Note by the Translator.

THE early part of the following paper is no doubt familiar to the more important persons in this country, and therefore need perhaps hardly have been translated. Nevertheless, as these experiments of Hertz form a sort of apotheosis of Maxwell's theory, it is natural to reproduce this portion as well as the rest; and inasmuch as Hertz did not at first express his discoveries in Maxwellian language, it is interesting to see how he regards the matter since his conversion, and how he now presents his ideas to foreigners.

I have translated the paper because it seems to me a remarkable example of clear theoretic insight in conjunction with great experimental skill, because it is pleasantly written, and because it deals in a powerful manner with a profoundly interesting subject.

I can hardly hope to have escaped errors in translating, but the original paper in *Wiedemann's Annalen* for January of this year is very accessible. OLIVER J. LODGE.

The results of the experiments on quick electric oscillation which I have carried out appear to me to lend to Maxwell's theory of electrodynamics an ascendancy over all others. At first I interpreted these experiments in terms of older notions, seeking to explain the phenomena in part by means of the cooperation of electrostatic and electro-magnetic forces. To Maxwell's theory in its pure development such a distinction is foreign. I wish, therefore, now to show that the phenomena can also be explained in terms of Maxwell's theory without any such distinction. If this attempt succeeds, questions about special propagation of electrostatic force, being meaningless in Maxwell's theory, are at once settled. And besides this special aim, a closer insight into the play of forces concerned in rectilinear oscillations is not without interest.

The Formulæ.

In what follows we have only to concern ourselves with forces in free ether. Let X, Y, Z, be the components of electric force acting on the points x, y, z; let L, M, N be the corresponding components of magnetic force; let t be the time, and let A stand for $\sqrt{(\mu K)}$. Then, according to Maxwell, the time-rate of change of the forces is dependent on their distribution in space in the following way:—

[1] Translated and communicated by Dr. Oliver Lodge.

$$\left.\begin{aligned} A\frac{dL}{dt} &= \frac{dZ}{dy} - \frac{dY}{dz} \\ A\frac{dM}{dt} &= \frac{dX}{dz} - \frac{dZ}{dx} \\ A\frac{dN}{dt} &= \frac{dY}{dx} - \frac{dX}{dy} \end{aligned}\right\} \quad \ldots\ldots (1)$$

$$\left.\begin{aligned} A\frac{dX}{dt} &= \frac{dM}{dz} - \frac{dN}{dy} \\ A\frac{dY}{dt} &= \frac{dN}{dx} - \frac{dL}{dz} \\ A\frac{dZ}{dt} &= \frac{dL}{dy} - \frac{dM}{dx} \end{aligned}\right\} \quad \ldots\ldots (2)$$

Originally, and therefore always, the following conditions must be satisfied:

$$\frac{dL}{dx} + \frac{dM}{dy} + \frac{dN}{dz} = 0, \text{ and } \frac{dX}{dx} + \frac{dY}{dy} + \frac{dZ}{dz} = 0 \quad . . (3)$$

The electric energy contained in a portion of ether of volume τ is—

$$\frac{1}{8\pi}\int(X^2 + Y^2 + Z^2)\,d\tau;$$

the magnetic energy is—

$$\frac{1}{8\pi}\int(L^2 + M^2 + N^2)\,d\tau;$$

the integration extending all through the volume. The total energy is the sum of both these portions.

These expressions form the essential ingredients of Maxwell's theory as it relates to the ether. Maxwell arrived at them by forsaking action at a distance, and by accummodating the ether with the properties of a highly dielectric medium. One can also get the same equations in another way. But hitherto no direct proof of the validity of these equations has been afforded by experience. It appears most logical, therefore, to regard them independently of any way in which they may have been arrived at, to consider them as hypothetical assumptions, and to let their probability depend upon the very great number of legitimate conclusions which they embrace. Taking this point of view, one can do without a series of auxiliary ideas, which render the understanding of Maxwell's theory more difficult, even if on no other ground than that, so soon as one finally excludes the hypothesis of immediate action at a distance, these notions possess no meaning.

Multiply equations (1) by L, M, N, and (2) by X, Y, Z; add the equations together, and integrate over the whole space, whose volume element is $d\tau$, and whose surface element is dS; we get—

$$\frac{d}{dt}\left\{E_e + E_m\right\}$$

$$= \frac{1}{4\pi A}\int\{(NY - MZ)\lambda + (LZ - NX)\mu + (MX - LY)\nu\}dS,$$

where λ, μ, ν are the direction-cosines of the normal to the surface.

This equation shows that the amount by which the energy of the space has increased can be regarded as having entered through its walls. The quantity entering through any single element of surface is equal to the product of the components of the electric and magnetic forces which belong to that element, multiplied by the sine of the angle between them, and divided by $4\pi A$. On this result it is well known that Prof. Poynting has founded a remarkable theory on the transfer of energy in the electro-magnetic field.

For the purpose of solving the equation, we limit ourselves to the special but important case where the distribution of the electric force is symmetrical about the axis of z, and hence that this force is absent at every point of the meridian planes intersecting in the axis of z, and only depends on the z co-ordinate of a point, and on its distance, $r = \sqrt{x^2 + y^2}$, from the z axis. We will denote the electric force in the direction of r, namely, $X\frac{x}{r} + Y\frac{y}{r}$, by R; and the component of the magnetic force which is normal to the meridian planes, viz. $L\frac{y}{r} - M\frac{y}{r}$, by P. We assert further that if Π is any function of r, z, and t, which satisfies the equation—

$$A^2\frac{d^2\Pi}{dt^2} = \nabla\Pi;$$

and if we put $Q = r\frac{d\Pi}{dr}$, the following is a possible solution of our equations:—

$$Z = \frac{1}{r}\frac{dQ}{dr},$$

$$R = -\frac{1}{r}\frac{dQ}{dz},$$

$$P = \frac{A}{r}\frac{dQ}{dt},$$

$$N = 0.$$

To prove this assertion we observe that

$$X = R\frac{dr}{dx} = -\frac{d^2\Pi}{dxdz},$$

$$Y = R\frac{dr}{dy} = -\frac{d^2\Pi}{dydz},$$

$$Z = \frac{1}{r}\frac{d}{dr}\left(r\frac{d\Pi}{dr}\right) = \frac{d^2\Pi}{dx^2} + \frac{d^2\Pi}{dy^2},$$

$$L = P\frac{dr}{dy} = A\frac{d^2\Pi}{dydt},$$

$$M = -P\frac{dr}{dx} = -A\frac{d^2\Pi}{dxdt},$$

$$N = 0.$$

One has only to insert these expressions into equations (1), (2), (3), to find equations (2) and (3) identically satisfied, and (1) also if we have regard to the differential equation for Π.

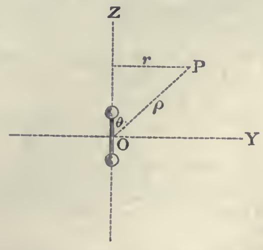
It may be mentioned that also inversely, neglecting certain practically unimportant limitations, every possible distribution of electric force which is symmetrical to the axis of z can be represented in the above form, but it is not necessary for the sequel to substantiate this assertion.

The function Q is of importance. The lines in which the surface of rotation Q = const. cut the meridian planes are the lines of electric force; the construction of the same for one meridian plane furnishes at every instant an immediate presentation of the force distribution.

If we cut the shell between Q and $Q + dQ$ by a surface of rotation round the axis of z, the product of electric force and surface which Maxwell calls the "induction" is for every such surface the same. If we arrange the system of surfaces Q = const. in such a way that Q increases in arithmetic progression, the same statement remains true when we compare the sections of the different shells with one another.

In the plane diagram which consists of sections of the meridian plane with the equidistant surfaces Q = constant, the electric force is inversely proportional to the normal distance of consecutive lines Q = const. only for the case when points compared lie at the same distance from the axis of z. In general the rule is that the force is inversely proportional to the product of this distance and of the co-ordinate r of the point considered.

If we introduce polar co-ordinates ρ and θ they will be like this.



The figure represents an electric oscillator at origin of co-ordinates as intended to be understood by Hertz.

The Forces concerned in a Rectilinear Oscillation.

Let E denote a quantity of electricity, and l a length; let $m = \frac{\pi}{\lambda}$ be a reciprocal length, and $n = \frac{\pi}{T}$ a reciprocal time; and let us put

$$\Pi = E\frac{l}{\rho}\sin(m\rho - nt).$$

This value satisfies the equation—

$$A^2 \frac{d^2\Pi}{dt^2} = \nabla\Pi,$$

so soon as we settle that,

$$\frac{m}{n} = \frac{T}{\lambda} = A,$$

and $\frac{\lambda}{T}$ will be the velocity of light. And, indeed, the introduced equation is satisfied everywhere, except at the origin.

In order to find out what electrical processes are set up by the distribution of forces specified by Π, we investigate its immediate surroundings.

We put ρ vanishing in comparison with λ, and neglect $m\rho$ in comparison with nt.

Then—

$$\Pi = E\frac{l}{\rho} \sin nt.$$

Since, now—

$$\left(\frac{d^2}{dx^2} + \frac{d^2}{dy^2}\right)\frac{1}{\rho} = -\frac{d^2}{dz^2}\left(\frac{1}{\rho}\right),$$

we have—

$$X = -\frac{d^2\Pi}{dxdz}, \quad Y = -\frac{d^2\Pi}{dydz}, \quad Z = -\frac{d^2\Pi}{dzdz}.$$

So the electric forces appear as the derivative of a potential—

$$\phi = \frac{d\Pi}{dz} = El \sin nt \cdot \frac{d}{dz}\left(\frac{1}{\rho}\right);$$

and this expresses an electrical [*Doppelpunkt*, by which I suppose is meant either an involution or a spherical harmonic] whose axis coincides with the z axis, and whose moment oscillates between the extreme values El and $-El$ with the period T.

Our force distribution, therefore, represents the action of a rectilinear oscillator which has the very small length l, and on whose poles at the maximum the quantities of electricity $\pm$ E are free.

The magnetic force perpendicular to the direction of the oscillator is, in the immediate neighbourhood,

$$P = -AEln \cos nt \frac{\sin\theta}{\rho^2}.$$

According to the Biot-Savart law, this is the force of a current element in the direction of the axis of z, of length l, whose intensity, magnetically measured, oscillates between the extreme values $\pm \frac{\pi AE}{T}$. In fact the motion of the electricity E corresponds to a current of that magnitude.

From Π we get—

$$Q = Elm \left\{\cos(m\rho - nt) - \frac{\sin(m\rho - nt)}{m\rho}\right\} \sin^2\theta,$$

and from this the forces Z, R, P follow by differentiation.

The formulæ are too complicated for it to be possible to obtain immediately from them in their general form a representation of the distribution of the forces. For some special cases the results are meanwhile proportionately simple. We get these at once—

(1) The immediate neighbourhood of the oscillator we have already treated.

(2) In the z axis, *i.e.* in the direction of swing, we have $dr = \rho d\theta$, $dz = d\rho$, $\theta = 0$; so then—

$$R = 0, \quad P = 0,$$

$$Z = \frac{2Elm}{\rho^2}\left\{\cos(m\rho - nt) - \frac{\sin(m\rho - nt)}{m\rho}\right\}.$$

The electric force acts always in the direction of the oscillator; it diminishes for small distances as the inverse cube, for greater distances as the inverse square, of the distance.

(3) In the xy plane, or $z = 0$, we have $dz = -\rho d\theta$, $d\rho = dr$, $\theta = 90$, and so—

$$P = \frac{AElmn}{r}\left\{\sin(mr - nt) + \frac{\cos(mr - nt)}{mr}\right\}$$

$$R = 0,$$

$$Z = \frac{Elm^2}{r}\left\{-\sin(mr - nt) - \frac{\cos(mr - nt)}{mr} + \frac{\sin(mr - nt)}{m^2r^2}\right\}.$$

The electric force in the equatorial plane through the oscillator is parallel to the oscillation, its amplitude being—

$$\frac{El}{r^3}\sqrt{1 - m^2r^2 + m^4r^4}.$$

The force decreases with distance, at first quickly as the inverse cube, later only slowly, and inversely as the distance itself. At great distances the action of the oscillator can only be noticed in the equatorial plane, not in the axis itself.

(4) At very great distances we can neglect higher powers of $1/\rho$, compared with lower ones. So we get at such distances—

$$Q = Elm \cos(m\rho - nt) \sin^2\theta,$$

whence—

$$P = A\frac{Elmn}{\rho} \sin(m\rho - nt) \sin\theta,$$

$$Z = -\frac{Elm^2}{\rho} \sin(m\rho - nt) \sin^2\theta,$$

$$R = \frac{Elm^2}{\rho} \sin(m\rho - nt) \sin\theta \cos\theta.$$

Thence follows—

$$Z\cos\theta + R\sin\theta = 0.$$

The direction of the force is therefore at great distances everywhere normal to the radius vector from the origin of force; the spreading out occurs in pure transverse waves. The magnitude of the force is equal to $\frac{Elm^2}{\rho}\sin(m\rho - nt)\sin\theta$, and decreases at a constant distance from the origin towards either axis, being proportional to the distance from this latter.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—A Report on the new Chemical Laboratory states that it has cost in all £33,697, an excess of £3000 above the estimate in 1885. The fittings and machinery have cost £1900 more than was estimated, and the foundations had to be made stronger at a cost of £450. The Financial Board state that they have every reason to believe that the University has full value for the outlay. The building and fittings are substantial and well adapted to their purpose. Prof. Liveing reports that the lecture-rooms are such that the audience can both see and hear, can sit comfortably and write with ease, and that the laboratories will bear comparison with the best of those to be found elsewhere.

£200 is asked for the purchase of additional apparatus for the Pathological Laboratory (the old Chemical Laboratory).

At Cavendish College eight scholarships of £30 per annum will be offered on July 23 next to students who will be under eighteen years old on October 1 next. One or more may be given for natural science (chemistry, physics, botany, geology, in all of which there will be a practical examination). Further particulars may be obtained from the Master of Cavendish College.

SCIENTIFIC SERIALS.

Bulletins de la Société d'Anthropologie de Paris, tome 11, fasc. 3, 1888.—Continuations of M. G. Hervé's observations on the cerebral convolution known as "Broca's." Great importance attaches to the discovery by the late eminent M. Broca of this anatomical characteristic in man, which he found to be absent in all animals below the Anthropomorpha, and while it appears in the latter only in a simple and rudimentary form, it is fully developed in the human brain. This fact in itself gives support to the hypothesis that intermediate types, now lost, must have been interposed between man and the still existing forms of the Anthropomorpha, and yet more important are the results yielded by recent physical researches, which clearly show that the normal human brain possesses a quadruple system of the frontal convolutions due to the doubling of the binary frontal lobes, while in Broca's convolution we must, moreover, recognize the origin and function of speech and memory. In the microcephali, in idiots, deaf-mutes, and in all persons of inferior intelligence, this convolution is more or less atrophied, especially within the insula or centre, where it unites with the other frontal convolutions near the extremity of the olfactory channel.—Close of the statistical inquiry regarding the colours of the eyes and hair in France, by M. Topinard. After having collected the results of 180,000 observations, M. Topinard an-

nounces that he is about to incorporate them in a chart for France. Three memoirs have already been published as parts of this inquiry, embracing Tunis, Denmark, and Pointe du Raz in France. Among various other interesting results the curious fact has been deduced that where the race is of a mixed blonde and brunette character the hereditary blonde colouring is especially manifested in the eyes, while the brunette element has a tendency to reappear in the hair.—On the origin and intellectual evolution of the pointer dog, by M. C. A. Piètrement. This is a *résumé* by the author of a special section of his great work regarding the legislative enactments in force under the Frank kings of the two earliest dynasties, concerning domestic animals. Beginning with a notice of the fourteen species of house and sporting dogs to be recognized on the monuments of Egypt, Assyria, and Nineveh, he goes on to the references in Greek and Roman writers to the dogs known in their time, in which he finds no indication that pointers and setters existed in their present stage of development as sporting dogs before our era; while, on the other hand, he proves by extracts from the lists of birds and other animals used in the chase, given in the capitularies of Dagobert, that falconry, whose introduction into Western Europe is usually ascribed to the Crusaders, was practised in Gaul as early as the seventh century. The article although long and verbose, contains much interesting information regarding sport among the Franks and Gauls since the time of the Romans.—On the long bones found at Spy, by M. Topinard. The sudden angular deviation of the tibial bones leads to the inference that the lower extremities of the men of Spy were half-flexed, as is generally the case in the arboreal Anthropomorpha.—Cut flints found at Pierrefitte, by M. Simoneau. These finds were taken from the lime-beds of Toucy, and were obtained through the agency of shepherd-boys, successfully enlisted in the work of collecting.—On the grouping of pyramidal cellules in the motor region of the limbs, by M. Mahoudeau. These researches prove the existence of microscopic functional centres in certain parts of the brain, which may be regarded as the true physiological elements of the cerebral organ.—Replies made by M. Bink, while stationed in New Guinea, to the Society's printed category of questions regarding sociology and ethnography.—Report of M. Nicolas on the graves found in the gravel beds of Gadagno (Vaucluse), which he assumed to be of the Stone Age, but which are now shown to have a Ligurian character.—On the dolmens of Kergo, Carnac, by M. Gaillard, who, in another communication, treats of a curious circumstance in regard to the system of arrangement followed in the lines of menhirs in Morbihan. Here he found that a single stone, having a rounded top, was interposed at irregular distances between the regular lines of menhirs, which it always exceeded in height. Continued observations showed that on different and special days of the year, the sun appeared, at its rising, to rest upon the summit of one or other of these isolated menhirs. —On graves containing cinerary vessels belonging to the Polished Stone Age, in the *commune* of Montigny-l'Engrain, by M. Vauvillé.—Observations on the restoration of the tumulus of Kerlescan, by M. Gaillard. The cromlech, which has here been readjusted, consists of twenty-nine of the ancient menhirs, which had escaped destruction.—On the form of the wrist in supination and pronation, and on the differences of outline exhibited by the metacarpal bones when observed on the skeleton, or when the cuticle has been removed, and the muscles have been laid bare, by M. Cuyer, who writes for the artist rather than the anatomist, and illustrates his remarks by various useful woodcuts.—On the relative length, among various peoples, ancient and modern, of the index and ring finger, by Colonel Duhousset.—On the relative length of the two first toes in the Mongolian races, by Dr. Maurel.—On the significance of the practice of measuring the throat of young women, common among the peasantry of Brittany, as well as various Kabyle tribes, by M. Letourneau. Among these remotely separated peoples, similar notions prevail as to the connection between the volume of the neck in women and the age of puberty. The practices that have arisen from a popular belief in regard to this relation are so nearly alike in both instances as to suggest some common origin.—The importance of studying the character of the masticatory process and apparatus, in judging of the nature and habits of mammals, when considered from an anthropological point of view, by Dr. Fauvelle.—On the survival of communal proprietorship in the Morbihan, by M. Letourneau. In this district, various ancient customs still prevail, and in the Islands of Hœdic and Houat, the *curé*, assisted by a council of notables, governs the islands after a patriarchial or fatherly fashion.—A communication, by M. Variot, concerning a new method of effacing the traces of tattooing.

Rivista Scientifico-Industriale, December 31, 1888.—The Chladni figures and Wheatstone's methods, by Prof. Lodovico Malavasi. Here the author proposes to apply Wheatstone's theory on the acoustic vibrations of square plates to the explanation of the figures observed by Chladni in rectangular plates.—The same number contains some remarks by G. Cariati on Mr. Edison's perfected phonograph; and on the solar photographs taken by Prof. Riccò at the Observatory of Palermo.

SOCIETIES AND ACADEMIES.

London.

Linnean Society, February 7.—Mr. C. B. Clarke, F.R.S., Vice-President, in the chair.—The Rev. E. S. Marshall exhibited several interesting varieties of British plants collected by him in Scotland, and made remarks thereon.—Mr. E. M. Holmes exhibited a new British seaweed from Bognor, *Rhododermis elegans*, var. *polystromatica*, a variety new to science.—A paper was then read by Mr. A. D. Michael on three new species of parasitic *Acari* discovered by him in Derbyshire during the autumn of 1888. These were a *Myocoptes*, proposed to be called *M. tenax*, parasitic on the field vole, *Arvicola agrestis*; a *Symbiotes*, proposed to be called *S. tripilis*, parasitic upon the hedgehog; and *Goniomerus masculinus* (gen. et sp. nov.), a minute parasite found on the ear of the field vole. Specimens of all three were exhibited under the microscope, and a discussion followed, in which Profs. Mivart, Stewart, and Howes took part.—Prof. Martin Duncan then gave the substance of an important paper which he had prepared, entitled "A Revision of the Families and Genera of the *Echinoidea* Recent and Fossil." Reviewing the labours of his predecessors, Prof. Duncan traced the growth of the literature of his subject, and showed that, although many lists and papers had been published from time to time, no general review of the class *Echinoidea* had been attempted since 1846. Dealing with all the material at his command, he found it necessary to propose certain alterations in the classification, and to dispense with a good many genera and sub-genera which he considered had been needlessly founded. Above all, he had set himself the task of revising the descriptions of the genera, giving positive instead of comparative characters, a course which he believed would prove of great utility to students. The paper was criticized by Mr. Sladen, Prof. Stewart, and Mr. Breeze, all of whom testified to the necessity which had arisen for some authoritative revision of the subject such as had been undertaken by Prof. Duncan, whose researches would undoubtedly lighten very considerably the labours of future inquirers.

Physical Society, February 9.—Annual General Meeting. —Prof. Reinold, President, in the chair.—The Reports of the Council and Treasurer were read and adopted. From the former it appears that the number of members has only slightly increased during the year, due, it is supposed, to the advantages offered by the Society not being generally known, and a fly-leaf has been prepared, giving a short account of the Society's objects and procedure, copies of which may be obtained from the Secretaries. During the past year vol. i. Part 1, of "Physical Memoirs selected and translated from Foreign Sources," has been printed and issued to members; and the translations of important memoirs by Fourier, Hittorf, and Volta are well advanced, and will be published shortly. The Treasurer's Report showed the financial position of the Society to be very satisfactory. Owing to the lamented death of Prof. Clausius, a vacancy has occurred in the list of honorary members, to fill which Prof. R. W. Bunsen was nominated by the Council and duly elected. The following gentlemen were elected to form the new Council:—President: Prof. A. W. Reinold, F.R.S.; Vice-Presidents: Dr. E. Atkinson, Prof. W. E. Ayrton, F.R.S., Shelford Bidwell, F.R.S., the Right Hon. Lord Rayleigh, Sec. R.S., ; Secretaries: Walter Baily, Prof. J. Perry, F.R.S.; Treasurer: Prof. A. W. Rücker, F.R.S.; Demonstrator: C. Vernon Boys, F.R.S.; other Members of Council: Hon. R. Abercromby, T. H. Blakesley, W. H. Coffin, Conrad W. Cooke, Prof. O. Lodge, F.R.S., Prof. W. Ramsay, F.R.S., W. N. Shaw, Prof. S. P. Thompson, H. Tomlinson, and Dr. G. M. Whipple. Cordial votes of thanks were passed (1) to the Lords of the Committee of Council on Education for the

use of the rooms and apparatus of the Normal School of Science; (2) to the Council and Officers of the Society for their services during the past year; and (3) to the Auditors, Mr. Inwards and Prof. Minchin, for examining the accounts. In returning thanks for the Council and Officers, the President attributed the great success of their meetings to the indefatigable zeal displayed by the Hon. Secretaries.—The meeting was then resolved into an ordinary science meeting.—Prof. A. S. Herschel, F.R.S., read a paper on physico-geometrical models, and exhibited a collection of geometrical figure models illustrating elementary forms of crystallographical and chemical form constructions. The subject, he said, had suggested itself to him as an important one for study some five years ago, from the possibility which he then discovered of effecting a four-limbed mechanical cycle, similar to a thermodynamic cycle, with a conically revolving pendulum, the mathematical conditions of whose motion involved the logarithum of the circle radius of the knob's revolution, as a quantity equivalent to entropy in the thermal cycle. The mechanical cycle may be effected as follows. Suppose the pendulum knob to be revolving at velocity V_1, and radius r_1; imagine the controlling force to decrease so that the radius varies from r_1 to r_2, the velocity remaining constant at V_1; then increase the controlling force to its original value, and simultaneously impart velocity to the knob to keep the radius constant at r_2, the final velocity being V_2. Next, further increase the controlling force so as to cause the radius to decrease to r_1 at velocity V_2; and finally, decrease the controlling force to its initial value, simultaneously retarding the knob till the velocity is V_1 at radius r_1. The net energy concerned in the operations was given as $W = (V_1^2 - V_2^2)\phi$; where $\phi = \log \frac{r_1}{r_2}$, and the cycle affords an illustration of transformation of gyratory motion into energy of reverberatory push along the axis. The logarithm ϕ is also measured by the area of a hyperbolic sector, and is connected (as was shown) with a sector of the circle which a lamp's conical beam would inscribe upon a square glass screen or plate dividing a long room of square cross-section into two parts. The conical beam prolonged through the glass then traces, on the walls beyond, hyperbolas, whose sectors from their summits are related to the circle's corresponding sectors by a well-known hyperbolocyclic trigonometrical connection. From this mode of constructing the connection, and from a discordance which it shows at the asymptote extremities with Euclid's definition of parallel straight lines, the author concludes that cubic space cannot be continuous in its structure, but must be, in a physically constructive sense, like material particles indestructively atomic or molecular. The models showed modes of constructing cubic space in various ways, chiefly by means of tetrahedra, octahedra, and dodecahedra, the common element of form in these being also shown to be the right-angled tetrahedron or "biquoin" obtained by dividing a cube into six equal parts by three planes through its diagonal. A model of Sir W. Thomson's soap foam figure, and some wooden models representing Haüy's polyhedral atoms and their combinations, were exhibited to illustrate the structural view thus taken of geometry; and chemical-figure models of the ring of tetrahedral carbon molecules in benzene, and of the asymmetric groups of similar atoms in active and inactive tartaric acid, were shown.

Zoological Society, February 5.—Dr. St. George Mivart, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of January 1889.—Mr. Sclater exhibited a living specimen of the Thick-billed Lark (*Rhamphocoris clotbeyi*) lately received by the Society from Southern Algeria, and called attention to its structural peculiarities.—Mr. G. A. Boulenger read a paper on the species of Batrachians of the genus *Rhacophorus*, hitherto confounded under the name of *R. maculatus*, and pointed out their distinctions.—Mr. Sclater pointed out the characters of some new species of birds of the family Dendrocolaptidæ, which were proposed to be called *Upucerthia bridgesi*, *Phacellodomus rufipennis*, *Thripophaga fusciceps*, *Philydor cervicalis*, and *Picolaptes parvirostris*.—A communication was read from the Rev. O. P. Cambridge on some new species and a new genus of Araneidea. Two of these species (*Pachylomenus natalensis* and *Stegodyphus gregarius*) were based on specimens living in the Insect-house in the Society's Gardens.—A communication was read from Prof. F. Jeffrey Bell, containing descriptions of new or rare Holothurians of the genera *Plexaura* and *Plexaurella*.—Dr. Günther, F.R.S., exhibited and made remarks on some fishes which had been dredged up by Mr. John Murray off the west coast of Scotland, and were not previously known to occur in British waters, viz. *Cottus lilljeborgii* (Collett), *Triglops murrayi*, sp. n., *Gadus esmarckii* (Nills.) *Onus reinhardti* (Collett), *Fierasfer acus* (Brünn.), *Scopelus scoticus*, sp. n., and *Stomias ferox* (Rnhrdt.).—Dr. Günther also exhibited and described a specimen of *Lichia vadigo* (Risso), a species of which only a few specimens were previously known from the Mediterranean and Madeira. This specimen was obtained by Captain MacDonald on September 17, 1888, off Waternish Point, Isle of Skye. He also exhibited a hybrid between the Roach (*Leuciscus rutilus*) and the Bleak (*Alburnus alburnus*), sent to him by Lord Lilford from the River Nun, Northamptonshire.—Mr. Beddard read a paper descriptive of the coloured epidermic cell of *Æolosoma tenebrarum*.—Mr. Boulenger exhibited and made marks on a series of living specimens of Tortoises of the genus *Homopus* from Cape Colony, lately received by the Society from the Rev. G. H. R. Fisk, C.M.Z.S.

Geological Society, February 6.—W. T. Blanford, F.R.S., President, in the chair.—The following communication was read:—On the occurrence of Palæolithic flint implements in the neighbourhood of Ightham, Kent, their distribution and probable age, by Joseph Prestwich, F.R.S. The author stated that Mr. Harrison, of Ightham, has discovered over 400 Palæolithic implements lying on the surface at various heights and over a wide area around Ightham. A description of the physiography of the district and of the distribution of the various gravels and drifts was given, and in the absence of fossils, attention was called to the different levels at which the deposits occurred, and to their physical features and characters. Besides the river-gravels, two groups of unclassed gravels were described, one occupying a low level, and the other levels higher than that to which the river-drifts reach; the latter is of varied composition. In the case of the Shode valley, only beds below the contour-level of 350 feet in its upper part, and of 300 feet or less in its lower part, can be referred to the former action of the Shode, and those above this belong to a high-level drift of uncertain age. The composition of the various gravels was described in detail. The implements are found on the surface of the land at all levels up to 600 feet, and Mr. Harrison has discovered them at forty localities in the hydrographical basins of the Shode, the Darent, the Leybourne stream, and in part of the Thames basin. Two groups of implements extend far beyond the limits assigned to the river-drifts formed since the present hydrographical basins were established, and must be accounted for by some other means than those in connection with the former *régime* of the existing streams. A description of the general characters and variations observable in the implements was given. It is evident from the condition of most of the implements, that they have been embedded in some matrix which has produced an external change of structure and colour. In the case of the river-gravel sites, the question presents no difficulty. Three cases of implements have been found: (i.) where the flint still shows some of its original colour; (ii.) those of which the surface has turned from black to white, has been altered in structure, and acquired a bright patina, and which show no trace of wear; (iii.) those of which the flint has also lost its original colour, but has been stained, and is with or without patina,—these are generally much rolled. The characters of the first call for no comment. Those of (ii.) and (iii.) are very marked, and there is no difficulty in referring each to a distinct matrix. The implements of class ii. have been embedded in a stiff brick-earth, generally of a reddish colour, and those of class iii. seem to have lain in ferruginous beds of sand or gravel. Reasons were given for supposing the surface to have been once covered with a deposit of clay or loess, since denuded except where preserved in pipes, and that a continuous plane descended from the high range of the Lower Greensand to the Thames Valley, which has since been lowered 300 feet or more. It was also shown that the high-level deposits were formed anteriorly to the post-glacial drifts of the Medway and Thames Valleys. It is probable that the loess is a deposit from flood-waters, and that some of it may be referred to the Medway flowing at a higher level; but the highest deposits cannot be so accounted for, and the author referred to the possibility of glacial action, without insisting on it. The deposit on the chalk-plateau is abruptly cut off by the river-valleys, and the rudest forms of implements, such as those of Ash and Bower Lane, occur on this plateau at from 500 to 550 feet, and the author thinks they may possibly be of pre-glacial age. The changes which have taken place in the

physiography of the district, and the great height of the ol chalk-plateau, with its clay-with-flints and southern drifts, poin to long intervals of time, and to the great antiquity of the rude implements found in association with these drifts. That the removal of the material indicates the existence of agents of greater force than those operating under the present river *régime* closes up the time required for the completion of the great physical phenomena, though the author's inquiry tends to carry man further back geologically than is usually admitted. After the reading of the paper there was a discussion, in which Dr. Evans, Mr. Topley, and others took part.

Entomological Society, February 6.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—The President announced that he had nominated Captain H. J. Elwes, Mr. F. Du Cane-Godman, F.R.S., and Dr. Sharp, Vice-Presidents for the session 1889-90.—Lord Walsingham exhibited a larva of *Lophostethus dumolini*, Guer., sent to him by Mr. Gilbert Carter, from Bathurst, West Coast of Africa.—Mr. G. T. Porritt exhibited several melanic specimens of *Boarmia repandata* from Huddersfield, and, for comparison, two specimens from the Hebrides. Mr. McLachlan, F.R.S., remarked that melanism appeared to be more prevalent in Yorkshire and the North Midlands than in the more northern latitudes of the United Kingdom.—Captain Elwes read a paper entitled "On the Genus *Erebia*, and its Geographical Distribution." The author, after referring to the number of species and named varieties, many of which appeared to be inconstant as local forms, made some remarks on the nomenclature of the genus, and suggested that a better system of classification might be arrived at by anatomical investigation. It was stated that little was known of the early stages and life-history of species of this genus, the geographical distribution of which was Alpine rather than Arctic. The author remarked that it was curious that there was no species peculiar to the Caucasus, and that no species occurred in the Himalayas, where the genus is replaced by *Callerebia*; that none were found in the Himalo-Chinese sub-region, and none in the Eastern United States of America. He also called attention to the similarity of the species in Colorado and North-West America to the European species. Lord Walsingham, Mr. Waterhouse, Mr. O. Janson, Mr. McLachlan, Dr. Sharp, and Mr. Jenner-Weir took part in the discussion which ensued.—Mr. W. Warren read a paper entitled "On the *Pyralidina* collected in 1874 and 1875 by Mr. J. W. H. Traill in the basin of the Amazons."—Mr. C. J. Gahan read a paper entitled "Descriptions of New or little-known Species of *Glenea* in the Collection of the British Museum."—Dr. J. S. Baly communicated a paper entitled "Notes on *Aulocophora* and Allied Genera."

Paris.

Academy of Sciences, February 11.—M. Des Cloizeaux, President, in the chair.—On the loss of gaseous nitrogen during the decay of organic substances, by M. Th. Schlœsing. In continuation of his previous communication of February 4, he gives the results of some of the experiments already described.—Fresh researches showing that the toxic property of exhaled air does not depend on the carbonic acid, by MM. Brown-Séquard and d'Arsonval. In three previous communications (November 28, 1887, and January 9 and 16, 1888) the authors showed the nature of the relations existing between pulmonary tuberculosis and the air exhaled from the lungs of human beings and domestic animals, as well as the toxic property of one or more substances derived from the lungs. Fresh researches here described show that the poison or poisons escaping with the exhaled air may kill even in small doses, and even without being injected directly into the arterial or venous blood. Subcutaneous injection killed seventeen of eighteen rabbits operated on, generally within twelve to twenty-four hours. In a large number of cases they found that pure carbonic acid (not charged with the vapours of hydrochloric acid) may be inhaled in considerable quantity in the atmospheric air by human beings, dogs, rabbits, and other mammals. The authors themselves breathed for over one or two hours an atmosphere containing 20 per cent of CO_2 without any marked inconvenience, and especially without any lasting consequence. Other still more crucial experiments satisfied them that the fatal results are due, not to the carbonic acid, but to some other toxic substances exhaled by patients suffering from pulmonary affections.—On the invasions of locusts in Algeria, by M. J. Künckel d'Herculais. A careful study of the available documents has convinced the author that the most disastrous years (1845, 1866, 1874) are those that coincide with the simultaneous appearance of the indigenous species (*Stauronotus maroccanus*) and of the foreign variety (*Acridium peregrinum*) arriving from Central Africa.—Observations of the new planet discovered on February 8 at the Observatory of Nice, by M. Charlois. This planet, which is of 12·5 magnitude, is the second discovered by the young astronomer within a fortnight.—On the reductions of the problem of n bodies preserving certain mutual distances, by M. Andrade. It is here shown that the already communicated solution of this problem for a particular case is the most general possible.—On the phenomena of electrolysis, by MM. Violle and Chassagny. The decomposition of water by means of an energetic current is accompanied by luminous and calorific phenomena, which were described by Fizeau and Foucault over forty years ago, and afterwards studied by many physicists. By employing a Gramme machine of 40 amperes with an electromotor force of 110 volts, the authors have been enabled easily to produce these phenomena, to observe them under clearly defined conditions, and to record some hitherto unnoticed circumstances, which are here described.—On the actinometric observations made at Kief, by M. R. Savelief. These observations had special reference to the phenomenon of solar radiation during the year 1888, and to the determination of the solar constant at Kief. M. Savelief's paper was followed by some remarks by M. A. Crova, who directed attention to the great interest it presented, as showing that the law of the annual variations of solar radiation is practically the same at Kief as at Montpellier; that the calorific transparency is greater at the former than at the latter station, and that the solar constant determined on a clear winter's day in Russia may attain the value of 3 calories. This is higher than any recorded at Montpellier, and approached only by the records of an actinometer placed on the summit of Mount Ventoux, which are about the same as those obtained by Mr. Langley during his remarkable researches on the top of Pike's Peak, Colorado.—On some reactions of the chlorides of mercurammonium, by M. G. André. After concluding his researches relative to the decomposing action of water on the amidochloride (*Comptes rendus*, cviii. p. 233), M. André determines the conditions under which the chloride of dimercuriammonium is transformed to amidochloride at contact with sal-ammoniac.—On amorphous bismuth, by M. F. Hérard. The author has succeeded in obtaining this substance by applying to bismuth the same process that he had already employed to obtain amorphous antimony, as described in the *Comptes rendus* for August 13, 1888.—Syntheses effected by means of cyanosuccinic ether, by M. L. Barthe. In a previous note (*Comptes rendus*, cvi. p. 143), MM. Barthe and Haller described the preparation of cyanosuccinic ether, obtained by making monochloracetic ether react on sodium cyanacetic ether. By means of that ether they have now obtained the following syntheses: (1) methyl cyanosuccinic ether; (2) ethylcyanosuccinic ether; (3) ethylethenyltricarbonic ether; (4) propylcyanosuccinic ether, of which the respective formulas are given.—Discovery of a new Quaternary station in Dordogne, by M. Émile Rivière. This station, to which M. Rivière has given the name of Pageyral, in honour of M. Mercier-Pageyral, lies about 2 kilometres from the celebrated Cro-Magnon Cave on the left bank of the Vézère nearly opposite Laugerie, and facing the islet of Malaga. A first exploration has yielded the remains of the reindeer, *Cervus elaphus*, *Cervus capreolus*, *Sus scrofa*, *Canis aureus*, besides various objects of human workmanship characteristic of the Madeleine epoch, such as flint knives, scrapers, arrow-heads, cores, and numerous chippings.

Berlin.

Physiological Society, January 18.—Prof. du Bois-Reymond, President, in the chair.—Dr. Baginski gave an account of his further researches on the Bacteria which occur in the fæces of children fed on milk. Of the two Bacteria which are thus found—namely, *Bacterium lactis* and *Bacterium coli*—he had previously experimented with the first, and proved that it does not induce a lactic acid, but an acetic acid fermentation of milk-sugar, and should hence more appropriately receive the name of *Bacterium aceti*. Recently he has investigated the *Bacterium coli*. Sown in a solution of milk-sugar it produces no change, but when some white of egg is mixed with it a fermentation is set up, not only when the access of air is permitted, but also when it is prevented. The products of this fermentation were proved to be, by an elaborate series of chemical investigations, lactic, acetic, and formic acids, the occurrence of the latter being proved by crystallographic measurements of its barium salts. Both the Bacteria exert an influence which is antagonistic to the development of any alkaline fermentations or decompositions.—Dr. A. König gave an account of his experiments on the action of

santonate of soda on the perception of colours. As is well known, a distinction is drawn between congenital and acquired anomalies of the colour-sense (colour-blindness); of these the first only gives rise to colour-blindness to red or green, while colour-blindness to violet is never observed as a congenital defect. On the other hand, it was supposed that, in the anomalous perception of colour which results from the action of santonin or santonate of soda, we had to deal with a typical case of acquired colour-blindness to violet. The speaker had hence been led to make a number of experiments with santonate of soda on himself, and, apart from the fact that as soon as its action is manifest all objects appear of a yellow colour, had established the following phenomena. The spectrum ceases to be visible on the hinder side of the blue, and not a trace of violet is ever visible; the neutral point, as deduced from closely-agreeing measurements, is situated at wave-length 573—that is to say, exactly at that point which is complementary to the missing violet. The speaker based upon these observations the conclusion that the visual phenomena which are observed after the administration of santonin are not really of the nature of colour-blindness to violet, but can be completely explained by the assumption that the violet rays are absorbed by those media of the eye which have been affected by the drug. Prof. Preyer was unable to agree with the above conclusion, speaking with the experience of the experiments he had himself made with santonate of soda in 1868. The fact that after the administration of the drug the violet part of the spectrum can be seen when it is looked at not directly but indirectly, is opposed to Dr. König's views—that is to say, when its image is allowed to fall upon peripheral parts of the retina. Moreover, Prof. Preyer stated that he experienced a distinct sensation of violet when he had taken the drug while his eyes were closed, and then opened them after the action of the drug had become manifest. He believes that the visual phenomena which accompany the action of santonin can only be explained by assuming that it affects the central nervous system, and that this view is supported by the abnormal gustatory, olfactory, and auditory sensations which are simultaneously observed.—The President communicated some instances of the occurrence of real gustatory and olfactory dreams.

Physical Society, January 25.—Prof. von Helmholtz, President, in the chair.—Dr. A. König spoke on the dependence of visual acuteness upon the intensity of light when objects are illuminated by spectral colours, his remarks being based upon experiments made by Dr. Uhthoff. Earlier researches have shown that, for red, yellow, and green light, the visual acuteness increases at first very rapidly, then more slowly, and then finally shows scarcely any further change as the intensity of the illumination is increased; and that the curve of visual acuteness on the abscissæ which represent the varying intensity of illumination is a parabola, whereas with blue light the curve is a straight line. Dr. Uhthoff had repeated these experiments with spectral colours, taking care that the several lights used were in all cases of equal intensity, a result obtained by altering the width of the slit. The speaker described fully the apparatus he had used, and the series of preliminary experiments he had made, by which he had proved that the narrower the slit is in the screen upon which the spectrum falls, the greater is the acuteness of vision, and that the observations are more trustworthy when a dark mark on a light ground is used as the object whose brightness is to be determined than when a light mark on a dark ground is employed. As regards the apparatus it may be mentioned that the dispersion is produced by a fluid-prism 1 decimetre in diameter. The result of these experiments, as of former ones, was that the visual acuteness increases with the intensity of light in the blue part of the spectrum. When the visual acuteness is compared in the different spectral colours, the intensity of light being in all cases the same, a curve is obtained with a maximum lying near its centre. When the intensity of the light is less, the curve of acuteness on the abscissæ of the spectral colours becomes more pointed, and the maximum moves simultaneously towards the red end. When the intensity of light is the least possible, the maximum for the visual acuteness coincides with the point of greatest brightness in the spectrum. The above holds good not only for the normal trichromatic eye, but also for the dichromatic or red- and green-colour-blind eye.—Prof. Kundt exhibited a photograph of the spectrum of cyanogen extending from the line H up to about the line L, which had been sent to him by Prof. Keyser, of Hanover. For size, beauty, and clearness of the several groups of lines, this photograph is scarcely likely to find its equal.

AMSTERDAM.

Royal Academy of Sciences, January 26.—M. Buys Ballot communicated the results of his observations during the last forty years at the Meteorological Institute at Utrecht, and stated how much temperature, air-pressure, and rain deviated to the right or left from the mean values, and how long this occasionally continues on a stretch before compensation comes about.—M. Beÿerinck spoke on a method of determining the action of different substances on the growth and on some other vital functions of micro-organisms, and illustrated his assertions by preparations. The method consists in applying small quantities of various substances on gelatine plates, either pure or prepared for the purpose, and infected with yeast or Bacteria of some kind or other, and then watching if the micro-organisms in the centres of diffusion of those substances—whether remaining pure or meeting each other on their way—multiply or not, or if they do so in a greater or less degree.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Electrical Trades Directory and Hand-book, 1889 (*Electrician* Office).—The Elementary Principles of Electric Lighting, 2nd edition: A. A. C. Swinton (Lockwood).—A Dictionary of Photography: E. J. Wall (Hazell).—A Manual of Cursive Shorthand: H. L. Callendar (Clay).—The Chemical Analysis of Iron: A. A. Blair (Whittaker).—Hourly Readings, 1886, Part 1, January to March (Eyre and Spottiswoode).—Greek Geometry from Thales to Euclid: G. J. Allman (Longmans).—*Challenger* Report, Zoology, vol. xxix. Text, 2 Parts (Eyre and Spottiswoode).—The First Ascent of the Kasaï: C. S. L. Bateman (Philip).—Therapeutics ought to become a Science: Dr. W. Sharp (Bell).—Therapeutics can become a Science: Dr. W. Sharp (Bell).—The Great Lake Basins of the St. Lawrence: A. T. Drummond.—La Pénétration de la Lumière dans les Lacs d'Eau Douce: Dr. Forel (Leipzig, Engelmann).—Die Zusammendrückbarkeit des Wasserstoffes: S. von Wroblewski (Wien, Tempsky).—Eskimo of Hudson's Strait: F. F. Payne (Toronto).—The Navajo Tanner: R. W. Shufeldt.—Journal of the Anthropological Institute, February (Trübner).—Natural History Transactions of Northumberland, Durham, and Newcastle-upon-Tyne, vol. x. Part 1 (Williams and Norgate).—Journal of the Royal Microscopical Society, December 1888 to February 1889 (Williams and Norgate).—Rendiconto dell' Accademia delle Scienze Fisiche e Mathematiche (Sezione della Società Reale di Napoli) Serie 2a, vol. ii. Fasc. 1° to 12° (Napoli).

CONTENTS.

THURSDAY, FEBRUARY 28, 1889.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873–76, *under the command of Captain George S. Nares, R.N., F.R.S., and the late Captain Frank T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., &c., Director of the Civilian Staff on board, and now of John Murray, LL.D., Ph.D., &c., one of the Naturalists of the Expedition. Zoology—Vol. XXVIII. Published by Order of Her Majesty's Government. (London: Printed for Her Majesty's Stationery Office, and sold by Eyre and Spottiswoode, 1888.)

THIS volume contains but one Report, that on the Siphonophoræ collected during the voyage of the *Challenger*, by Ernst Haeckel, M.D., Ph.D., &c. The manuscript was received in instalments between February 8 and July 5, 1888.

In the editorial note we read:—" Prof. Haeckel, through his long-continued and elaborate investigations of living Siphonophoræ and Medusæ in the Mediterranean, Indian and Atlantic Oceans, was in a very special manner fitted to undertake such a task as this Report; and it must be regarded as fortunate that he should have been willing to undertake the work on condition that some of his own unpublished observations should be incorporated. This important and masterly Report has thus become a monograph of the whole class more complete than hitherto published; the classification has been reformed and placed on a new basis. The introduction, giving a general account of the morphology of the order, was translated from the German manuscript by Mr. J. Arthur Thomson, M.A. All the remaining part of the Report was written by Prof. Haeckel in the English language."

It may be fearlessly asserted that no living biologist is more capable of writing a Report on the *Challenger* Siphonophoræ than Prof. Haeckel, and but little objection could be taken to the request that his previous original work should be incorporated with that more particularly devoted to the results of the *Challenger's* expedition; nay, personally, we are inclined to think that a Report should benefit immensely by the incorporation of such work, the more especially in the case of a class like that of the Siphonophoræ, where the drawings from the life are of paramount importance.

It is well known to all interested in the beautiful and graceful forms which constitute the class of the Siphonophoræ, that Prof. Haeckel's " System der Siphonophoren, auf phylogenetischer Grundlage entworfen" appeared in the *Jenaische Zeitschrift* for May 1888; and in the present Report (p. 356) he himself tells us that a separate edition of this important paper was published in December 1887. The general introduction to the Report, which the editor informs us was translated from the German manuscript furnished between February and July 1888, is declared by Prof. Haeckel to be translated from the general part of the above-mentioned separate edition; and, as a matter of fact, a reference to the *Jenaische Zeitschrift* shows this to be the case, with an addition of three pages on " the fundamental form (promorph)." This is but a small matter, but the statement in the preface is certainly one apt to mislead the reader, who ought to have been informed of the previous publication of the general introduction, without being left to stumble upon the statement of the fact on almost the last page of the volume.

Since the publication of the memoir on the Siphonophoræ in the *Jenaische Zeitschrift*, the classification of the group has been but little " reformed," and has certainly not been placed "on a new basis." In the " System" there are seventy-five genera enumerated, which are included in twenty-two families, divided into five orders. In the Report the orders remain the same, two of the subfamilies are elevated to the rank of families, and some half-dozen of the genera are subdivided. Their numerical arrangement is unaltered, but letters are added to the running numbers of the " System"; thus Genus 75 in both the " System" and the Report is Physalia, Lamk., while in the Report the genus Caravella, Hkl., is added as 75*b*. There being no index of genera and species, this partly numerical and partly alphabetical systematic arrangement of the genera makes the volume difficult of reference.

That this Report, able as it is, cannot be regarded as a " monograph" of the order will be generally acknowledged when the following facts are considered. The first order, Disconectæ, contains three families; the first of these, with a circular and regular octoradial umbrella, and with mouthless blastostyles, is called Discalidæ; it contains two genera, Discalia and Disconalia. Of the former two species are named, both inhabitants of the deep sea, and both found by Haeckel in the *Challenger* collection; the first species is described and beautifully figured as *D. medusina*, the second species, *D. primordialis*, n. sp., was captured in the tropical Pacific, at Station 274, and at a depth of 2750 fathoms; but we read, p. 46, " as its state of preservation was not sufficient, I give only the description of the first well-preserved species." Of the genus Disconalia, " two species (both deep-sea inhabitants) were found by me in the *Challenger* collection, one from the Southern Pacific (Station 181), the other from the Indian Ocean, south of Australia (Station 157). The latter (*Disconalia pectyllis*) had much longer and less ramified tentacles, and a larger pneumatocyst, than the former (*Dis. gastroblasta*); but its state of preservation was not sufficient for a full description" (p. 48); nevertheless, we are promised that *Disconalia pectyllis* will be described afterwards in " my 'Morphologie der Siphonophoren'" (p. 357). These two instances occur within the margins of the first two dozen pages of the descriptive portion of the Report; and when we call to mind the really wonderful way in which, from a few fragments, Prof. Haeckel has, in this very volume, diagnosed and even figured some of the *Challenger* species, we regret all the more that he has not here given us, at the very least, those descriptions which he has reserved for elsewhere. Among the list of *Challenger* species apparently good enough for future description, but though named yet not described in this Report, we note the following: *Eudoxella didyma*, Hkl. (Station 343, p. 108),—this genus is quoted from the " System" as Eudoxella, Hkl.; but in the " System," we find it printed as Eudoxon, Hkl., which name would

have priority; *Anthemodes articulata*, Hkl., *Bathyphysa gigantea*, Hkl.

Numerous are the species named from living or preserved specimens, for the descriptions of which the readers of this "monograph" are referred to the forthcoming "Morphology," such as *Strobalia cupola*, Hkl. "This beautiful species was observed living by me in the Indian Ocean, and will be described in my 'Morphology of the Siphonophoræ'; a fragment of a similar species, *S. conifera*, was collected by the *Challenger* in the South Pacific (Station 288)" (p. 243); and again, "*Auralia profunda*, the single species of this genus which I have examined, was taken in the depths of the tropical Atlantic, and will be described in my 'Morphology,' &c." (p. 301).

We have probably quoted enough to show the very uneven treatment that has been meted out to the species of this class. Two hundred and forty of them are referred to by name, but only a small percentage of these are diagnosed; of those that are, some of them were found during the voyage of the *Challenger*, some of them were found by Prof. Haeckel during his own memorable and fruitful sojourns at Madeira and Ceylon, but for many of them we have nothing more than names. There is no doubt that to have made this Report a "monograph" a larger volume, perhaps another volume, would have been needed; but had it been kept within the limits of a Report on the *Challenger* species, it would not have occupied the space it at present does.

The volume contains 380 pages, and is illustrated by 50 plates, several of which are beautifully printed in colours. The plates were in some instances printed off before the text was printed; and of those coloured, all but one are after original coloured drawings of the author's. Amidst the number of strange, beautiful, and interesting forms described and figured, it is hard to make a selection; perhaps the greatest interest will be taken in the forms belonging to the new order of the Auronectæ. In this order there is present a peculiar and most remarkable organ, called the air-bell or aurophore: "it seems to be the modified umbrella of a medusome, and is adapted for the production and emission of the 'gas' contained in the large pneumatophore; the trunk of the siphosome is also thickened and bulbous, and traversed by a network of anastomosing canals, similar to the fleshy or cartilaginous cœnosome of the Alcyonidæ." There are two families, Stephalidæ and Rhodalidæ. The family Stephalidæ has two genera, Stephalia and Stephonalia. Of the former genus, the single species *S. corona* was found by Dr. Murray during the *Triton* Expedition, in the Faroe Channel; and of the second genus the single species *S. bathyphysa* was found by the *Challenger* in the South Pacific. The family Rhodalidæ also contains two genera, Auralia and Rhodalia. Of the first of these, *A. profunda*, as before mentioned, "the single species of this genus which I have examined was taken in the depths of the tropical Atlantic, and will be described afterwards in my 'Morphology of the Siphonophoræ.' Its external appearance is similar to that of *Stephalia corona*; but the nectophores of the simple corona are more numerous and the tentacles are of the same shape as in Rhodalia." As this is all we are told about the species, we cannot be certain whether it was a *Challenger* form or not. Rhodalia likewise has but one species, *R. miranda*. This wonderful form is described from specimens taken at Station 320, south-east of Buenos Ayres, and is figured on Plates 1-5.

Prof. Haeckel thinks that these Auronectæ are permanent deep-sea Siphonophoræ, which may move up and down within certain limits of depth, but never come to the surface. He regards their discovery as one of the most splendid made during the cruise of the *Challenger*; yet for a knowledge of what is known about them we must look beyond the pages of this Report.

A species of Anthophysa, taken at Station 334, has been called *A. darwinii*. Several other species are alluded to by name; it seems possible that Merten's manuscript name Anthophysa, published by Brandt, in 1835, is preoccupied by Bory de Saint Vincent for a genus of flagellate Infusoria.

In addition to the author's preface, and the general introduction, there is the usual description of the families, genera, and species, the deficiencies in which we have sufficiently alluded to. This is followed by a bibliography of the Siphonophoræ; a list of the families, genera, and species; a very useful glossary of the terms used in Latin, English, and German; and lastly, a statistical synopsis, from which we learn that 85 genera are enumerated, these containing 240 species. The diagnoses of 47 new species are given in this Report, of which 27 were found by the *Challenger*.

The characteristics of the orders and families are given in most satisfactory detail, and this portion of the Report would in itself constitute a most important and valuable general history of the group, and must serve for the ground-work of all future writings on the subject.

If too much has been claimed for this meritorious contribution to natural science from a monographical point of view, such a claim in no way takes from its merits as a profoundly important contribution to natural science.

THE ENCYCLOPÆDIC DICTIONARY.

The Encyclopædic Dictionary. Vol. VII., Part II. (London: Cassell and Co., 1888.)

ALL concerned in the production of this work may be congratulated on the completion of their undertaking. It is a work of more than ephemeral interest, and well deserves the favourable reception which has already been accorded to it. It was planned seventeen years ago, and the first divisional volume appeared in 1879. The intention at that time was that the Dictionary should consist of twelve divisional volumes, but, as the execution of the scheme went on, it became obvious that, unless the contents of the concluding volumes were to be presented in a very inadequate form, two additional divisional volumes would be necessary. It was arranged that these volumes should be supplied, and thus ample opportunity was provided for the maintenance, to the end, of a high standard of excellence.

The scale on which the work has been done may be seen from the fact that in round numbers it contains some 180,000 words or headings. This is a great advance on earlier dictionaries. The new edition of the Imperial Dictionary, which stands next, has 130,000 headings; the latest edition of Webster's Dictionary, with supplement, has 118,000; the early edition of Webster had

70,000; and Todd's edition of Johnson's Dictionary had 58,000. Even a general statement of the number of headings does not give an adequate impression of the amount of the contents of the present work, because, as pointed out in the preface, each word has been subdivided as far as possible into the various meanings which it has assumed at different times.

Judged simply as an ordinary dictionary, the work has high merits. No doubt, when Dr. Murray's vast undertaking is completed, both this and all other existing dictionaries will seem in many respects deficient; but for the present we do not know that anyone wishing to possess, for frequent reference, a sound, thoroughly trustworthy dictionary, could anywhere find his want more satisfactorily supplied than in these volumes. The question, what words can legitimately claim admission, had of course to be met at the outset, and it has been settled in a way that will commend itself to the judgment of all persons competent to form an opinion on the matter. All technical terms, so far as known, have been included; slang words, colloquialisms, and provincialisms have not been wholly omitted; specially coined words have also, in some instances, been recognized; and due respect has been shown to semi-naturalized words and to hybrid compounds. The department of etymology, we need hardly say, has received much careful attention; and great pains have been taken to provide quotations illustrative of every sense of each word, with references as full as could be given. The pronunciation of words is clearly indicated, and the adoption of various styles of type makes it easy for persons using the Dictionary to distinguish between various divisions and subdivisions of words.

As the title indicates, it is not merely as a dictionary that the work should be estimated. The aim has been to combine the advantages of a dictionary with those of an encyclopædia, and this idea has been realized with a remarkable degree of success. No one will expect to find here, under any heading, full information, such as one reasonably looks for in a great encyclopædia. Nothing of this kind has been attempted. But under each heading the thing is described as well as the word; and although the descriptions are necessarily brief, they are, so far as they go, clear and accurate, and no one who refers to them will afterwards have to unlearn anything they may have taught him. The short articles relating to the various branches of natural science are especially well done—a fact which will be readily understood when we say that the editor and publishers acknowledge the services of many eminent scientific workers, including Prof. Huxley, Dr. Sclater, Dr. Günther, and Mr. Carruthers.

OUR BOOK SHELF.

Manuel Pratique de Cristallographie. G. Wyrouboff. (Paris: Gauthier-Villars et Fils, 1889.)

MANY books have been written about crystallographic calculation, and their size and the uninviting appearance of their contents have probably created an impression that there is something peculiarly difficult in the subject. M. Wyrouboff's manual will not, we fear, dispel this erroneous idea, for about 300 of his pages are devoted to an exposition of the methods of calculation; but the book, being eminently practical, consists almost entirely of examples, which are fully worked out by simple and intelligible methods, while very little space is wasted upon the geometrical principles involved. The work is therefore very different in character from the geometrical treatises which have frequently adorned the subject, and will doubtless be of great use to the class for whom it is written—namely, "those to whom crystallography is only a means for the determination and description of species," to whom and to all who are attempting to acquire a practical knowledge of crystallography it may be warmly recommended. We would only suggest to such two words of caution: in the first place, the calculations are in each case conducted in two ways, by means of plane angles and solid geometry, and by means of spherical trigonometry,—the former method is unnecessary and undesirable; in the second place, the Millerian axes and notation of three indices should certainly be used in the rhombohedral system in preference to the notation of four axes.

M. Wyrouboff is so well known as the author of valuable contributions to the science of crystallography (by which we mean the knowledge of the relation between the physical, chemical, and geometrical characters of crystals as distinguished from either the study of mineralogy or the art of crystal-measurement), that the following remarks, taken from his preface, deserve particular attention:—

"How far more advanced would be our knowledge of the intimate structure of bodies if chemists were to describe accurately the form of the innumerable substances which issue from their laboratories, and which, being sometimes accidentally obtained, difficult to reproduce, or prone to rapid decomposition, are lost to science; and if, on the other hand, physicists were accustomed to connect the properties which they discover with the symmetry which belongs to the crystalline exterior. It is true that in doubtful cases, or such as are of particular interest, they both have willing recourse to the professional crystallographer; but, apart from the fact that he is not always at hand, experience teaches that such a division of labour rarely gives good results. Forms placed in arbitrary positions without regard to the forms of allied substances often serve only to conceal the analogies which are professedly sought," &c.

From which we gather that the position of crystallography in France is much the same as in England, and that such books as that of M. Wyrouboff, although not liable to be choked by the thorns of competition, are at present somewhat likely to fall upon the stony ground of indifference.

Assistant to the Board of Trade Examinations. By Captain D. Forbes, F.R.A.S. (London: Relfe Bros., 1888.)

IN preparing candidates for the Board of Trade examinations for officers' certificates, Captain Forbes has felt the want of a handy book of reference to the various questions asked, and he hopes to supply that want by the little book before us. In these examinations, the candidate has to show his knowledge by answering a number of a set of questions which is the same from year to year, although those selected by the examiner may be different. The questions put to the candidates, and the answers to them which the author suggests, are given in this little book. As examples of brevity, the definitions given of the various astronomical terms are unequalled in any book we are acquainted with. Thus, right ascension is defined as "the distance which a heavenly body is eastward of the first point of Aries"; while not the slightest idea is given of the meaning of the latter term. On p. 14, sidereal *time* is stated to begin when the first point of Aries is on the meridian, and to *end* when it returns to it

again; how much information this will convey to a student we leave our readers to judge. The answers to the various questions which are asked on the sextant are far better than the definitions, as are also the answers to the questions on cyclones. The questions on the deviation of the compass, for masters' certificates, are also fairly well answered, although a little further explanation of Napier's diagram, which forms the frontispiece, might have been given with advantage.

For reference to the questions given, the book will no doubt be very useful to intending candidates, but the answers given are good examples of the system of cramming for examinations, which cannot be too strongly condemned.

Guatemala: The Land of the Quetzal. By William T. Brigham. (London: Fisher Unwin.)

MR. BRIGHAM, an American author, has made three journeys in Guatemala, and in the present volume he has brought together all that seemed to be important in the notes written during his travels. The work is one of great interest, and ought to be not less welcome to the general reader than to persons who have special reasons for studying the subject. Mr. Brigham is a keen observer, and records his impressions clearly, simply, and effectively. No one who, in imagination, attends him in his course across the continent to Coban, from Coban to Quezaltenango, from Quezaltenango to the Pacific, will fail to be attracted by what he has to say about the physical features of the country and about the manners of its inhabitants. There are also excellent chapters on Guatemala city, and on Esquipulas and Quirigua. A sufficiently full account is given of the vegetable and animal productions of Guatemala, and of its earthquakes and volcanoes. In an introductory chapter, Mr. Brigham has something to tell us about Central America generally, and it may be worth noting that these regions will one day, in his opinion, be "the garden and orchard of the United States, not necessarily by political annexation, but by commercial intercourse." Great care has been taken to secure the accuracy of the illustrations, most of which are direct reproductions from negatives.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Weismann's Theory of Variation.

I THINK it may be reasonably objected to Mr. Cunningham's ingenious *reductio ad absurdum* of Weismann's theory, that because we cannot exactly state what happens in the mysterious fusion of sperm- and germ-nucleus, it by no means follows that such fusion does not largely account for the observed variation. I say largely, because Prof. Weismann's more recent paper, "Ueber die Zahl der Richtungskörper und über ihre Bedeutung für die Vererbung" (Jena, 1887), completes in a most important manner the paper quoted and criticized by Mr. Cunningham.

The argument is briefly as follows. The expulsion of the second polar body by sexual eggs is the removal of half the ancestral germ-plasms in order to make room for those which are added in fertilization. For this reason we must suppose that an equivalent reduction of the sperm-nucleus also takes place. If this reduction did not occur, the number of germ-plasms would be doubled in each sexually-produced generation—an unthinkable result.

If there were only one kind of germ-plasm in each nucleus, and sexual reproduction commenced, there would be 1024 distinct germ-plasms in each of them at the end of ten generations. But the number of generations must be so vastly in excess of ten that the number of ancestral germ-plasms in each sperm- and germ-nucleus must be considered to be infinite, or at any rate so large that the expelled halves would never be the same. It therefore follows that no two sperm- or germ-nuclei can be alike, and individual variation must follow. A little consideration also shows that while the children of the same parents must differ, they must also resemble each other more than the children of other parents, and while they must differ from their parents they must also resemble them more than the parents of other children.

The occurrence of atavism may be explained as a direct consequence of an unusual predominance of ancestral germ-plasms; the fact that atavism is a rare exception also follows from the fact that the expulsion of half the germ-plasms in each generation will nearly always prevent such predominance.

It is impossible to carry the subject further in the scope of a letter, or one might refer to the evidence for the absence of variability in parthenogenetic species, in which the second polar body is not expelled from the ovum; and to the identity of twins produced from a single ovum which has presumably divided after fertilization.

E. B. POULTON.

Oxford.

A Correction.

WITH reference to my communication in last week's NATURE, Prof. Oliver Lodge has called our attention to the fact that we had made a mistake in stating that the wave-length of the vibration was 33 centimetres. It is the semi-wave-length that is 33 centimetres; the wave-length is 66 centimetres, as is evident on consideration of the size of the "vibrator."

FRED. T. TROUTON.

Temperature Observations in Rivers.

THE Committee appointed at the Bath meeting of the British Association to investigate seasonal variations of temperature in rivers and streams was able to arrange through local scientific Societies for thirty observers, commencing work in January 1889. Of these there are ten in England, ten in Scotland, and ten in Ireland. Each observer is supplied with a specially designed thermometer (costing *2s. 6d.*) which has been compared with a standard instrument, and books for recording the observations and full instructions are provided by the Committee. It is desirable to extend the observations to rivers not yet taken up, and I therefore wish to direct the attention of local scientific Societies, of meteorological observers and others interested in similar work, to the opportunity now offered of taking part in a systematic investigation, the preliminary results of which show many interesting features. I shall be pleased to answer any inquiries of intending observers.

HUGH ROBERT MILL,
Secretary Brit. Assoc. Committee.

Heriot-Watt College, Edinburgh, February 20.

"Bishop's Ring."

IN your review of the Report of the Krakatão Committee of the Royal Society, it is stated that "Bishop's ring" has quite disappeared. Now this is hardly correct, for although I have not heard of anyone perceiving it in the middle of the day for a long time past, it is still visible about sunrise and sunset, though becoming on the whole gradually fainter. I presume that it is really the same phenomenon. At the time when "Bishop's ring" was most conspicuous in the full day-time, it was always far more so when the sun was rising and setting.

Sunderland, February 20. T. W. BACKHOUSE.

Peripatus in Australia.

Peripatus has been found not only in Queensland and Victoria, but also at Cassilis, in New South Wales, by Mr. A. S. Olliff, of Sydney. The Victorian and New South Wales localities are recorded in a postcript appended to my monograph of the genus as reprinted from vol. iv. of the "Studies from the Morphological Laboratory of the University of Cambridge." My knowledge of them is due to Mr. Olliff, who was kind enough to send me his specimen and his description of it (Proc. Linn. Soc. of

New South Wales, November 30, 1887). The New South Wales species is, I think, identical with that found in Queensland, and I should be inclined to doubt the distinctness of the Victorian species recorded by Mr. Dendy in NATURE (p. 366), and previously by Mr. Fletcher.

Mr. Dendy appears to lay some stress on the differences of colour as between his specimen and the specimens of *P. leuckarti* hitherto described, but it must be remembered that in some species of Peripatus—*e.g. capensis* and *novæ-zealandiæ*—the range of individual colour-variation is very considerable.

All the species that I have seen are very beautiful when alive; but the beauty, which is partly due to the texture of the skin, is very hard to reproduce in a drawing.

It is a remarkable fact that a creature which lives so entirely in the dark as does *Peripatus* should present such rich coloration and such complicated markings.

The egg of *Peripatus leuckarti* is heavily yolked and of a fair size, but smaller apparently than that of the New Zealand species. Its development cannot fail to be of the greatest interest, and it is sincerely to be hoped that the Australian zoologists will lose no time in working it out.

A. SEDGWICK.

Trinity College, Cambridge, February 18.

Anthelia.

I HAVE been following with much interest your notices of anthelia, and was about to add my mite to the information given, when, by the mail just in, I have your issue of October 25 last, wherein is a notice of the phenomenon as observed in Ceylon. I have witnessed it there scores and scores of times in my early tramps bird collecting, and I have also seen it at the Cape, in Brazil, on the Amazon, in Fiji, and in this island. On turning up my dear old friend Sir E. Tennant's book on Ceylon, I find that at p. 73, vol. i., he gives a very fair figure of the effect produced. It may be, as he says, that the Buddhists took from it the idea of a "halo" or "flame" for the head of Buddha, but there is one peculiarity about these flames that always struck me. In whatever position you find the Buddha, the flame is invariably in a straight line with the body even if the figure is recumbent. In form it always resembles the "tongues of fire" depicted by old painters as falling on the apostles on the Day of Pentecost.

I have seen many instances of what I suppose may be called "anthelia" in calm water, but the appearance is usually more *rayed*. I have an exquisite engraving in my print collection of the "Madonna and Dead Christ" by Aldegrever (1502–58). It has often occurred to me, in looking at it, that the artist has taken his idea of the halo round the Virgin's head from the appearance presented by the "anthelia" in water. There is the same luminous centre, and then the divergent rays. The halo round the head of the dead Christ in her lap is a four-cornered luminous star, issuing rays, of which three points only are visible—like nothing in nature with which I am acquainted.

E. L. LAYARD.

British Consulate, Noumea, January 3.

Mass and Inertia.

I AM pleased to see that Dr. Lodge has adopted my suggestion made in the *Engineer* about four years ago of using the term inertia for the quantity mass-acceleration. In making the suggestion I considered that I merely asked a return to the meaning implied by Newton in the phrase "*vis inertiæ*."

Unless this is the meaning of the term, the reason why Σmr^2 is called moment of inertia is almost incomprehensible. With it the connection is obvious; for, if ψ is the angular acceleration of a body about an axis, and r the distance of any particle, its linear acceleration is ψr, its inertia $m\psi r$, and its moment of inertia $rm\psi r$, or $m\psi r^2$. As the angular acceleration is the same for all particles of the body, the moment of inertia of the body is $\psi\Sigma mr^2$.

As Dr. Lodge mentions that he is bringing the matter before the British Association Committee on Units and Nomenclature, might I suggest that in future Σmr^2 should be called the *moment of inertia constant*, thereby implying the existence of the *variable factor* ψ, the angular acceleration, in the expression for moment of inertia.

E. LOUSLEY.

Royal College of Science, Dublin, February 16.

To find the Factors of any Proposed Number.

IT has long been a desideratum of mathematicians to discover a formula or method for ascertaining the factors of any proposed number, and also determining whether it be a prime or not. Their endeavours during the twenty centuries that have elapsed since Eratosthenes (B.C. 276–196) made the first recorded attempt to produce a practical rule for the purpose have not been attended with success.

As it may interest many readers of NATURE, and others, I propose, with a few preliminary remarks, to make known a simple arithmetical method by which this desideratum can now be attained.

Factors of an even number can readily be found, as 2 is always one of them, but it is not always so easy to find the factors of an odd number, especially if it be a high one, and, if the number be the product of two primes, the difficulty in this respect is still greater, because they are its only factors. Hitherto they could be ascertained only by trying in succession, as divisors, the prime numbers of less magnitude than its square root.

To find by such process the factors of 8616460799 (the square root of which is between 92824 and 92825), it might, possibly, be necessary to try 8967 prime numbers as divisors (out of the 8969 that there are) before they could be ascertained. By my process, division sums are altogether avoided. This high number occurs in a chapter on "Induction as an Inverse Operation," in "Principles of Science," by Stanley Jevons, second edition. His emphatic remarks as to the difficulties attending on inverse operations in general, and particularly those with reference to finding the factors of this number, were the incentive to my endeavouring to discover some process for ascertaining them which might possibly have escaped being previously tried. He states:—"The inverse process in mathematics is far more difficult than the direct process. . . . In an infinite majority of cases it surpasses the resources of mathematicians. . . . There are no infallible rules for its accomplishment. . . . It must be done by trial, . . . by guess-work. . . . This difficulty occurs in many scientific processes. . . . Can any reader say what two numbers multiplied together will produce 8616460799? I think it unlikely that anyone but myself will ever know. They are two prime numbers, and can only be discovered by trying in succession a long series of prime divisors, until the right one be fallen upon. The work would probably occupy a good computer many weeks. It occupied only a few minutes to multiply them together."

Mr. Jevons adds: "There is no direct process known for discovering whether any number be a prime or not, except by the process known as the 'sieve of Eratosthenes,' the results being registered in tables of prime numbers."

In the article on prime numbers in "Rees's Cyclopædia" (ed. 1819), the writer states: "It is in fact demonstrable that no such formula" (for discovering whether a number be a prime or not) "can be found, though some formulæ of this kind are remarkable for the number of primes included in them."

The difficulty of finding the factors of numbers is also referred to by the eminent writer (at that time President of the Mathematical Society)—under the initials C. W. M.—of an interesting review of "Glaisher's Factor Tables," in NATURE, vol. xxi. p. 462. In course of his remarks he mentions the number 3979769, and respecting it says: "It would require hundreds of division sums to ascertain by trial that it had 1979 for a divisor, and that consequently it was the product of 1979 × 2011;" and he adds, ". . . . there is no general mathematical principle which enables us to dispense with the trial, or even to shorten it, so as to bring it within practical limits."

These extracts afford conclusive evidence that no direct rule or method has hitherto been known, by which the factors of a number could be ascertained, and also that it is considered it would be a task of almost insuperable difficulty to devise one. Yet it seemed to me not unreasonable to think that, as two factors multiplied together formed a product, it ought to be possible to unmultiply or split up (as "C. W. M." expresses it) that product into its factors again, "without the enormous labour of trying for its divisors."

Strongly impressed with this idea, I attempted to realize it, and before long succeeded in discovering a simple arithmetical process for the purpose, and different from any previously tried. When applied to find the factors of 8616460799, instead of "many weeks being occupied" in the task, it showed, within a very reasonable time, that they were 96079 × 89681. When

applied to find the factors of 3979769, instead of "hundreds of division sums" being requisite,—about two minutes of time, no division sum at all, only one of subtraction, showed them to be 2011×1979.

My method or rule consists in finding the next higher square (call it "A") to the proposed number, from which, if the proposed number be deducted, the difference, or remainder, will be a square number (call it "B"). Then the square root of A plus the square root of B will be one factor, and the square root of A minus the square root of B will be the other.

It is essential to state, in addition to this general rule, that, when the "difference" between the proposed number and the higher square first used (not necessarily the next higher square to it) is a square, the process is virtually ended, for the factors can then be readily found by the directions given above. But if "the difference" be not a square, successive additions must be made to it of a progressive series of numbers, whose common difference is 2, and commencing with twice the square root of the square first used, plus 1, until their sum becomes a square number, which will be that called B in the paragraph above.

This is simply an easier mode of ascertaining what the differences may be between the proposed number and the higher squares, than by subtracting one from the other at each step of the process. The aggregate sum of the additions, taken at any step, is always equal to the square of the square root corresponding to it, minus the proposed number. The square root corresponding to any step is always half the serial number then added, plus $\frac{1}{2}$.

Hence my method may also be said to consist in the successive addition to the difference between any proposed number and a higher square, of a series of numbers as specified above, until their sum becomes a square.

The length of the process varies, and is longest when the difference between the factors of the proposed number is greatest, and especially if it be a high prime. But in many cases it can be shortened very considerably. It would require many examples to show how this can best be done under many varying circumstances. At present I give only a few examples to show the operation according to the general rule, and of one or two ways of shortening the work.

Examples.—Find factors of 1443, 57, 110467, 8616460799, and 3979769.

Proposed number 1443
Next higher square 38^2 1444

Difference ... $1 = 1^2$

Then ... ... $38 + 1$ $38 - 1$

Factors are ... $39 \times 37 = 1443$.

Proposed number 57
Next higher square (8^2) 64

Difference ... 7

Add $8 \times 2 + 1$ (or $8 + 9$) ... $17 = 24$
„ $9 \times 2 + 1$ (or $9 + 10$) ... $19 = 43$
„ $10 \times 2 + 1$ (or $10 + 11$) ... $21 = 64 = 8^2$

Then ... ... $11 + 8$ $11 - 8$

Factors are ... $19 \times 3 = 57$.

Proposed number 110467
Next higher square (333^2) 110889

Difference ... 422
Add $333 \times 2 + 1$ (or $333 + 334$)... 667

$1089 = 33^2$

Then ... ... $334 + 33$ $334 - 33$

Factors are ... $367 \times 301 = 110467$.

Jevons's proposed number 8616460799
Next higher square (92825^2) 8616480625

Difference ... 19826
Add $92825 \times 2 + 1$ (or $92825 + 92826$) $185651 = 205477$ (not a sq.)
„ $92826 \times 2 + 1$ $185653 = 391130$ (not a sq.)

therefore, add fifty-four more serial numbers, to $92880 \times 2 + 1$; the sum of the additions will then be found to correspond with—

$$\left.\begin{array}{r} 92880^2 = 8626694400 \\ - 8616460799 \end{array}\right\} = 10233601 = 3199^2$$

Then ... ... $92880 + 3199$ $92880 - 3199$

Factors are ... $96079 \times 89681 = 8616460799$.

"C.W.M.'s" proposed number ... 3979769
Next higher square (1995^2) 3980025

Difference ... $256 = 16^2$

Then ... ... $1995 + 16$ $1995 - 16$

Factors are ... $2011 \times 1979 = 3979769$.

To find the lowest factors of 12267—

$$\left.\begin{array}{lr} \text{Proposed number ... ...} & 12267 \\ \text{Sum of digits being 18 it is divisible by } 3^2 = & 9 \end{array}\right\} = 1363$$

Next higher square (37^2) ... ... $= 1369$

Difference 6
Add $37 \times 2 + 1$ or $37 + 38$ 75

$81 = 9^2$

Then ... ... $38 + 9$ $38 - 9$

Lowest factors... $47 \times 29 \times 3 \times 3 = 12267$.

To find the factors of 73, by the general rule, 28 steps in the process are requisite, until the sum of additions to first difference reaches a square, $1296 = 36^2 = 37^2 - 73$.

Then $\left.\begin{array}{l} 37 + 36 = 73 \\ 37 - 36 = 1 \end{array}\right\}$ the only factors, therefore 73 is a prime.

Instead of 28 steps being taken, the process may be shortened thus:—

Proposed number 73
Next higher square (9^2)...81

— 36 64 (even squares)

First difference ... 8 . . . 28 . . . 56
Add $9 \times 2 + 1$ (or $9 + 10$)...19 (diff. 6) (diff. 2)

Second difference... 27 . . . 22 . . . 54

49 81 (odd squares).

It must be noted that the first difference (8) is an even number, and that the second (27) is an odd one. In line with 27, put down difference (22) between it and next higher odd square (49), and in line with 8, the difference (28) between it and the even square (36) next below 49. As the difference 6 (between 28 and 22) will not divide either without a remainder, the process must be repeated, with the succeeding higher even (64) and odd (81) squares, for each line, until we obtain two numbers, divisible by their difference, without remainder. In this example we find that the second step gives what is required. The difference (2) between 56 and 54, divides either.

Then $\frac{56}{2} = 28$, and $28 + 9$ (9 is first square root used) $= 37$;

or $\frac{54}{2} = 27$, and $27 + 10$ (10 is second sq. root used) $= 37$;

and $\left.\begin{array}{r} 37^2 = 1369 \\ - 73 \end{array}\right\} = 1296 = 36^2$, as stated above.

The following example of a shortening process is applicable only to proposed even numbers. As such are divisible by 2, it may not be of much practical use; and only of interest to show what can be done. If the proposed number be 328, 18 steps are requisite by general rule; but 7 steps are sufficient, thus:—

Proposed number	328	
Next higher square (19^2) ...	361	
Difference	33	
Add $2 \times 19 + 1$ (or $19 + 20$)	39 =	72
(or $20 + 21$)	41 =	113
(or $21 + 22$)	43 =	156
(or $22 + 23$)	45 =	201
(or $23 + 24$)	47 =	248 = 8 below 16^2.
(or $24 + 25$)	49 =	297 = 8 above 17^2.

It must be noted that at the 6th step the sum of additions is 248, or 8 less than the next higher square ($256 = 16^2$), and at the 7th step it is 297, or 8 more than the next lower square ($289 = 17^2$). The mean of these consecutive roots is 16½. The consecutive square roots, corresponding for the 6th and 7th steps, are seen to be 24 and 25. The mean is 24½.

$$\left.\begin{array}{l} \text{Then } 24\tfrac{1}{2} + 16\tfrac{1}{2} = 41 \\ \phantom{\text{Then }} 24\tfrac{1}{2} - 16\tfrac{1}{2} = 8 \end{array}\right\} \text{therefore } 41 \times 8 \text{ are factors of } 328.$$

There are other short ways of working the process, varying with different numbers. For high numbers, when the difference between the factors is very great, I have not completed a shortening process that is altogether satisfactory, but I hope to succeed before long.

The examples given above are worked out by means of "increasing squares, roots, and numbers." Similar results may generally be obtained by operating with "decreasing squares, roots," &c., but I prefer the "increasing" method. To show the process by "decreasing squares," &c., I give two simple examples—

Proposed number	65
Next lower square (8^2)	64
Their sum is	129
Deduct $2 \times 8 - 1$ (or $8 + 7$) ... =	15
	114
,, $2 \times 7 - 1$ (or $7 + 6$) .. =	13
	101
,, $2 \times 6 - 1$ (or $6 + 5$) ... =	11
	90
,, $2 \times 5 - 1$ (or $5 + 4$) ... =	9
	$81 = 9^2$

$$\begin{array}{lccc} \text{Then} & 9 & & 9 \\ & +4 & & -4 \\ \text{Factors are} & 13 & \times & 5 = 65. \end{array}$$

Proposed number	60	
Next lower square (7^2)	49	
Their sum	109	
Deduct $7 \times 2 - 1$ (or $7 + 6$) =	13	
Mean 5½	$96 = 4$ from 10^2	Mean 9½
($6 + 5$) =	11	
	$85 + 4$ above 9^2	
Deduct $6 \times 2 - 1$ (or $5 + 4$) =	9	
Mean 3½	$76 - 5$ from 9^2	Mean 8½
($4 + 3$) =	7	
	$69 + 5$ above 8^2	
Deduct $5 \times 2 - 1$ (or $3 + 2$) =	5	
	$64 = 8^2$	

$$\begin{array}{lccccccccccc} \text{Then} & 8 & & 8 & \text{or} & 8\tfrac{1}{2} & & 8\tfrac{1}{2} & \text{or} & 9\tfrac{1}{2} & & 9\tfrac{1}{2} \\ & +2 & & -2 & & +3\tfrac{1}{2} & & -3\tfrac{1}{2} & & +5\tfrac{1}{2} & & -5\tfrac{1}{2} \\ & 10 & \times & 6 & & 12 & \times & 5 & & 15 & \times & 4 \end{array}$$

Showing 3 sets of factors for 60.

A table of squares and square roots, such as Barlow's, is requisite for enabling the operator to ascertain readily, as his work proceeds, when the sum of the additions to the difference between the proposed number and the higher square, first used, becomes a square; and also to show, in connection with shortening processes, to what extent they may differ, at any step, from being square numbeis.

Whether the principle of this method or rule be useful in working algebraical or other problems I am at present unable to say, but it can no longer be said there is not a direct rule for ascertaining the factors of any number, and consequently of showing whether it be a prime or not. It may be impossible to devise an algebraical formula, but there certainly is this simple arithmetical method, applicable to all numbers.

17 Morden Road, Blackheath. CHARLES J. BUSK.

THE FORMATION OF LEDGES ON MOUNTAIN-SLOPES AND HILL-SIDES.

IT is well known that Darwin attributed to the castings of earthworms the principal part in the formation of these ledges; although he mentions in his book the case of a valley in Westmoreland, where it was "in no way connected with the action of worms." "It appeared," he concludes, "as if the whole superficial, somewhat argillaceous earth, while partially held together by the roots of the grasses, had slided a little way down the mountain-sides; and in that sliding, had yielded and cracked in horizontal lines, transversely to the slope."[1]

Ledges of this description are exceedingly common on the mountain-slopes round Caracas, and indeed almost everywhere in Venezuela. They attracted my attention on my arrival in this country, and they did so all the more as it appeared to me impossible to admit the explanation given by the people, that they were the result of the trampling of cattle, for there were no cattle grazing on these slopes, nor could anybody give me trustworthy information that such had been formerly the case. I was pretty soon convinced that this peculiar feature of the surface was due to a downward sliding of the superficial layer, and after having read Darwin's book, a copy of which I had been so happy as to receive from himself in November 1881, I at once wrote to him about the ledges, stating that I believed the real cause of their formation to be what he had suggested in the passage quoted above. (Having no copy of my letter, I cannot give the exact wording of it.) The 3rd of April (only a fortnight before he closed his great life), he answered me as follows:—"Should you observe the ledges on the mountains, I shall like much to hear the results, though I do not suppose that I shall ever again publish on the subject. Since the appearance of my book, I have become doubtful whether I have not exaggerated the importance of worms in the formation of the ledges. Perhaps they may be due to the sliding down and horizontal cracking of (the) whole of the surface soil."

Since that time I have given a good deal of attention to the subject, and the result is, at least in this neighbourhood, absolutely in accordance with this latter suggestion. There is no reason for giving any importance to the action of worms, these animals being extremely rare in the soil of the slopes. I find in my note-book only six instances of their castings having been observed; and three, when by the tearing out of plants a worm was brought to light.

Our mountains are mainly built up of gneiss, which is rather easily converted on the surface into a kind of sandy loam. This surface soil is covered by a dense

[1] Darwin, "The Formation of Vegetable Mould through the Action of Worms" (London, 1881), p. 283.

vegetation of grasses with a number of other small plants, their roots being matted together in an almost continuous mass, possessing a certain degree of elasticity. During the rainy seasons this stratum absorbs a considerable quantity of water, getting thus much softer and offering less resistance to the underlying loam and its downward thrust. The whole mass is therefore in a state of plasticity, and consequently a sliding motion begins more or less as if it were a glacier. The slope not being uniform in all its parts, nor the disintegration of the rock everywhere of the same depth and degree, it follows that the sliding too will be unequal, and so an extraordinarily complicated system of stresses and counter-stresses is developed, which of course causes the surface to take a wrinkled or wavy appearance. I have tried in vain to find numerical values for the limits of sloping which allow of the formation of these surface-waves or ledges; the fact is that it depends to a considerable degree on the interior conditions of the soil and subsoil, which are not visible from outside. The lowest slope, however, that I have seen covered with ledges, was between 8° and 10°, the steepest 45°. The total amount of sliding soil is in some places far from being insignificant. I remember a locality in this neighbourhood, where there was twenty-five years ago a shallow depression, in which during each rainy season a small pool formed with *Najas microdon*, A.Br., *Wolffia Welwitschii*, Hegelm., and even *Marsilea subangulata*, A.Br. This depression has gradually been filling up, and is now on the verge of disappearing altogether, the material having been derived from a grassy slope on its northern side, which is covered by finely developed ledges.

My observations refer to the valley of Caracas; but as identical causes must be at work in other countries, it appears to me that everywhere the formation of ledges on mountain-slopes and hill-sides will probably depend, first of all, on the conditions of the ground and its vegetation, the action of earthworms being of secondary importance.

Caracas, January 6. A. ERNST.

FIG. 1.

A MOVABLE ZOOLOGICAL STATION.

IN Bohemia, much attention has been given for more than twenty years to the study of the fauna of ponds and lakes, but the work has been rendered difficult by the impossibility of the organisms being examined instantly in their habitats. The transportation of the material a long way has led to most of the finer objects being destroyed. Last year, a little movable station, suitable for real biological work, constructed after a sketch drawn by Dr. Ant. Fritsch, was presented by Mr. Ferdinand Perner to the Committee for the Physical Exploration of Bohemia; and there is good reason to hope that the use of this structure may be attended by important scientific results. There is room (12 square metres) for from two to four workers. The building consists of eighty pieces, the

total weight being 1000 kilogrammes. Two windows on the northern side are closed by wooden covers, which, when opened, present two ample working tables. The building-up of the station at the first place to which it was sent (a pond near Biechovic) required two hours and a half.

The scientific work began in the second half of June, and since that time, every week or fortnight, Dr. Fritsch with his assistants has visited the station. After the temperature of the air, and of the water on the surface and at different depths, has been noted, the surface fauna is taken by a tow-net (Fig. 2, 1). This contains mostly Copepods, Rotators, and *Daphnia kahlbergensis*, Schœdler. Then the fauna at a depth of 1 metre is taken by a net fixed on a long bamboo (Fig. 2, 2). The net contains generally the genera Daphnia, Bosmina, and Leptodora. Next, the fauna at a depth of 2 metres is taken by a long net (Fig. 2, 3), on which weights are fixed, and which is drawn out of the water by a string that closes the net by tightening it in the centre. This manipulation prevents the fauna from a depth of 2 metres from being mixed with that of the higher portions of the water. Large Daphnids are found here in great numbers. The same instrument is used in deeper parts of the pond. Mud is carried up by a strong net (Fig. 2, 4), and washed in sieves (7 and 8). The *Allona leydigii* is a common appearance there. These operations finished, the littoral fauna at various places is taken, consisting most commonly of large Sida and Lynceus. Fishing has been carried on in the same way by night, in January, under the ice-cover. Sometimes carp (*Cyprinus carpio*) have been taken at night, that the contents of their alimentary canal might be examined. The living material acquired in this manner is carefully killed by osmic and chromic acids, and preserved in strong alcohol.

The investigations will be continued throughout the year, and the results afterwards published in the *Archiv für naturwissenschaftliche Landesdurchforschung von Böhmen*. The station will, by and by, be transferred to some of the ponds in Southern Bohemia, or to one of the mountain lakes.

Fig. 2.

NOTES.

Arrangements are being made by the Berlin Academy of Sciences for an interesting scientific undertaking. During the summer of this year a ship is to be despatched for the investigation of the pelagic fauna of the Atlantic, especially along the coast from Greenland to Brazil. Prof. Hensen, of Kiel, will be at the head of the party, which is expected to start in July.

The death of M. G. Meneghini is announced. He had been Professor of Geology at Pisa from 1849, and died at the age of 78.

We have also to record the death of Dr. Heinrich Ernst Karl von Dechen, the eminent geologist and mineralogist, well known for his numerous works on geology. He was born at Berlin on March 25, 1800, and died at Bonn on February 15.

A LADY, Mdlle. G. Cattani, has been appointed a *privat-docent* of general pathology in the Faculty of Medicine at Turin.

PROF. E. WIEDEMANN has issued a circular about a matter which has considerable interest for men of science. Journals devoted to particular branches of science very often, as he points out, reprint papers which have been originally published by scientific Societies, and omit to indicate the sources from which the papers are taken. It also happens sometimes that articles are sent simultaneously to various periodicals, and appear sooner or later in all of them, either in the original language or in translations. The consequence is that those whose duty it is to look out for the latest information on any subject are put to a vast amount of inconvenience, since it is frequently impossible for them to feel sure that communications which seem to be new are really new. Prof. Wiedemann—who has of course had much experience, in connection with the *Annalen der Physik und Chemie*, of the trouble caused in this way—appeals to men of science generally, and to the scientific Press, to indicate, in every case in which a paper is reprinted, the fact that it has appeared before, and to state whether the paper has been shortened or expanded or otherwise changed.

ACCORDING to *Die Natur*, Dr. Otto Zacharias, of Hirschberg, Silesia, intends to establish a zoological station for the observation of fresh-water fauna. It is considered that the banks of the Plöner Lake in Holstein would be suitable for the purpose, and funds for maintaining the station for four years are now being collected.

AT the half-yearly meeting of the Forth Railway Bridge Company, held the other day in Edinburgh, very favourable reports as to the progress of the works were submitted. During the past six months the work of erection has proceeded more rapidly and satisfactorily than in any previous half-year, and at the present time the total weight of steel work erected, including the viaduct approach, is 43,500 tons. The last bays of the cantilevers are in an advanced state of manufacture in the shops, and the material is being delivered for the central girders connecting the cantilevers, the erection of which will complete the bridge.

DR. HANS REUSCH, the well-known Norwegian geologist, invites through the Press reports respecting the recent severe earthquakes in Norway. From those to hand it appears that the earthquake of December 23, at 12.15 p.m., was felt along the whole coast of the North Bergenhus province and in the districts around the Stat promontory. On December 27, about midnight, another shock was felt at Bremanger; and on January 6, at 8 p.m., one at Florö, also on the Bergen coast. On January 12, at 4.7 p.m., a fourth shock was felt in and around Bergen. Finally, a shock was felt in the neighbourhood of Christiansand on December 27, at 11.44 a.m.

WE have received from the Hydrographer of the United States a discussion, by Lieut. Everett Hayden, U.S.N., of the great storm of March 11–14, 1888, known as the New York "blizzard." In NATURE, vol. xxxviii. p. 204, we referred briefly to the behaviour of this memorable storm over the land; the discussion now in question deals with its action over the ocean, and more particularly off the Atlantic coast of the United States, from all data at present available. The meteorological conditions at noon (G.M.T.) for the area included between latitude 25° and 50° N., and longitude 50° and 85° W., are exhibited on charts for each of the above four days. The charts show that on the 11th a trough of low pressure was extending from the coast of Florida towards the southern limits of Hudson Bay, and was moving towards the coast at the rate of 600 miles a day. On the morning of the 12th the centre of the storm passed almost directly over New York, blowing with hurricane force, the barometer reading 29·2 inches. On the 13th, the storm area was still skirting the coast of the United States, the centre being about midway between New York and Boston, and the barometer had fallen to 28·9 inches. By the 14th, the great wave of low barometer had overspread the entire western portion of the North Atlantic. The discussion furnishes an instructive example of a somewhat uncommon class of storms, where the usual law founded on the circular theory was to a large extent inapplicable.

THE Pilot Chart of the North Atlantic Ocean for February, issued by the Hydrographer of the United States, shows that the weather in the Atlantic during January was somewhat milder than usual. No severe storms were reported during the first three weeks of the month. There was a noticeable increase in the amount of fog encountered, but it was confined principally to the neighbourhood of the Grand Banks and the British Isles. A supplement to the Pilot Chart describes the remarkable cruise of the derelict American schooner *W. L. White*, which was abandoned off Delaware Bay during the great blizzard, on March 13, 1888. She started off to the southward under the influence of the in-shore current and the north-west gale. Upon reaching the Gulf Stream she turned away to the eastward, and commenced her long cruise towards Europe. On reaching the mid-ocean, between latitude 44° and 51° N., and longitude 33° and 44° W., she followed a remarkable zig-zag track, from the beginning of May till the end of October, being drifted backwards and forwards by the Labrador current and the Gulf Stream, and during these six months alone she was reported by thirty-six vessels. On January 23 she was stranded on one of the rocky islands of the Hebrides, after drifting for ten months and ten days, and traversing a distance of more than 5000 miles.

THE Hydrographer of the Navy has issued a notice that storm-signals are now shown at Manilla (Philippine Islands). The signals consist of (1) a drum (colour not stated), indicating a storm at a great distance, in an unknown direction; (2) a cone, point upwards or downwards, indicating that a cyclone will pass some distance to the northward or southward, respectively; (3) a cone, point upwards over or under a drum, indicating that a cyclone will pass close to the northward or southward; (4) a ball, signifying that a typhoon is approaching, and that all traffic is prohibited. The signal-staffs are painted white.

IT is estimated that the magnificent stalactite cave lately discovered near Reclère, Canton Berne, is about 1600 metres long, 600 metres broad, and from 4 to 20 metres high. The greater part of it has not yet been investigated. A pool has been discovered, measuring 25 metres square, and it is supposed to be the only one in the cave.

A WRITER in the American *Monthly Microscopical Journal*, calls attention to what seems to be a real danger in connection with the kissing of the Bible in courts of law. "The lips," he says, "are most sensitive to the reception of disease-germs, and from the motley throng of dirty and diseased persons who appear in court and kiss the book, what infectious germs may not be obtained through this medium of distribution? It would be interesting for microscopists to examine such greasy and worn backs of court Bibles as they can have access to, and to report the kinds and amounts of Bacteria found thereon. . . . In a Massachusetts school where scarlet fever and measles had prevailed, some text-books fell into disuse, were put away for a time, and, when wanted, got out and re-distributed, several months having

elapsed. In but a few days after the re-issue of the books the children began to come down with measles. There can be little doubt that scarlet fever is transmitted in the same way."

SIR HENRY PEEK has compiled an interesting catalogue of his collection of birds at Rousdon. The catalogue consists of two parts. The first is accompanied by an outline index plate of each case, by means of which one may identify every bird in the case, and find out its name and some other details regarding it, by consulting the list. The second part consists of an alphabetically arranged list of both the English and the scientific names of every bird, with reference to its case, compartment, and number on the index plate, so that any particular bird required may be found without difficulty. The index plates, as Sir Henry Peek points out in the preface, save the necessity of affixing labels or numbers to the birds, which would have interfered with the artistic appearance of the groups.

UNDER the title "The True Position of Patentees," Mr. H. Moy Thomas has published, through Messrs. Simpkin, Marshall, and Co., a little book in which the patent laws and regulations at home, abroad, and in our colonies and dependencies, are explained for the information of English inventors.

MESSRS. MACMILLAN AND CO. will publish in September the Eleventh Book of Euclid, Propositions 1–21, with alternative proofs, exercises, and additional theorems and examples, by Mr. F. H. Stevens, of Clifton College, joint editor of Hall and Stevens's Euclid, Books i.–vi. Later on it is the intention of Mr. Stevens to issue a book on elementary solid geometry and mensuration, containing the matter included in the above-mentioned volume, with a section on polyhedrons and solids of revolution, treated geometrically and numerically, with exercises on the mensuration of plane and solid figures.

MESSRS. LONGMANS AND CO. have in the press "A Handbook of Cryptogamic Botany," by A. W. Bennett and George Murray. No general hand-book of Cryptogamic botany has appeared in the English language since Berkeley's, published in 1857. The present volume will give descriptions of all the classes and more important orders of Cryptogams, including all the most recent discoveries and observations.

A NUMBER of remarkable salts of hydrazine or amidogen have been prepared by Drs. Curtius and Jay, in the course of their work upon the hydrazine compounds, which has so successfully resulted, as noticed in these columns a fortnight ago (p. 377), in the isolation of the hydrate of amidogen, $N_2H_4 . H_2O$. Amidogen, as indicated by the strong alkalinity of its hydrate, and by the well-known reactions of its organic substitution-products, is a powerful base, and combines with acids to form salts of considerable stability at ordinary temperatures. Perhaps the most interesting of these salts, and the one utilized for the preparation of the liquid hydrate itself, is the di-hydrochloride, $\begin{array}{l} NH_2 . HCl \\ | \\ NH_2 . HCl \end{array}$. This salt possesses a most striking resemblance to ammonium chloride, crystallizing in the cubic system, and depositing, on rapid evaporation, in the same beautiful feathery forms. It differs, however, from ammonium chloride in being very deliquescent. When heated to 198° C., it fuses, with evolution of hydrochloric acid, to a clear glass consisting of the mono-hydrochloride, $\begin{array}{l} NH_2 . HCl \\ | \\ NH_2 \end{array}$. On further heating to 240°, it is entirely decomposed, with evolution of free nitrogen and hydrogen and sublimation of ammonium chloride—

$$2 \begin{array}{l} NH_2 . HCl \\ | \\ NH_2 . HCl \end{array} = 2NH_4Cl + N_2 + H_2 + 2HCl.$$

Contrary to the usual behaviour of bases of the ammonium type, platinum chloride in acid solution gives no platinochloride; the reducing action comes so violently into play that the platinic salt is reduced to platinous chloride, with copious evolution of nitrogen gas. The sulphate, $\begin{array}{l} NH_2 \\ | \\ NH_2 \end{array} . H_2SO_4$, like ammonium sulphate, is anhydrous, and crystallizes in optically biaxial prisms. It is the most insoluble of the hydrazine salts in cold water, and is precipitated from cold solutions of all the other investigated salts by dilute sulphuric acid; it readily dissolves, however, in hot water. On heating it melts at 254° with explosive evolution of gas, breaking up into ammonium sulphite, sulphurous acid, sulphuretted hydrogen, and, most remarkable of all, large quantities of free sulphur. The carbonate is obtained by saturating a solution of the hydrate with carbonic acid gas, and evaporation *in vacuo* as a highly deliquescent mass. The nitrate can readily be obtained from the carbonate by treatment with nitric acid; it crystallizes well, and is also extremely soluble. The acetate and oxalate have also been obtained in fine crystals. All these salts possess the same exceptional reducing powers, free nitrogen and water being formed in the process. They all decompose on heating with formation of salts of ammonium, and of gaseous nitrogen and hydrogen. One of their most important properties is their reaction with nitrous acid; on mixing any salt of hydrazine with a salt of nitrous acid, free nitrogen is evolved with almost explosive violence. It is finally of considerable significance that those which crystallize well appear to be isomorphous with the corresponding ammonium salts.

A BELGIAN, M. Nizet, of the Royal Library of Brussels, proposes to issue a periodical catalogue or table of all papers published in all the periodicals in the world. Of course the titles of the papers are to be methodically grouped under a certain number of divisions.

THE following are the arrangements for science lectures at the Royal Victoria Hall during the month of March:—Tuesday, March 5, "A Visit to the Moon: how we got there and what we saw," by Prof. Carlton Lambert; Tuesday, March 12, "A Tramp among the Mountains at Home and Abroad," by Prof. Kennedy, F.R.S.; Tuesday, March 19 and 26, two lectures, by Prof. H. E. Armstrong, on the "History of a Crime unravelled by a Piece of Rusting Iron, a Horse-shoe, and a Match."

THE *Indian Forester*, in a review of the work of the Forest Department under the rule of Lord Dufferin, says that the total number of prosecutions for offences against the forest laws has been steadily diminishing. The area under protection from fire has risen from 15,570 to 18,691 square miles. Large areas have been withdrawn from nomadic grazing; and, generally speaking, the old and wasteful methods have been displaced by an entirely new system, which is only now making itself felt. The influence of forest conservancy on the rainfall, the temperature, and the water-supply is becoming appreciated by the people of India. Large additions have been made to the teak plantations of Burma, and great quantities of mahogany have been planted at Nilambur. The revenue has risen from 67 lacs in the years 1876–80, to 94 lacs during the viceroyalty of Lord Ripon, and to 116 during that of Lord Dufferin. These figures do not include the money received from Upper Burma, where the loosely-drawn leases have given infinite trouble to the officials. The staff is too weak for the work, but a reorganization scheme is under consideration. The forest class at Cooper's Hill, and the working of the Dehra Dun School, will no doubt improve the standard of knowledge of the officials.

IN the recent Report of the Bombay Chamber of Commerce it is said that the opinion of the Chamber was solicited as to the advisability of introducing a uniform standard of weight through-

out the Presidency. A Committee was appointed, but there was some difference of opinion amongst the members as to the most convenient standard. Some were in favour of adopting the scale used by the railway companies in Bombay—that is, 180 grains = 1 tola, and so on up to 1 candy; but the majority thought that the English system of hundredweights, quarters, and pounds would be the best, and to this view the Chambers of Commerce of Bengal, Madras, and Kurrachee have assented. The Committee, however, refrained from urging on the Government to alter the present standards. The Government of India have appointed a Commission to inquire into the whole matter.

M. RESIN has published, in the *Izvestia* of the Russian Geographical Society, interesting sketches of the natives of Kamchatka, made during a cruise on the schooner *Sibir*. It appears that the Kamchadales and the Aleutes are rapidly abandoning their mother language, and speak a very broken Russian—the more broken as the Russians themselves, in order to be better understood, speak a language which has hardly any likness to their mother tongue. The population of Kamchatka, which was carefully registered in 1878 and 1879, shows a regular decrease; since the time of Krasheninnikoff's journey (in 1741) the population seems to have been reduced to one-half of what it was 148 years ago. Years of scarcity of fish, the staple food of the population, are quite common. In such cases the Lamutes and the Koryaks usually bring to the Kamchadales a number of their reindeer; but this voluntary help is not sufficient to prevent starvation. Hunting becomes less and less profitable. The following figures represent the results of hunting during the winter of 1884–85: 2,915 sables, 159 foxes, 321 otters, 302 ermines, 120 mountain sheep, and 767 reindeer. It is noteworthy that no diminution of population is remarked among the Tchukches on the coasts of the Arctic Ocean.

LAST week we quoted from Prjevalsky's "Fourth Journey to Central Asia" a description of fiery sunsets he had observed in crossing the Gobi. The same work contains the following remarks as to the action of wind upon the soil in the deserts of Central Asia:—"One must see the wind blowing in the desert to appreciate its force. Not only dust and sand fill the atmosphere, but sometimes smaller gravel is lifted into the air, while larger stones are rolled over the surface of the soil. At the foot of the Altyn-tagh, in the neighbourhood of the Lob-nor, we saw how stones as big as a man's fist, having been whirled into the hollow of a larger piece of rock by the storm, were making deep holes, and had even pierced through a slab of sandstone 2 feet thick." In accordance with Richthofen's views, Prjevalsky was inclined to explain the formation of loess by the force of wind, but he was not aware of the geographical distribution of loess—that is, its disposition as a girdle along the foot of all the mountains of Central Asia, and its absence on the plateaus, even in the parts which are protected from wind. On the other hand, the winds of the desert really afford an explanation of the strange coarse gravel, quite devoid of any particles of finer sand and loam, which covers the Gobi. The winds must have blown away the finer parts of the gravel.

WE have received the Triennial Calendar of the Tungwen College of Pekin, at which over a hundred students are prepared for the Government service. The full course, literary and scientific, extends over eight years, the first three being devoted exclusively to foreign languages, and the remainder to the acquisition of scientific and general knowledge through the medium of these languages. After the completion of the general course, students may, if they please, remain in the College, or be sent abroad at the option of the Government, for the pursuit of special studies with a view to professional use. The method of paying the students is thus described in the Calendar:—The object of the College being to train men for special service, the number is limited, and all admitted are, with a few exceptions, paid by the Government. During the first year the student is on probation, and receives his food and lodging only. He then gets an allowance of about three taels (14*s.* or 15*s.*) a month. In two or three years this stipend is doubled if the student progresses satisfactorily, and it is again increased to ten taels a month at the end of five or six years. If a student is sent to pursue his studies in foreign countries, the allowance is one hundred taels a month, which is increased to two hundred taels when he is made a third-class interpreter. The preparation of books for the diffusion of scientific and general knowledge is part of the work of the College. Amongst those prepared and published already are works on natural philosophy, chemistry, practical economy, chemical analysis, mathematical physics, anatomy, astronomy, &c.

IN the "Note" last week about a Greenland whale which went ashore in the Sound (p. 398), *for* "90 feet in length" *read* "60 feet in length."

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. Nicol; an Indian Chevrotain (*Tragulus meminna* ♀ juv.) from India, presented by Mr. George Score; a Hybrid Polecat (between *Mustela putorius* and *M. furo*), British, presented by Mr. J. Herbert B. Cowley; an Owen's Apteryx (*Apteryx oweni*) from New Zealand, presented by Prof. T. Jeffrey Parker, C.M.Z.S.; a Common Buzzard (*Buteo vulgaris*) from Spain, presented by Captain J. V. Harvey; eight Common Swans (*Cygnus olor*), European, deposited; seven Common Gulls (*Larus canus*), from Holland, five White's Tree Frogs (*Hyla cœrula*) from Australia, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *MARCH* 3–9.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 3

Sun rises, 6h. 42m.; souths, 12h. 12m. 3·0s.; sets, 17h. 42m.: right asc. on meridian, 22h. 57·9m.; decl. 6° 37′ S. Sidereal Time at Sunset, 4h. 29m.

Moon (at First Quarter on March 9, 18h.) rises, 7h. 48m.; souths, 13h. 35m.; sets, 19h. 35m.: right asc. on meridian, 0h. 20·7m.; decl. 3° 6′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 47 ...	10 36 ...	15 25 ...	21 21·9 ...	14 18 S.
Venus.....	7 39 ...	14 57 ...	22 15 ...	1 43·8 ...	14 6 N.
Mars......	7 30 ...	13 54 ...	20 18 ...	0 40·5 ...	3 56 N.
Jupiter....	3 37 ...	7 33 ...	11 29 ...	18 18·4 ...	23 4 S.
Saturn....	14 46 ...	22 23 ...	6 0* ...	9 10·9 ...	17 30 N.
Uranus...	21 12* ...	2 36 ...	8 0 ...	13 20·1 ...	7 45 S.
Neptune..	9 22 ...	17 5 ...	0 48* ...	3 51·4 ...	18 29 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Mar.	h.	
4 ...	0 ...	Mars in conjunction with and 5° 2′ north of the Moon.
5 ...	11 ...	Venus in conjunction with and 8° 57′ north of the Moon.
5 ...	20 ...	Venus at least distance from the Sun.

Saturn, March 3.—Outer major axis of outer ring = 45″·2; outer minor axis of outer ring = 12″·5: southern surface visible.

Meteor-Showers.

	R.A.	Decl.	
Near Capella...	50 ...	48 N.	
From Coma Berenices...	190 ...	25 N. ...	March 8.
Near γ Herculis	244 ...	16 N. ...	March 7. Very swift.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′			h. m.	
U Cephei	0 52·5	... 81 17 N. ...	Mar.	4,	18 8	*m*
R Arietis	2 9·8	... 24 32 N. ...	,,	4,		*M*
Algol	3 1·0	... 40 32 N. ...	,,	3,	2 41	*m*
			,,	5,	23 30	*m*
R Canis Majoris...	7 14·5	... 16 11 N. ...	,,	3,	21 6	*m*
		and at intervals of			27 16	
V Geminorum ...	7 16·9	... 13 18 N. ...	Mar.	3,		*M*
V Leonis	9 53·9	... 21 47 N. ...	,,	9,		*M*
U Coronæ	15 13·7	... 32 3 N. ...	,,	4,	2 50	*m*
T Herculis	18 4·9	... 31 0 N. ...	,,	7,		*m*
β Lyræ...	18 46·0	... 33 14 N. ...	,,	4,	22 30	*M*
			,,	8,	4 0	m_2
R Lyræ	18 52·0	... 43 48 N. ...	,,	5,		*M*
Y Cygni	20 47·6	... 34 14 N. ...	,,	4,	5 40	*m*
			,,	7,	5 40	*m*
δ Cephei	22 25·0	... 57 51 N. ...	,,	7,	3 0	*M*

M signifies maximum ; *m* minimum ; m_2 secondary minimum.

GEOGRAPHICAL NOTES.

THE paper read at Monday's meeting of the Royal Geographical Society was by Captain Vangèle, giving an account of his exploration of the Welle-Mobangi river, the great northern tributary of the Congo. His first exploration was made in the end of 1886 in a flat-bottomed boat, the *Henry Read*, with a stern paddle-wheel. On this first journey Captain Vangèle did not succeed in getting beyond Mr. Grenfell's furthest, the Zongé Falls, just where the river turns sharply to the east. He gives an interesting account of the Ba-Ati, the people who inhabit the banks of the river, and who are in every way of a superior type, though cannibals. A little distance above its mouth the Mobangi or Ubangi measures about 2730 yards in breadth ; its greatest depth is 5 fathoms, its lowest 1 fathom ; it flows at the rate of $3\frac{1}{4}$ feet a second. Under the 4th degree, just below the rapids, it still has a breadth of 1300 yards, a depth of 4 fathoms, and a velocity of 4 feet a second. Between these two points, though continually varying in breadth, it never exceeds about 4000 yards, including the islands. The general appearance of the river is pretty much the same as that of the Congo near Bolobo—strewn with islands, and having low wooded banks. The colour of the water is a light brown. Captain Vangèle's second journey was made a year later, and with better means of forcing his way up the rapids of the Mobangi. This time, though he encountered several obstacles, he managed to push his way up the river to over 22° E. longitude, and to within sixty miles of Junker's furthest point on the Wellé. This has been accepted as clearly proving the identity of these two rivers, so that the long-standing problem of the Wellé may be regarded as solved. At his furthest point Captain Vangèle had to turn back owing to the hostility of the natives, the only instance in which he met with real opposition. Between rocks and islands, rapids and cataracts, the navigation of the lower Mobangi is beset with difficulties, though it is evidently practicable with suitable vessels, and a thorough knowledge of the river. The river is subject to great variations of level, according to the season of the year. Above the Zongo Falls, the people, named Bakombé, differ considerably from those on the lower river, and evidently spread far inland. From above the Zongo rapids the river opens out, flowing straight from the north-east, and the outlook is described as superb. It is free from all obstacles, from 900 to 1000 yards wide, with a depth of $12\frac{1}{2}$ fathoms, flowing between banks 6 to 10 feet high, grassy plains alternating with clusters of trees. After thirty miles in the north-east direction the river turns due east, which direction it maintained to the end of the voyage, 170 miles. The banks are densely inhabited, and provisions of all kinds abound. Between the Zongo Falls and the steamer's furthest point only one tributary was met with—the Bangasso—coming from the north. After the paper was read, Sir Francis De Winton made some remarks with regard to the position of Mr. Stanley. He totally disbelieves the conjecture of Lieut. Baert that Stanley has any intention of taking Khartoum. On the contrary, Sir Francis believes he is now on his way home by the east coast.

IN the last issued number (4ᵉ trimestre, 1888) of the *Bulletin* of the Paris Geographical Society will be found a very complete examination of the route for a proposed Euphrates Valley Railway, by M. A. Dumont. M. Adrien Blondel contributes a detailed account of the Island of Réunion. M. Jules Marcou, in concluding his paper on the origin of the name of America, decides against Vespucci and in favour of an aboriginal place-name.

IT has been arranged that the eighth German *Geographentag* shall be held at Berlin from April 24 to 26 next.

THE *Ceylon Observer* states that Mr. Stephens, who has recently been amongst the Veddas of Ceylon, and who subsequently explored New Guinea, is now in Ceylon on his way to Singapore to organize an expedition at the instance of Prof. Virchow to explore the unknown portions of the Malayan Peninsula. Mr. Stephens's instructions are to start from Malacca and travel north-north-west through the vast expanse of unexplored territory which stretches northwards for some 500 miles. There are on the coast various settlements near mines and plantations, but the greater portion of the interior has been hitherto unexplored. The inhabitants are said to be jealous and bloodthirsty.

M. LŒWY'S INVENTIONS AND RESEARCHES.[1]

IT is now my pleasing duty to lay before you the grounds on which the Council have awarded the gold medal to M. Maurice Lœwy for his invention of the equatorial *coudé*, of a new method of determining the constant of aberration, and for his other astronomical researches.

On examining the series of memoirs in which M. Lœwy has set forth his new methods of astronomical research, we are at once impressed by the originality of conception which characterizes all his ideas, and by the thoroughness with which he has worked out the details necessary for the practical application of his new methods of observation. Observational astronomy has for many years past proceeded on such well-defined lines, that we have not unnaturally come to look rather to improvements of detail than to the introduction of new instruments for the advancement of our knowledge. It is, therefore, a matter of great satisfaction to find that M. Lœwy has placed at our disposal various methods of observation based on entirely new principles, and calculated to give astronomers improved and quite independent means of attacking several of the most important problems in our science.

The first of these new instruments with which I will deal is the equatorial *coudé*.

It was in the year 1871 that M. Lœwy proposed his new form of equatorial, to which the name of "equatorial *coudé*" has been given, and M. Delaunay, then Director of the Paris Observatory, was so struck with the value of the principle that he arranged for the construction of an instrument on this plan. M. Delaunay's death, however, interrupted the work, and the first equatorial *coudé*, having an object-glass of 0·27 metre, or about $10\frac{5}{8}$ inches aperture, was not completed till the year 1882. The success of this instrument was so marked that its value could not fail to be recognized, and it was not long before the construction of several larger equatorials on the same principle was commenced. At the present time six equatorial *coudés* have been completed, and four of these are already mounted and in regular use at the Observatories of Paris, Lyons, Besançon, and Algiers. The other two are intended for the Observatories of Paris and Vienna.

In principle the equatorial *coudé* may be described as an adaptation of the form of transit instrument with axial view to the requirements of an equatorial, by the addition of a plane mirror inclined at 45° outside the object-glass, this mirror being capable of rotation about the axis of the telescope, so as to reflect into the latter the rays from any object in a perpendicular plane. The axis of the instrument is mounted as a polar axis between two piers, the telescope being broken at a right angle near the lower pivot, so that the rays from the object-glass are reflected by an internal mirror up the polar axis to the hollow upper pivot, where the image is formed. The rotation of the outer mirror thus brings into the field the image of any object in the hour-circle perpendicular to the object-end of the telescope, and by the rotation of the polar axis, as in an ordinary equatorial, the telescope is directed to any hour-angle. The declination-axis in the equatorial *coudé* is the axis of the object-end of the telescope about which the outer mirror turns, and the declination-circle placed at the eye-end, in the same plane with

[1] Address delivered by the President of the Royal Astronomical Society, Mr. W. H. M. Christie, F.R.S., Astronomer-Royal, on presenting the Gold Medal of the Society to M. M. Lœwy at the anniversary meeting on February 8, 1889.

the hour-circle, is connected with the axis of the outer mirror by gearing, so that the observer at the stationary eye-piece has both the hour and declination circles immediately under his eye. He can thus direct the instrument to any object without moving from his chair, and his observations are made under the most favourable conditions for his own comfort, similar to those under which the microscope is used by the student of natural history. The observing-room, which may be artificially warmed, is quite separated from the object-glass, and other external parts of the instrument. These latter are protected from the weather by a suitable hut, which can be rolled away on rails before observing, so that the optical parts of the equatorial are in the open air under the best conditions for establishing an equilibrium of temperature.

The importance of obtaining the favourable conditions for observation secured by M. Lœwy's equatorial *coudé* has long been recognized, and various attempts have been made to enable the observer to command any part of the sky without changing his position. In 1858, Dr. Steinheil proposed[1] a new method of mounting a reflector, so that the axis of the concave mirror formed the polar axis, the rays from a star being reflected down the axis to the concave mirror by a plane mirror, which could be rotated about a declination-axis and a polar axis. The observer looked down the polar axis through a hole in the plane mirror, but with this arrangement he could not observe stars much north of the equator unless the plane mirror were made very large, and the range of the equatorial was thus very restricted. A more extended range might be obtained by interchanging the concave and plane mirrors, so that the observer would look up in the direction of the pole; but the concave mirror and its support would block out the view of the region near the pole, and of all the sky below the pole. Sir H. Grubb has applied the same principle to the construction of a siderostatic refractor.

As compared with Dr. Steinheil's form, the equatorial *coudé* possesses the great advantage of commanding every part of the sky, the arm of the telescope below the elbow being made long enough to project beyond the sides of the observing-room when viewing objects near the meridian.

The siderostat of Foucault, though useful for many purposes, is open to the same objection as Dr. Steinheil's, of not permitting of a view of every part of the sky; and there is the further difficulty that the apparent direction of the diurnal motion is continually changing. In the equatorial *coudé* this direction changes with the declination, but M. Lœwy has now arranged that the micrometer is turned with the declination-circle, and is thus always set to the zero of position-angle.

The success obtained by M. Lœwy in the construction of the equatorial *coudé* is due to the following circumstances:—

(1) The absence of flexure in the mirrors, which are made much thicker than usual.

(2) The more perfect achromatism secured by the greater focal length which this form of mounting allows of.

The first condition was established by careful experiment, which showed that in order to avoid deformation by flexure the thickness of a mirror should be between one-fifth and one-sixth of the diameter, instead of one-ninth or one-tenth as had been usual hitherto.

As regards achromatism, M. Lœwy urges that, in order to be able to see better with a larger object-glass, the achromatism must be made more perfect, and that, therefore, the ratio of focal length to aperture must increase with the aperture in order to diminish the effect of the secondary spectrum.

Notwithstanding the two reflections, the definition obtained with the equatorial *coudé* appears to be very good, the components of ω Leonis, distant only 0″·5, having been separated with the Paris instrument, which has an object-glass of 0·27 metre or about $10\frac{5}{8}$ inches. With one of the new instruments of 0·31 metre, or $12\frac{1}{4}$ inches aperture, M. Trépied, at Algiers, easily divided γ^2 Andromedæ. The loss of light by the two reflections from silvered mirrors is computed by M. Lœwy at only 12 per cent., and it would seem that it is at any rate very small, as successful observations of a minor planet of 13·5 magnitude were obtained with the Paris instrument as well as of very faint nebulæ and comets. The comet 1885 *d* (Fabry) was discovered with this instrument.

One of the objects which M. Lœwy had in view in planning his equatorial *coudé* was to obtain greater stability than is attainable with ordinary equatorials, and to make the measurement of large angular distances possible. The form of mounting of the equatorial *coudé* seems peculiarly adapted to give great stability, provided the fixity of the mirrors in their cells can be secured, and this is a condition to which M. Lœwy has given special attention. Each mirror rests in its cell on thick felt or flannel, and is held by three clips, which are just brought into contact with it when in the horizontal position, as tested by the disappearance of the least trace of light between the clip and its reflected image. This adjustment being made for the horizontal position, in which the weight of the mirror has its full effect, perfect contact between the mirror and its clips will be maintained in all positions.

M. Lœwy, in conjunction with M. P. Puiseux, has investigated very completely the theory of the instrumental adjustments of the equatorial *coudé*, including the effect of flexure of the polar axis and of the telescope arm, and has shown the relation of his formulæ to those for ordinary equatorials. He arrives at the two following conditions of optical adjustment as sufficient for astronomical purposes:—

(1) The axis of the telescope arm should be perpendicular to the polar axis.

(2) The interior mirror should reflect to the centre of the field a ray entering the telescope along the axis of the arm, supposed to be perpendicular to the polar axis.

The discussion of the instrumental errors of the Paris instrument, partly by astronomical observations, and partly by means of a collimator attached to the mounting of the exterior mirror, shows a very satisfactory accordance in the determinations on different days, and in the result the instrumental errors were found to be very small, the largest amounting only to 23″. The coefficients of flexure are, however, rather larger quantities, being 91″ and 53″ for the polar axis and telescope arm respectively, as found by means of the collimator. It may be expected that in the new instruments the effects of flexure would be very much less, as important improvements have been made in their mechanical construction.

It is not a little remarkable that the first instrument made on this new principle should have given such excellent results, both optically and mechanically; and its success is evidence of the thoroughness with which M. Lœwy has worked out his idea, and of the skill with which MM. Henry and M. Gauthier have respectively carried out the optical and mechanical portions of the instrument.

I now pass on to M. Lœwy's new method of determining the constant of aberration. It is hardly necessary to insist on the importance of this constant, not only for obtaining the true positions of the stars, but, in a higher degree, for the determination of the solar parallax by means of the velocity of light. It must be admitted that the nine independent determinations of the constant of aberration made at Pulkowa with three different instruments show a satisfactory accordance, but in the opinion of M. Nyrén, who has published the latest researches on the subject, none of these can be asserted to be free from systematic error. M. Nyrén's definitive value is 20″·492, exceeding by 0″·047 W. Struve's original value, which has hitherto been generally used by astronomers. Under these circumstances, M. Lœwy's method, which is based on differential measures with an equatorial, constitutes a new departure of great value in astronomy of precision, and its value is enhanced by the circumstance that it is also applicable to the determination of the constant and law of refraction.

The principle of M. Lœwy's method is the measurement of the angular distance between two stars by means of a double mirror, formed by silvering two faces of a large prism of glass and placed in front of the object-glass of an equatorial. The double mirror is capable of rotation about the axis of the telescope, so that by reflection from the two silvered surfaces the images of two stars in different parts of the sky may be brought into the field side by side, and the distance between them measured in the direction of the common plane of reflection. In his memoir on the determination of refraction by the new method, M. Lœwy proves that the projection of the distance between the two images on the trace of the common plane of reflection is independent of the rotation of the equatorial, of any movements of the double mirror, and of the displacement of the images by the diurnal motion, when the observation is not made rigorously in the plane of reflection.

M. Lœwy's exposition of his method of determining the constant of aberration is contained in a series of communications made to the French Académie des Sciences and published in the *Comptes rendus*, vols. civ. and cv. In giving an account of this

[1] *Astron. Nachrichten*, No. 1138; *Monthly Notices*, vol. xix. p. 56.

investigation, I will proceed at once to the general method for determining aberration, which M. Lœwy discusses after treating some special cases.

The determination of aberration requires the measurement of the distance between a pair of stars at successive epochs when the effect of aberration on the angular distance is reversed. The observations are made when the two stars have the same altitude, so that the effect of refraction is a minimum, and the comparison of the two measures gives a multiple of the constant of aberration, which is independent of all instrumental errors and also of precession and nutation, as the distance between two stars is unaffected by any movements of the earth's axis or of the ecliptic. There is the further advantage in the new method, that the effect of aberration as measured is much greater than in the ordinary methods of observation.

But the result might be affected by change of refraction or by alteration in the angles of the double mirror resulting from thermal expansion between the two epochs of observation, and M. Lœwy has therefore imagined a general method of observation which eliminates any possible effects of the kind, as well as methods applicable to special cases which determine any changes due to refraction or expansion of the mirror.

The essence of the general method is that two pairs of stars are observed, the four stars being selected so that at the time of observation they are all simultaneously at the same altitude, and that the effects of aberration on the two arcs connecting the stars of each pair are large and of opposite sign. Thus the two arcs formed respectively by the two pairs of stars are compared simultaneously both at the first and at the second epochs.

The first point for investigation is the effect of aberration on the angular distance between a given pair of stars. From the geometrical conditions, M. Lœwy arrives readily at the result that the effect is proportional to the cosine of the angle between the median[1] of the arc and the direction of the earth's motion.

Calling Δ the angular distance between two stars, p the angle between the median of the arc joining them and the direction of the earth's motion, and k the coefficient of aberration, the effect of aberration is given by the formula—

$$d\Delta = 2k \sin \frac{\Delta}{2} \cos p.$$

It readily follows from this that the effect of aberration on the difference of the two arcs connecting two pairs of stars will be greatest when the two medians are on the same vertical circle on opposite sides of the zenith. Under these circumstances, the effect of aberration on the difference of the two arcs is equal to

$$4k \sin \frac{\Delta}{2} \sin \frac{\Delta'}{2} \cos \mathrm{L},$$

Δ' being the angular distance between the two medians, and L the angle between the direction of the earth's motion and the line of intersection of the vertical plane through the medians with the horizon. Thus the effect is proportional to the cosine of this angle, and the greatest effect will be obtained when the vertical plane of the medians, the ecliptic and the horizon intersect in the same line, and the observations are made at the two epochs six months apart when the direction of the earth's motion coincides with this line, L having the values 0° and 180° at the two epochs respectively. In that case the effect of aberration on the difference of the two arcs has opposite signs at the two epochs, and the comparison of the two sets of measures of the two arcs gives

$$\mathrm{E} = 8k \sin \frac{\Delta}{2} \sin \frac{\Delta'}{2},$$

where E is the difference of the two measures of difference of arcs at the first and second epochs respectively.

The next point for consideration is the choice of the angle for the double mirror, their angular distance (Δ) between the two stars in each pair being necessarily twice this angle. Obviously the altitude at which the observation of the four stars is made diminishes as Δ and Δ' increase, and M. Lœwy shows that the maximum effect at any given altitude is obtained by making $\Delta' = \Delta$, or the angular distance between the medians the same as that between the two stars in each pair. He then gives the following table of the altitude h and of the effect of aberration $\frac{\mathrm{E}}{k}$ corresponding to the several values of the angle of the double mirror α:—

α	30°	35°	40°	45°	50°	55°	60°
h	48° 35′	42° 9′	35° 58′	30° 0′	24° 24′	19° 12′	14° 29′
$\frac{\mathrm{E}}{k}$	2·0	2·6	3·3	4·0	4·7	5·4	6·0

M. Lœwy concludes that the angle of the double mirror should not exceed 50°, and he considers that, on the whole, it would be well to make it 45°, so that the altitude of the stars would be 30°, and the angular distance for each pair 90°. Under these conditions, observations made at two epochs six months apart would give as the quantity measured four times the constant of aberration, while the ordinary methods of observation only give at the maximum a measure of twice the constant. But, in order to avoid daylight observations, M. Lœwy thinks it would be advisable to be satisfied with a slightly smaller coefficient of k (the constant of aberration), say three instead of four, which would reduce the interval between the two epochs to about ninety-eight days; and, by combining the observations in the first five weeks with those in the last five, a series of equations would be obtained, in which the coefficient of k would vary from three to one, the mean value being about two. All the observations could then be made in the night hours.

Besides the general method of observation just described, M. Lœwy has, as already mentioned, devised two methods applicable to special cases which are well suited to give independent determinations of the constant of aberration.

The first method consists in the observation of two pairs of stars, of which one pair gives, at the end of two or three months, the measure of twice the constant of aberration, and the other, completely unaffected by aberration, exhibits the effect of temperature on the double mirror. The first pair of stars should be in the neighbourhood of the ecliptic; the second pair is, as will be seen from geometrical considerations, to be chosen so that the latitudes of the two stars are the same, and that their longitudes differ by 180°, in order that the arc joining them may be unaffected by aberration.

This method is, however, not applicable at observatories within 20° of the equator, and on this account, as well as to give another independent determination of the constant of aberration, M. Lœwy proposes a second method, according to which the angular distance of a single pair of stars near the ecliptic is to be observed for a period of three months or longer, the measures in the first and last twenty-five days of the period being used to determine the aberration, and those in the intermediate forty days to deduce the effect of temperature on the double mirror.

The question of the adjustment of the double mirror remains to be mentioned. This must be mounted so as to turn about the optical axis, and this axis should coincide nearly with the axis of figure. The effects of any movements of the double mirror will then be as follow:—

(1) In turning round the axis of figure the two images are displaced in opposite directions, but perpendicularly to the trace of the common plane of reflection.

(2) In turning round an axis in this plane and perpendicular to the axis of figure the two images move in the same direction perpendicularly to the trace of the plane of reflection.

(3) If the double mirror turns about an axis perpendicular to the plane of reflection, the two images move along the trace without changing their relative distance.

Reference has already been made to the applicability of M. Lœwy's new method to the determination of refraction at various altitudes. This was, in fact, the immediate object which M. Lœwy had in view when he devised the method, and his investigation of the conditions of the problem was communicated to the French Académie des Sciences early in 1886, the year before he published his memoir on aberration.

In his series of papers on the determination of refraction published in the *Comptes rendus*, vol. cii., M. Lœwy first gives a method for determining the constant of refraction, the law according to which refraction varies with the altitude being known. A pair of stars is observed when refraction has its maximum effect on their angular distance, and again when the effect of refraction is a minimum. For the maximum effect one of the stars must be on the horizon, and the other in the same

[1] The median is the line bisecting the angle between the directions of the two stars.

vertical circle with it, while for the minimum both stars must be at the same altitude. M. Lœwy then finds that the greatest variation of refraction will be obtained with an angle of 30° for the double mirror, but as with this there would be (for the latitude of Paris) a minimum interval of 6h. 35m. between the two epochs of observation, he prefers to take an angle of 45° for the double mirror, sacrificing only 15" in the effect of refraction, while reducing the interval between the observations to 4h. 44m. This is the minimum value of the interval found by selecting the pair of stars so that their common zenith distance at the second epoch is equal to the angle of the double mirror, or half the angular distance between the two stars.

The geometrical conditions thus found by M. Lœwy to give the maximum effect in the minimum interval of time between the observations may be somewhat modified in practice, provided the angular distance between the stars does not differ by more than a few minutes from twice the angle of the double mirror. M. Lœwy has thus been able to find some twenty pairs of bright stars suitable for the determination of refraction by this method. In its practical form the method consists in the measurement of the angular distance between a pair of stars 90° apart when one of the stars is near the horizon and the other near the zenith, and again when both the stars are at about the same altitude. It is not necessary that at the former epoch the low star should be very near the horizon, for, as M. Lœwy points out, observations may be advantageously continued till the altitude is nearly 20°, and thus the constant of refraction may be determined from observations which are practically unaffected by any uncertainty in the law of refraction.

It will readily be understood that the observation of the low star may be made either when it is rising or when it is setting. In the latter case the observation of the stars at equal altitude would precede that for which one of the stars is setting. By combining the observations of two pairs of stars chosen so that the first pair is rising when the effect of refraction on the second is a minimum, and that the first pair is at the minimum when the second pair is setting, the influence of any change in the angle of the double mirror will be eliminated by taking the mean of the two determinations, while the difference of these will give four times the change of angle in the interval, thus affording a precise determination of any such change, if it exists.

Various other methods are proposed by M. Lœwy for determining the refraction at any altitude without assuming its law of variation. These methods, however, appear to involve practical difficulties, as they either assume the absence of irregular variations in the refraction at an altitude of 10°, or require the construction of several double mirrors with different angles. They may be considered as supplementing the first method; and they are of interest as giving a direct measure of refraction independently of any theory.

The practical determination of the constants of aberration and refraction by the new method is being carried out by M. Lœwy and M. P. Puiseux with the equatorial *coudé* of the Paris Observatory, and the series of observations made during the past twelve months confirms in the most satisfactory manner the theoretical conclusions. M. Lœwy finds that the variations of the distances are really free from systematic errors, and he considers that the constant of refraction will be more accurately determined from a few nights' observations with his new method than from years of meridian observations.

In conclusion, I can only allude in the briefest terms to the other important researches for which astronomers are indebted to M. Lœwy. The following is a summary of the other new methods of instrumental research which M. Lœwy has devised in the last few years:—

(1) A method for determining the flexure of transit-circles at various zenith distances by means of an optical apparatus inserted in the central cube. This has been used to find the flexure of two transit-circles at the Paris Observatory, the absolute values of the flexure for the two ends of the telescope and for the axis being independently determined.

(2) A method for obtaining the latitude without making use of the declinations of fundamental stars.

(3) A general method for determining right ascensions without relying on assumed right ascensions of polar stars.

(4) A method for finding on each night the absolute declinations of stars without the necessity for observations of polar stars at upper and lower transit.

(5) Methods for determining directly the two co-ordinates of polar stars without a previous investigation of the instrumental errors.

All these methods except the first are based on the observation of close circumpolar stars in R.A. and N.P.D. out of the meridian at various points of the circles described by them. Conjugate observations either of a single star or of a pair of stars having the same N.P.D. are made with a transit-circle, having a field of view of 2°, at equal intervals (about two hours) before and after meridian passage or before and after passage over the hour-circle of 6h. east or west. The special methods of observation are developed in a series of communications to the French Académie des Sciences made in the years 1883 and 1885, and during the last two years M. Rénan has applied these new methods to a determination of the latitude of the Paris Observatory based on eighty very accurate results.

The account which I have given of M. Lœwy's inventions and researches is necessarily very imperfect, and I have had to pass over many points of interest in the application of his methods. But I trust that the summary I have made will at any rate suffice to show the very high importance of M. Lœwy's labours, and that they fully deserve the recognition which is to-day given to them, whether we have regard to the originality of the methods or to the value of the results which are to be obtained from them.

STRUCTURE, ORIGIN, AND DISTRIBUTION OF CORAL REEFS AND ISLANDS.[1]

THE picturesque beauty of the coral atoll, seated 'mid a waste of troubled waters, with its circlet of living green, its quiet, placid lagoon, and its marvellous submarine zoological gardens, has long been celebrated in the descriptions of voyagers to tropical seas. The attempt to arrive at a correct explanation of the general and characteristic form and features of these reefs and islands has, for an equally long period of time, exercised the ingenuity of thoughtfulmen.

Coral reefs are the most gigantic and remarkable organic accumulations on the face of the earth. They are met with in certain tropical regions, and are huge masses of carbonate of lime, secreted from ocean waters by myriads of marine organisms. While the great bulk of the reef consists of dead corals, skeletons, and shells, the outer surface is clothed with a living mantle of plants and animals. This is especially the case on the outer and seaward face of the reef, where there are, at all times, myriads upon myriads of outstretched and hungry mouths, and not the least interesting questions connected with a coral reef are those relating to how these hungry mouths are satisfied.

It is to the power of these organisms of secreting carbonate of lime from sea-water—building up and out generation after generation on their dead selves—that the coral reef owes its origin. So wonderful and unique is the result, that combination for a definite end has sometimes been attributed to these reef-builders.

There is, however, another process ever at work in the ocean, in a sense antagonistic to that of secretion of carbonate of lime by organisms, which has much to do in fashioning the more characteristic features of coral reefs. This is the solution of all dead carbonate of lime shells, skeletons, and calcareous *débris*, wherever these are exposed to the action of sea-water. As soon as life loses its hold on the coral structures, and wherever these dead carbonate of lime remains are unprotected by rapid accumulation or crystalline depositions, they are silently, surely, and steadily removed in solution. This appears to be one of the best established oceanographical facts, and any theories concerning the general economy of the ocean which fail to take account of this universal agency are most likely to be at fault. We know something about the rate of solution, probably more than we do about the rate of growth and secretion of carbonate of lime by the coral Polyps. It has been shown that the rate of solution varies with temperature, with pressure, and with the amount of carbonic acid present in the water. It is on the play of these two opposing forces—the one vital and the other chemical—and their varying activity in different regions and under different circumstances, that we rely for the explanation of many oceanographical phenomena, especially many of those connected with oceanic deposits and coral reefs. In some regions there may be more growth, secretion, and deposition of shell and coral materials than solution

[1] Lecture delivered by Dr. John Murray at the Royal Institution on Friday, March 16, 1888. Recently revised by the Author.

by sea-water, and then there results the formation of coral reefs and vast calcareous deposits at the bottom of the ocean. There may be an almost exact balance between these processes. And again, there may be more solution than secretion, as, for instance, in the red clay areas, which occupy the deepest parts of the ocean, and in some coral-reef lagoons.

What is the nature of the foundations of these coral islands, surrounded as they sometimes are by an ocean miles in depth? Why have some elongated reefs no lagoons? Why have most of the lagoons of the smaller atolls been filled up? Why is the circle of land or reef in the perfect atolls only, at most, a few hundred yards in diameter? What is the origin of the lagoon? What relation exists between the depth of the lagoon, its area, and the depth of the water beyond the outer reef? How has the dry land of these islands been formed, provided with a soil, a fauna and a flora? These appear to be the chief questions that demand an answer from any theory of coral island formation.

These coral formations are essentially structures belonging to the great oceans and ocean basins. They are dots of land within the oceanic areas that might be compared or contrasted with the small salt lakes which are scattered over the surface of the continental lands. A rapid survey of some of the more general phenomena of the great oceans may, then, lead to a better appreciation of the problems connected with coral reefs.

The great ocean basins occupy over two-thirds of the earth's surface, and have a mean depth of over two miles. The central portions of these basins, called the abysmal regions, occupy about one-half of the earth's surface, and have a mean depression below the general level of the continents of over three miles. The abysmal regions are vast undulating plains, sometimes rising to less than two miles from the surface of the sea, and again sinking to four and five miles beneath it. Volcanic cones rise singly or in clusters from these great submerged plains. When they shoot above the level of the sea they form single islands, like Ascension and St. Paul's Rocks, or groups, like the Azores, the Sandwich, the Fiji, and the Society Islands. As might have been expected, there are many more of these cones hidden beneath the waves than rise above them. When the *Challenger* sounded along the west coast of Africa, there was no suspicion that between her stations she was sailing over submerged cones. Since then, however, the soundings of telegraph ships have correctly mapped out no less than seven of these peaks between the latitude of Lisbon and the Island of Teneriffe. The depths on the summits of these vary from 12 to 500 fathoms. On one of them, at 400 fathoms, two species of coral (*Lophohelia prolifera* and *Amphihelia oculata*) were growing luxuriantly. Throughout the ocean basins about 300 such submarine cones, rising from great depths up to within depths of from 500 to 10 fathoms from the surface, are already known, or indicated by soundings.

All the physical agencies at work above the lower limit of wave action tend to wear away and level down these cones, and thus to form banks. Graham's Island, thrown up in the Mediterranean in 1831, was 200 feet in height and three miles in circumference, and was washed away in a year or two. The bank left on the spot, at first very shallow, has now 24 feet of water over it. Instances similar to this historical example must often have happened in the great ocean basins. Again, the same agencies produce wide banks around volcanic islands by washing away and spreading out the materials of the softer rocks. Such banks, with depths of less than 60 fathoms, are found extending many miles seawards around some volcanic islands.

On the other hand, all the deeply submerged summits are continually being built up to the lower limit of wave action by the accumulation of the remains of animals which live on them and by the fall of shells upon them from the surface waters. In the Solomon Islands, Dr. Brougham Guppy has shown that there are upraised coral islands with central volcanic cones covered with thick layers of marine deposits; Christmas Island, in the Indian Ocean, is another instance, and similar deposits must now be forming over hundreds of submerged mountains. In this way are foundations prepared for the true reef-building species, which only flourish in the shallower depths.

The bulk of the water of the ocean has a very low temperature; it is ice-cold at the bottom, even under the equator, but on the surface within the tropics there is a relatively thin film of warm water, with a temperature of from 70° to 84° F. This film of warm water is much deeper towards the western parts of the Atlantic and Pacific than it is in the eastern, the reason for this being that the trade winds, which blow continually from the east, carry all the warm surface water to the westward, and draw up cold water from beneath along the western shores of Africa and America to supply the place of that driven westward at the surface. Consequently, there is, at times, a very low temperature, and a great annual range of temperature, along these western shores. This is more clearly shown by the temperatures at 50 and 100 fathoms than by those at the surface. There are no coral reefs along the western shores of Africa and South America, a circumstance evidently connected with the low temperature, wide range, and, more directly, with the food supply, consequent on these conditions. It appears to be a confirmation of this view that, on the eastern shores of Africa, about Cape Guardafui, from off which the south-west monsoon blows for several months in the year, cold water is also drawn to the surface, and there, likewise, are no coral reefs, though they flourish to the north and south of this region.

Coral reefs flourish in mid-ocean and along the eastern shores of the continents, or wherever the coasts are bathed by the warmest and purest currents of water coming directly from the open sea. If we except Bermuda and one or two other outlying reefs, where the temperature may occasionally fall to 66° or 64° F., it may be said that reefs are never found where the surface temperature of the water, at any time of the year, sinks below 70° F., and where the annual range is greater than 12° F. In typical coral reef regions, however, the temperature is higher and the range much less.

The food supply of the coral reef is derived from pelagic oceanic organisms, which exist in the greatest variety and abundance in the surface and sub-surface waters of the ocean. These consist of myriads of Algæ, Rhizopods, Infusorians, Medusæ, Annelids, Molluscs, Crustaceans, Ascidians, and fishes. A very large number of these creatures, within the tropics, secrete carbonate of lime from the ocean to form their shells and skeletons, which, falling to the bottom after death, form the vast oceanic deposits known as Pteropod and Globigerina oozes. In falling to the bottom, they carry down some of the organic matter that composed their living bodies, and thus are the animals which live on the floor of the ocean chiefly supplied with food. Here it may be remarked, incidentally, that the abundance of life at depths of even over two miles is very great. Our small dredges sometimes bring up over sixty species and hundreds of specimens in one haul—of invertebrates and fishes, exclusive of the Protozoa. The pelagic organisms above mentioned oscillate from the surface down to about 80 or 100 fathoms, probably that stratum of the ocean affected by sunlight, and they apparently descend further in regions where the stratum of warm water has a greater depth. Many of the forms rise to the surface in the evening and during calms, and sink again in sunlight and during stormy weather. It is in the evening and when it is calm that this swarming life is most vividly forced on the attention by gorgeous phosphorescent displays. The lime-secreting organisms, like Coccospheres and Rhabdospheres, Foraminifera, Pteropods, and other Molluscs, are much more abundant, both in species and individuals, in the warmest and saltest waters than elsewhere. I have estimated, from tow-net experiments, that at least 16 tons of carbonate of lime, in the form of these shells, exist in a mass of the ocean, in coral-reef regions, one mile square by 100 fathoms in depth. If we take this estimate, which I consider much below the reality, and suppose one-sixteenth of these organisms to die and fall to the bottom each day, then they would take between 400 and 500 years to form a deposit one inch in thickness. I give this calculation more to indicate a method than to give even the roughest approximation to a rate of accumulation of deposits. The experiments were too few to warrant any definite deductions.

The great oceanic currents, moving westward at the rate of several miles an hour, bear these shoals of pelagic organisms on to the face of the reef, where millions of greedy mouths are ready and eager to receive them. The corals and other organisms situated on the outer and windward side of the reef receive the first and best supply; they are thus endowed with a greater amount of energy, and grow faster and more luxuriantly there than on other portions of the reef. The depth at which there is the most constant supply of this food is several fathoms beneath the surface, and there, too, the corals are found in most vigorous growth. It is only a relatively small quantity of this pelagic food that enters the lagoon, the corals that there struggle on in patches being largely supplied with the means of existence from the larvæ of reef-building animals.

So many observations were made during the *Challenger* Expedition on the pelagic fauna inside and outside reefs that

there is little, if any, doubt in my mind that the food supply is a most important factor in relation to the growth of corals in the different portions of a reef. Actual observations were made on the feeding of corals at a good many places, as well as numerous observations on the stomach contents. These observations have been confirmed by Alexander Agassiz.

It is as yet impossible to state in what form the lime, which is secreted as carbonate in such enormous quantities by marine organisms exists in the ocean.

Dana, in "Coral and Coral Islands," considers it "unnecessary to inquire whether the lime in sea-water exists as carbonate or sulphate, or whether chloride of calcium takes the place of these. The powers of life may take from the element present whatever results the function of the animal requires."

In connection with this question an interesting series of experiments are being conducted at the Scottish Marine Station, Granton, which go far to prove that the above hypothesis is correct.

The following table shows the average composition of sea-water salts, the acids and bases being combined in the way usually adopted by chemists—

Average Composition of Sea-Salt.

Chloride of sodium	77·758
Chloride of magnesium	10·878
Sulphate of magnesium	4·737
Sulphate of lime	3·600
Sulphate of potash	2·465
Bromide of magnesium	0·217
Carbonate of lime	0·345
	100·000

In the actual ocean water there are probably traces of every known element, and it is impossible to say what is the precise amount of the respective chlorides, sulphates, and carbonates present. Theoretically, every base may be combined with every acid, and the whole solution must be in a continual state of flux as to its internal composition. While the quantity of sea-salts in a given volume of water varies with position, yet it has been shown by hundreds of analyses that the actual ratio of acids and bases—that is, the ratio of the constituents of sea-salts—is constant in waters from all regions and depths, with one very significant exception—that of lime—which is present in slightly greater proportion in deep water.

The total amount of *calcium* in a cubic mile of sea-water is estimated at nearly 2,000,000 tons. The amount of the same element present in a cubic mile of river-water is nearly 150,000 tons. At the rate at which rivers carry down water from the land it is estimated that it would take 680,000 years to pour into the ocean an amount of calcium equal to that now held by the ocean in solution.

The amount of calcium existing in the 40,000,000 square miles of the typical calcareous deposits of the ocean exceeds, however, that at present held in solution if we merely take them to have an average thickness of 30 feet, and from this calculation we might say that, if the secretion and solution of lime in the other regions of the ocean be exactly balanced, and the calcium in the ocean remain always constant, those calcareous deposits of the thickness indicated would require between 600,000 and 700,000 years to accumulate. There is good evidence, however, that the rate of accumulation is much more rapid in some positions.

The lime thus carried down to the sea is originally derived from the decomposition of anhydrous minerals, and comes from the land in the form of carbonate, phosphate, and sulphate of lime—the carbonate being in the greatest abundance in river-water. On the other hand, the sulphate of lime very greatly predominates in sea-water, the carbonates being present in small quantity. We are not in a position to say whether or not the coral Polyps take the whole of the material for their skeletons from the carbonates, as is generally believed, or indeed to say what changes take place during the progress of secretion by organisms.

In the greatest depths of the Pacific coral seas there is striking evidence of the solvent power of ocean water. Our dredges bring up from a depth of three or four miles over a hundred ear-bones of whales and remnants of the dense Ziphioid beaks, but all the larger and more areolar bones of these immense animals have been almost entirely removed by solution. In a single haul there may also be many hundreds of sharks' teeth, some of them larger than the fossil *Carcharodon* teeth, but all that remains of them is the hard dentine. None of the numerous calcareous surface shells reach the bottom, although they are quite as abundant over the red clay areas as over those shallower areas, where they form Globigerina and Pteropod deposits. In consequence of the small amount of detrital material reaching these abysmal areas distant from continents, cosmic metallic spherules, manganese nodules, highly altered volcanic fragments, and zeolitic minerals, are there found in great numbers. Almost all these things are found occasionally in the other regions of the ocean's bed, but their presence is generally masked by the accumulation of other matters. In some regions Radiolarian and Diatom remains are found in the greatest depths, and they too are subject to the solvent power of sea-water, but to a much less extent than carbonate of lime shells.

As we ascend to shallower waters, a few fragments of the thicker-shelled specimens are met with at first; with lesser depths the carbonate of lime shells increase in number, until in the shallower deposits the remains of Pteropods, Heteropods, and the most delicate larval shells are present in the deposit at the bottom. This gradation in the appearance of the shells can be well seen in a series of soundings at different depths around a volcanic cone, such as has been described as forming the base of a coral atoll. There is no known way of accounting for this vertical distribution of these dead shells except by admitting that they have been dissolved away in sinking through the deeper strata of water, or shortly after reaching the bottom; indeed, an examination of the shells themselves almost shows the process in operation. It is rare to find any trace of fish-bones in deposits other than the otoliths.

These considerations, as well as numerous experiments in the laboratory, show that everywhere in the ocean dead or amorphous carbonate of lime structures quickly disappear wherever they are exposed to the action of sea-water, and in investigating the evolution of the general features of coral reefs it is as necessary to take cognizance of this fact as of the secretion of carbonate of lime by organisms. At the same time, too much stress cannot be laid upon the fact that carbonate of lime, although markedly soluble in sea-water in the amorphous form in which it exists in connection with (organic) life, becomes practically insoluble when after the death of the secreting animal it assumes the crystalline state.

In a paper read before the Royal Society of Edinburgh, embodying some of the results of his investigations on the solubility of carbonate of lime under different forms in sea-water, Mr. Irvine remarks, "It is due to this molecular change that coral deposits, shells, and calcareous plants are able to accumulate in the ocean, ultimately to form beds of limestone rocks."

The first stage, then, in the history of a coral island is the preparation of a suitable foundation on the submerged volcanic cones, or along the shores of a volcanic island, or the borders of a continent. In the case of the atoll the cone may have been reduced below the level of the sea by the waves and atmospheric influences, or built up to the lower limit of breaker action by the vast accumulation of organisms on its summit.

A time comes, however, should the peak be situated in a region where the temperature is sufficiently high, and the surface currents contain a suitable quality of food, that the reef-builders fix themselves on the bank. The massive structure which they secrete from ocean water enables them to build up and maintain their position in the very face of ocean currents, of breakers, of the overwhelming and outrageous sea.[1]

"Coral" with the sailor or marine surveyor is usually any carbonate of lime shell or skeleton or their broken-down parts. "Coral" is used by the naturalist in a much more restricted sense: he limits the term to animals classed as Madrepores, Hydrocorallines, and Alcyonarians. The animals belonging to the first two of these orders comprise those included under the vague term of reef corals. Besides these, however, very many other classes of animals contribute to the building up of coral

[1] Dr. Brougham Guppy says, "History can afford us no clue to the first appearance or the age of reefs; yet in the myths of the Pacific Islanders we find that the savage inhabitants of these regions regard the history of a coral atoll as commencing with the submerged shoal, which through the agency of God-like heroes is brought up by their fish-hooks to the surface." —Paper, Vict. Inst.

reefs and islands—such are Foraminifera, Sponges, Polyzoa, Annelids, Echinoderms, and Calcareous Algæ. The relative proportions of these different organisms in a reef vary with the region, with the depth, and with the temperature, but members of what are known under the term of reef corals appear always to predominate.

The animals of the true reef-building species resemble the common sea-anemones in structure and size; the individual Polyps may vary from the eighth of an inch in diameter to over a foot. Some of the structures built by colonies may exceed 20 feet in diameter.

There may be great variety in the appearance of submerged reefs as they rise from banks of a different nature, form, and extent, as, indeed, was pointed out long ago by Chamisso. There may be differences due also to the kinds and abundance of deep-sea animals living on such banks, as well as differences due to currents, temperature, and other meteorological conditions.

From the very first the plantations situated on the outer edge will have the advantage, from the more abundant supply of food and the absence of sand in the water, which last more or less injuriously affects those placed towards the interior. Chamisso attributed the existence of the lagoon to the more vigorous growth of the peripherally situated corals of a reef, as compared with those placed towards the middle, and in this he was to a large extent right, but the symmetrical form of the completed atoll is chiefly due to the solution of the dead carbonate of lime structures. The Great Chagos Bank illustrates the irregular way in which such a large bank of coral plantations approaches the surface. When these, however, reach the surface, they assume slowly a more regular outline, those on the outer edge coalesce, and ultimately form a complete ring of coral reef, and the lagoon becomes gradually cleared of its coral patches or islands, for, as the atoll becomes more perfect, the conditions of life within the lagoon become less and less favourable, and a larger quantity of dead coral is removed in solution.

The coral atoll varies greatly in size and form: it is usually more or less circular, horse-shoe shaped, and may be one or over fifty miles in diameter. The breakers spend their fury on the outer edge, and produce what is known as the broad shore platform; but within, trees descend to the very shore of the lagoon, where there is quiet water, and a ship may often enter on the lee side of the atoll and find safe anchorage.

In this connection it is important to bear in mind the relation which exists between the periphery and the superficial area of the lagoon in atolls of different sizes. If the coral plantations which rise from the top of a submerged mountain have an area of one square mile, then on reaching the surface of the waves there will be a shallow depression in the centre owing to the more rapid growth of the outer edge. Such an atoll will have, if it be a square, four miles of outer reef for the supply of coral sand and other *débris*, and these being washed and blown into the one square mile of shallow lagoon it is likely to become filled up, the result being a small island with dry lagoon, in which may be found deposits of sulphate of lime, magnesian and phosphatic rocks, and guano—all these testifying to the great age of the island and absence of subsidence in the region. It is only atolls with a diameter of less than two miles that thus become filled up. In other and larger plantations, rising from a more extensive bank, the conditions are very different. In this larger atoll—say four miles square—there is now only one mile of outer reef to each square mile of lagoon, instead of four miles of outer reef to the one square mile of lagoon in the smaller atoll. Only one-fourth of the detrital matter and food enters the larger lagoon, from the outside, per square mile of lagoon, and hence there is proportionately less living coral, the solvent agencies predominate, and the lagoon is widened and deepened. Growing seawards on the outer face and dissolving away in the lagoon, the whole expands after the manner of a fairy ring, and the ribbon of reef or land can never in consequence increase beyond a half or three-quarters of a mile in width, it being usually much less. I have recently made a very careful comparison of the latest Admiralty Survey of the lagoon of Diego Garcia with the one made many years ago, and the result appears to me to indicate that the area of the lagoon has considerably increased in the interval, and the average depth is a little greater than formerly, although shallower in some places.

Atolls may occur far away from any other land, but it more frequently happens that they are arranged in linear groups, in this respect resembling volcanic islands. Extensive banks may be crowded with small atolls, like the Northern Maldives; or a bank may be occupied by one great and perfect atoll twenty to forty miles in diameter, like some of the Southern Maldives and the Paumotus. In some instances the large atolls appear to have resulted from the growth and coalescence of the smaller marginal atolls; especially does this seem to have been the case with the large Southern Maldives.

The outer slopes vary greatly in different reefs, and in different parts of the same reef. When there is deep water beyond, the reef very often extends out with a gentle slope to a depth of 25 to 40 fathoms, and is studded with living coral, the bosses and knobs becoming larger in the deeper water farthest from the reef, where there are great overhanging cliffs, which eventually fall away by their own weight, and form a talus on which the reef may proceed further outwards. Occasionally there is a very steep descent almost at once from the outer edge. Thus, the deeper the water beyond, the more slowly will the reef extend seawards. In reefs with a very gentle slope outside, the corals are frequently overhanging at depths of 6 or 7 fathoms, for in these instances the lower part of the sea-face of the reef is rendered unsuitable for vigorous growth, in consequence of the sand which is carried in by waves coming over the comparatively shallow depths outside; in these cases, lines of growing corals, or a submerged barrier, are sometimes met with in deep water some distance seawards from the edge of the reef.

As has been stated, the lagoon in many of the smallest atolls has been filled up, but this never appears to happen in atolls with a diameter of over two miles unless there be distinct evidence of upheaval. In perfectly-formed atolls—that is, those in which the reefs are nearly continuous throughout—the deepest water is found towards the centre of the lagoon, and there is a relation between this depth and the depth of water beyond the outside reefs. In North and South Minerva reefs, in the South Pacific, where the outside depths are very great, there are depths down to 17 fathoms in the lagoons, which are apparently clear of coral heads. Here we may suppose that the central parts of the lagoon have for a long time been exposed to the solvent action of sea-water, owing to the slow lateral growth of the reef as a whole. In the same regions the Elizabeth and Middleton reefs, which are about the same size, have only 4 or 5 fathoms within the lagoons, and the depths outside the reefs are, at the distance of a mile, mostly within the 100-fathom line, and sometimes less than 50 fathoms. There are also many coral heads within the lagoons. Here we may suppose the atolls to be more recent, and to have extended more rapidly than in the case of the Minerva reefs. If the depths beyond the reefs be taken into consideration, then there is usually a direct relation between the depth of the lagoon and its diameter. The greatest depths, even in the largest atolls, do not exceed 50, or at most 60, fathoms; they are usually much less. In atolls which are deeply submerged, or have not yet reached the surface, which have wide and deep openings into lagoon-like spaces, this relation may not exist. In these instances the secretion and deposition of carbonate of lime may be in excess of solution in all parts of the lagoon. It is only when the atoll reaches the surface, becomes more perfect, and its lagoon waters consequently less favourable to growth, that the solution of the dead corals and calcareous *débris* exceeds any secretion and deposition that may take place throughout the whole extent of the lagoon; it is then widened and deepened, and formed into a more or less perfect cup-like depression, unless the lagoon be of small size and is filled up.

The whole of a coral reef is permeated with sea-water like a sponge; as this water is but slowly changed in the interior parts, it becomes saturated, and a deposition of crystalline carbonate of lime frequently takes place in the interstices of the corals and coral *débris*. In consequence of the solution of coral *débris* and the re-deposited lime occupying less space, large cavities are formed, and this process often results in local depressions in some islands, as, for instance, in Bermuda. At many points on a reef where evaporation takes place there is a deposition of amorphous carbonate of lime cementing the whole reef materials into a compact conglomerate-like rock.

The fragments of the various organisms broken off from the outer edge during gales or storms are piled up on the upper surface of the reef, and eventually ground into sand, the result being the formation of a sandy cay or shoal at some distance back from the outer edge of the reef—the first stage in the formation of dry land.

The fragments of pumice thrown up into the ocean during far-distant submarine eruptions, or washed down from volcanic

lands, are at all times to be found floating about on the surface of the sea, and these, being cast up on the newly-formed islet, produce, by their disintegration, the clayey materials for the formation of a soil—the red earth of coral islands. Just within the shore platform these pumice fragments are found in a fresh condition, but as the lagoon is approached they disappear, the soil becomes deeper, and the most luxuriant vegetation and largest trees are found close to the edge of the inner waters. The land is seldom continuous around the atoll; it occurs usually in patches. The water passes over the shallow spaces between the islets and through the deeper lagoon entrances, these last being kept open by the strong sand-bearing currents which pass at each tide.

The few species of plants and animals which inhabit these coral islands have been drifted to the new island like the pumice, or carried, many of them maybe, by birds; lastly, savage and civilized man finds there a home.

There is no essential difference between the reefs forming fringing and barrier reefs, and those which are known as atolls. In the former case, the corals have commenced to grow close to the shore, and as they grow outward, a small boat-passage, and then a ship-channel, is carved out between the reef and the shore by tidal scour and the solvent action of the water on the dead parts of the reef: thus, the fringing reef may be converted into a barrier reef; or the barrier may be formed directly by the upward growth of the corals at some distance from the shore. In some instances the corals find a suitable foundation on the banks that surround islands and front continental lands, it may be, at a great distance from the coast, and when they reach the surface they form a distant barrier, which proceeds seawards, ultimately on a talus made up of materials torn from its seaward face.

If the foregoing considerations be just and tenable, then it would appear that all the characteristic features of coral reefs can be produced, alike in stationary areas or in areas of slow elevation and subsidence, by processes continually at work in the ocean at the present time. Slow elevation or subsidence would only modify in a minor way a typical coral atoll or barrier reef, but subsidence in past times cannot be regarded as the cause of the leading characteristics of coral reefs. There are abundant evidences of elevation in coral-reef regions in recent times, but no direct evidence of subsidence. If it has been shown that atoll and barrier reefs can be formed without subsidence, then it is most unlikely that their presence in any way indicates regions of the earth's surface where there have been wide, general, and slow depressions.

According to Mr. Darwin's theory, which has been almost universally accepted during the past half-century, the corals commence to grow close to the shore of an island or continent: as the land slowly sinks, the corals meanwhile grow upwards to the surface of the sea, and a water space—the lagoon channel—is formed between the shore of the island and the encircling reef, the fringing being thus converted into a barrier reef. Eventually, the central island sinks altogether from sight, and the barrier reef is converted into an atoll, the lagoon marking the place where the volcanic or other land once existed. Encircling reefs and atolls are represented as becoming smaller and smaller as the sinking goes on, and the final stage of the atoll is a small coral islet, less than two miles in diameter, with the lagoon filled up and covered with deposits of sea-salts and guano.

It is at once evident that the views now advocated are in almost all respects the reverse of those demanded by Mr. Darwin's theory.

The recent deep-sea investigations do not appear in any way to support the view that large or small islands once filled the spaces now occupied by the lagoon waters, and that the reefs show approximately the position of the shores of a subsided island. The structure of the upraised coral islands, so far as yet examined, appears to lend no support to the Darwinian theory of formation. When we remember that the great growing surface of existing reefs is the seaward face from the sea surface down to 20 or 40 fathoms, that large quantities of coral *débris* must be annually removed from lagoons in suspension and solution, that reefs expand laterally and remain always but a few hundred yards in width, that the lagoons of finished atolls are deepest in the centre, and are relatively shallow compared with the depth of the outer reefs, then it seems impossible, with our present knowledge, to admit that atolls or barrier reefs have ever been developed after the manner indicated by Mr. Darwin' simple and beautiful theory of coral reefs.

DARWIN VERSUS LAMARCK.[1]

AFTER a brief sketch of the life of Lamarck (1744–1829), his theory was stated in his own words as follows:—

"(1) In every animal which has not arrived at maturity, the increased and continued employment of any organ strengthens that organ gradually, develops it, enlarges it, and gives it a power proportional to the duration of its employment: on the other hand, the continued disuse of any organ gradually weakens it, deteriorates it, progressively diminishes its faculties, and finally causes it to disappear.

"(2) Every feature which, under natural conditions, individuals have gained or lost by the action of circumstances to which their race has been for some time exposed—as, for instance, the results of excessive use or disuse of an organ—is preserved in reproduction and transmitted to the offspring, provided that the acquired changes were present in both parents."

The small changes thus produced and transmitted from generation to generation are increased in successive generations by the action of the same causes which originated them, and thus in long periods of time the form and structure of the descendants of an ancestral organism may be completely changed as compared with the form and structure of the ancestor.

Given sufficient time, these small changes can have produced man and the higher animals from simple primitive protoplasmic animalcules.

Prof. Lankester then pointed out the truth of the first law of Lamarck, but mentioned the preliminary objections to Lamarck's theory, which had prevented its acceptance by the naturalists of the first half of this century. He then briefly epitomized Darwin's theory as follows:—

(1) All plants and animals produce offspring which resemble their parents on the whole (heredity); these offspring, however, exhibit also new and individual features differing from those of their parents (congenital variations).

(2) In Nature there is a severe struggle for existence. Only one pair out of the many thousands often produced by a pair of plants or animals survive to maturity, and in their turn produce offspring.

(3) The survivors are those whose congenital variations have enabled them to gain advantage over their fellows.

(4) The surviving forms *may* be almost exactly like their parents, but often a departure from the parental form must be an advantage, however small. Such departure, or variation, when IN-BORN OR CONGENITAL, not only enables its possessor to survive and produce offspring, but is handed on by heredity to that offspring.

(5) A successful congenital variation is intensified in the new generation bred from parents in both of which it had congenitally appeared.

(6) By this process of natural selection of advantageous congenital variations, operating in countless millions of successive generations, the transformation of simple into more elaborate forms of life has been effected.

The real difference between Lamarck's and Darwin's theories was then explained. Congenital variation is an admitted and demonstrable fact; transmission of congenital variations is also an admitted and demonstrable fact. Change of structure acquired during life—as stated by Lamarck—is also a fact, though very limited. But the transmission of these latter changes to offspring is NOT PROVED EXPERIMENTALLY; all experiment tends to prove that they cannot be transmitted. Semper's book on this subject was cited as a failure in the attempt to prove such transmission.

The causes of congenital variations were next discussed, and the "stirring up" of the germ-plasma by the process of fertilization was pointed to as the chief.

Very minute congenital variations can be useful, and, therefore, selected; but congenital variations are not necessarily minute.

The subject of correlated variations was next mentioned, and their great importance pointed out. A mechanical model was used to explain this matter: it represented an antelope in which when the neck is made to elongate the legs simultaneously lengthen, whilst the horns disappear and the tail shortens.

The lecturer then gave examples of the successful explanation

[1] Abstract of a Lecture delivered at the London Institution, Finsbury Circus, on February 14, 1889, by Prof. Ray Lankester, LL.D, F.R.S.

of cases by Darwinism where Lamarckism fails. Mimicry and protective colouring, adaptation of flowers to insect visitors, instincts of neuter insects, and Lamarck's chosen case, the giraffe, were among these.

It was then pointed out that breeders have never produced new varieties by transmission of acquired characters (Lamarckian), but always by transmission of congenital characters (Darwinian).

Whilst all this tends to the complete rejection of Lamarck's theory, it is true that Darwin himself admitted Lamarckism as an explanation of some rudimentary organs (disuse), and of some instincts (transmission of acquired habit).

On the other hand, neo-Darwinians reject Lamarckism altogether, because (1) the fundamental fact of transmission of a change of structure or habit acquired during the life of an individual by the action of external agencies is not only not proved but is contrary to experience; (2) such transmission is highly improbable in view of the structure and origin of the reproductive germs; (3) even if admitted as possible, Lamarckism is not needed in order to explain the facts of the structure and habits of existing plants and animals, in addition to Darwinism. Pure Darwinism is sufficient.

Finally, the lecturer dealt with some cases advanced by Lamarckians as favourable to their views, and gave their Darwinian explanation.

Among these were rudimentary organs, where the fully-developed organ would not be injurious, *e.g.* the intrinsic muscles of the human ear. These were explained by panmixia and parsimony of growth.

Blind animals in caves and in the deep sea, *e.g.* blind cray-fish, Thaumastocheles, and blind fishes, were shown to be best explained by the natural selection of congenital blindness. Amongst a whole brood of animals swept by a flood into a cavern, or by a current into deep water, those with perfect eyes would escape by following the light, whilst those with congenitally defective eyes would remain and reproduce their defect in their offspring, and in each succeeding generation the same process of natural selection would be continued.

Wingless insects and birds were similarly explained.

Instincts, *e.g.* "shamming dead," nest-building, choice of food, were briefly considered, and shown to be explicable by Darwinism and not by Lamarckism.

In fact, it was declared that, in proportion as our knowledge of any class of such facts is extensive and thorough, the Darwinian explanation is found to be correct and the Lamarckian inadequate and inapplicable.

A consideration of the mental evolution of man, according to neo-Darwinism was promised as the subject of a future lecture. It was briefly stated that the results of education and circumstances, good or bad, cannot be transmitted, whilst hereditary qualities, good or bad, cannot be eliminated, except by selection in breeding.

The transmission of acquired experience does not take place by heredity, but (among civilized societies) by the agency of tradition and books.

In civilized societies the injurious effects of unlimited neglect of selective breeding is largely neutralized by panmixia, giving an average race, neither wholly good nor wholly bad.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Botanic Garden Syndicate, unlike some others at Cambridge, have been able to erect their new plant-houses within one pound of the estimate, £3000. The work has been satisfactorily done by Messrs. Boyd, of Paisley. Solid foundations have been laid, so that when required new wood-work may be built on the same walls. The new houses include a warm orchid-house, warm fern-house, stove, palm-house, aquarium, and stove-pit. A laboratory, for investigations required to be conducted near the plant-houses, has been built; it contains two large working-rooms and a dark chamber. The collections removed to the new houses are now in capital condition. The hardy cactuses, probably unsurpassed, have been removed to the border in front of the new stove. A new bed has been made for the choicer hardy Ericaceæ. Great progress has been made in naming and labelling. Among plants of scientific interest that have flowered in the gardens is *Pilocarpus pennatifolius*, which yields pilocarpine, *Erythroxylon Coca, Narcissus Broussoneti* (the corona a mere rim), and many others. Among the most interesting plants received have been *Gerbera Jamesoni*, a fine Composite from the Cape, *Isonandra Gutta* (yielding gutta-percha), *Washingtonia robusta*, a choice new palm, *Stachys tuberifera*, a new vegetable (the crosnes of the Paris markets), and numerous hardy bamboos.

The regulations altering the arrangement of papers in the Natural Sciences Tripos have been confirmed, making the papers special ones in subjects, instead of general ones covering all the subjects.

The following have been appointed members of the Boards of Electors to Professorships named: Moral Philosophy, Principal Caird; Chemistry, Prof. A. W. Williamson; Botany, Sir Joseph Hooker; Geology, Prof. A. H. Green; Jacksonian of Natural Philosophy, Prof. A. W. Williamson; Mineralogy, Prof. H. N. Story-Maskelyne; Political Economy, Right Hon. L. H. Courtney; Zoology, W. H. Flower, C.B.; Experimental Physics, Sir W. R. Grove; Mechanism and Applied Mechanics, Sir F. J. Bramwell, F.R.S.; Physiology, Prof. Humphry; Logic, Prof. Bain.

Mr. A. E. Shipley has been approved as a teacher of Comparative Anatomy for the purpose of medical study.

SCIENTIFIC SERIALS.

The American Journal of Science, February.—Points in the geological history of the Islands Maui and Oahu, Hawaii, by James D. Dana. The subjects illustrated by the present state of these islands are: the conditions of extinct volcanoes in different stages of degradation; the origin of long lines of precipice cutting deeply through the mountains; the extent and condition of one of the largest of craters at the period of extinction, and the relation of cinder and tufa cones to the parent volcano. The accompanying plates, reduced from the recent large Government maps, show the present general features of both islands. Incidental reference is made to the late controversy on Darwin's theory of coral islands, the author declaring emphatically that no facts have hitherto been published by Mr. Murray or Mr. Guppy that prove the theory false, or set aside the arguments in its favour. Some of the facts are more in favour than opposed to it, while none do more than offer a possible alternative.—An experiment bearing upon the question of the direction and velocity of the electric current, by Edward L. Nichols and William S. Franklin. The authors, who had already independently developed a method similar to that lately described by Foeppl (*Annalen der Physik und Chemie*), here repeat his experiment with an apparatus capable of indicating the direction and velocity of the current, supposing it to have direction, even though that velocity were very great indeed. They show that they would have been able to detect a change of deflection due to the motion of the coil, even though the velocity of the current had been considerably in excess of one thousand million metres per second.—On the occurrence of monazite as an accessory element in rocks, by Orville A. Derby. The researches of Mr. John Gordon and Prof. Gorceix have placed beyond doubt the wide distribution of monazite in the sea and river sands of Brazil, but under circumstances that give no clue to its origin. The petrographic analyses here described have resulted in the discovery that gneiss, granite, and syenite yield, besides zircon, a certain quantity of microscopic crystals of a heavy yellow mineral apparently identical with the Bahia monazite. Recently, also, Mr. Gordon has obtained residues of zircon and monazite from the river sands at Buenos Ayres, and from gneiss and granite decomposed *in situ* at Cordoba in the Argentine Republic.—On the use of steam in spectrum analysis, by John Trowbridge and W. C. Sabine. These experiments show that a remarkable degree of economy in time and in waste of apparatus results from the use of a jet of steam in spectrum analysis, when the spark method of obtaining the spectra of metals is employed.—A comparison of the electric theory of light and Sir William Thomson's theory of a quasi-labile ether, by J. Willard Gibbs. A comparison is here instituted between the electric theory of light and the new theory of an elastic ether expounded by Sir William Thomson in the *Philosophical Magazine* for November 1888. The result of this inquiry seems to be that both theories in their extreme cases give identical results. The greater or less degree of elegance, or completeness, or perspicuity, with which these laws may be developed by different physicists should weigh nothing in favour of either

theory. The elastic theory, however, is regarded as somewhat less convenient as a working hypothesis than the electric.—In this number appears Part 1 of an exhaustive monograph, with numerous illustrations, on the geology of the volcanic island of Fernando de Noronha, South-West Atlantic, by John C. Branner.

The last volume (xviii.) of the *Memoirs of the Kazan Society of Naturalists* contains an elaborate inquiry into the distribution of solanin (an alkaloid discovered by Desfosses in many *Solanaceæ*) in plants, by E. Wotschall; short reports on geological exploration in the Governments of Vyatka and Ufa, by A. Netschaeff and A. Lavrsky; and a description of the flora of the neighbourhood of Ufa, by A. Gordyaghin.

SOCIETIES AND ACADEMIES.

London.

Royal Society, February 7.—"The Principles of Training Rivers through Tidal Estuaries, as illustrated by Investigations into the Methods of improving the Navigation Channels of the Estuary of the Seine." By Leveson Francis Vernon-Harcourt, M.A., M.Inst.C.E.

After stating the principles upon which the training of the non-tidal portions of rivers are carried out, the undefined and unsatisfactory condition of the principles followed in training rivers through wide tidal estuaries, and the discordant views of engineers on the subject, were pointed out. The absence of definite principles, and the divergence of opinion amongst engineers, have received a remarkable illustration in the great variety of schemes proposed for extending the training walls in the Seine estuary beyond Berville, where the works were stopped, in 1870, owing to the unexpected changes the works had already produced. It occurred to the author in August 1886, that if it should be possible to reproduce, in a working model, the original state of the Seine estuary before the training works were commenced, and next the present state of the Seine, as modified by these works, could be obtained, then the successive introduction in the model of the several schemes, proposed for the extension of the training walls, might furnish results indicating approximately in miniature the changes which the works would actually produce if carried out in the estuary, and also afford a basis for the establishment of general principles for training rivers through wide estuaries. A model, accordingly, was made of the tidal Seine, to the scale of 1/40,000 horizontal, and 1/400 vertical; the bed was formed of fine sand, so that it could be moulded by the current; the fresh-water discharge was produced, at the upper end, by the flow of water from a small cistern; and the tidal ebb and flow were effected by the tipping of a tray, placed at a suitable angle at the lower end, representing the open sea. The model was first worked in November 1886, and the experiments were continued at intervals up to 1889. Silver sand was first used for forming the bed of the miniature estuary; and some of the phenomena of the actual estuary, such as the *bore*, the "*verhaulc*" or reverse current, and the shifting channels, were reproduced in the model; but when the training walls were introduced into the model, on the lines of the existing training walls, it was found that the silver sand could not be adequately carried in suspension by the small currents in the model to reproduce the accretion which has occurred in the estuary behind and beyond the training walls. A variety of fine powders, of low specific gravity, were consequently experimented on in the model, but they mostly proved too sticky, or pasty, or otherwise unsuitable. At last a fine sand from Chobham Common, belonging to the Bagshot beds, containing an admixture of peat, offered better results, and was employed for the subsequent experiments.

After working the model for some time with a bed formed of this Bagshot sand, the channels assumed a form very closely resembling in general outline the chart of the Seine of 1834. This result, by reproducing a former condition of the estuary, confirms the previous results obtained by Prof. Osborne Reynolds with a model of the Upper Mersey estuary, showing that it is quite practicable to reproduce in a model the main tidal channels in an estuary.

The second stage of the investigation involved the quite novel condition of introducing training walls in a model, and producing the resulting accretion. This most essential stage was the subject of a long series of experiments, but was at last satisfactorily accomplished with Bagshot sand. The existing training walls were inserted in the model, and the resulting deepening of the trained channel and the accretion outside and beyond were reproduced in the model, and also the shifting channel between the termination of the training walls and the sea.

The third stage of the investigation was then entered upon, consisting in the successive introduction in the model of the lines of the five principal schemes at present advocated, observing the changes they respectively produced in the model of the estuary, and recording them in the form of charts of the estuary, which are appended as plates to the paper. A final experiment was also made with an arrangement of training walls forming as gradually expanding a channel as practicable, without restricting the width of the outlet. The lines designed for the extension of the training walls in each scheme are briefly described in the paper, as well as indicated in the charts, and also the channels and accretion which they each produced.

The probability of the results obtained really representing in miniature the results which corresponding works in the estuary of the Seine would actually produce was then considered; for if the effects of any training works could be foreshadowed by experiments in a model, the value of such experiments, in guiding engineers towards the selection of the most suitable design, could not be over-estimated. Though the effects of winds and waves, and the actual rate of accretion, cannot be reproduced in a model, it is evident, from the first stage of the investigation, that the main forces at work, in the comparative shelter of an estuary, are the tidal ebb and flow and the fresh-water discharge, which are the forces which can be produced in a model. Moreover, the correspondence of the second stage of the investigation with the existing state of the Seine estuary confirms the accordance between the results in the model and the condition of the estuary. The extension of training walls decreases the influence of winds and waves; and therefore the results of the third stage of the investigation are more likely to correspond with the changes which such works would actually produce in the estuary, than those of the first and second stages. Also the results obtained in the model with the two earlier schemes are precisely those which the author predicted would occur, before the experiments were commenced, if the schemes were actually carried out in the estuary.

The paper concludes with a classification of the experiments, with the view of deducing general principles for guidance in training rivers through tidal estuaries. The three classes are, (1) outlet of estuary considerably restricted, and channel trained inside towards outlet; (2) channel trained in sinuous line, expanding towards outlet, but kept somewhat narrow at changes of curvature; (3) channel trained in as direct a course as practicable, and expanding regularly to outlet.

The experiments of the first class exhibited a deep outlet, and a fairly continuous channel inside where the training works were prolonged to the outlet. The channel, however, was irregular in depth near the outlet; and a bar appeared in front of the outlet outside. The breakwater, also, extending across part of the original outlet, occasioned deposits both inside and outside the estuary, by producing slack water in the sheltered recesses.

The second class of trained channel was designed to profit by the well-known scour at the concave face of bends in non-tidal rivers, and to continue the depth thus obtained by restricting the width between the bends. Experiment, however, did not bear out the advantages of this system, probably owing to the variable direction of the flood tide at different heights of tide, its being checked in its progress by the winding course, and not acting in unison with the ebb, from the difference in its direction and the width of the trained channel near the outlet. The third class of trained channel afforded a wide channel, tolerably uniform in depth, in the experiments; the flood tide was less impeded in its progress than with the other forms of training walls, and appeared to act more in concert with the ebb.

The experiments accordingly indicate that the only satisfactory principle for training rivers through wide estuaries with silt-bearing currents is to give the trained channel a gradually expanding form, with as direct a course as practicable to the outlet. The rate of increase in width between the training walls must be determined by the special conditions of the estuary.

February 14.—"On a Series of Salts of a Base containing Chromium and Urea," No. 2. By W. J. Sell and Prof. W. J. Lewis.

The paper is a continuation of that published by one of the authors (Proc. Roy. Soc., vol. xxxiii. p. 267). It is here shown

that the chief product of the reaction of chromyl dichloride on urea is the dichlortetrachlorochromate of a base containing the elements of urea with chromium to which the formula $\left\{ (CON_2H_4)_{12}Cr_2 \right\}\begin{matrix} 4CrO_3Cl \\ Cl_2 \end{matrix}$ is assigned. This compound crystallizes from hydrochloric acid in brown-yellow crusts, which are immediately decomposed by water with formation of the dichlordichromate and hydrochloric acid. Among a large number of other new salts described, of which the normal bromide, $(CON_2H_4)_{12}Cr_2Br_6 6H_2O$, and iodide, $(CON_2H_4)_{12}Cr_2I_6$, may be taken as typical, the base also forms a perbromide, $(CON_2H_4)_{12}Cr_2Br_6 6Br_2$, and a periodide of similar composition, behaving in this respect like the organic bases. These substances, as indeed all the salts hitherto obtained, crystallize with great facility, and are as a rule sparingly soluble.

Anthropological Institute, February 12.—Dr. John Beddoe, F.R.S., President, in the chair.—Dr. Beddoe read a paper on human remains discovered by General Pitt-Rivers at Woodcuts, Rotherley, and Winkelbury Camp.—Mr. Bernard Hollander read a paper on centres of ideation in the brain. The object of this paper was to furnish the basis of a scientific phrenology. The author took it for granted: (1) that all mind manifestation is dependent on brain matter; (2) that the various elements of the mind have distinct seats in the brain, which, however, have not been as yet determined; (3) that the recent researches by physiological experimenters and pathological investigators, which have resulted in defining distinct regions for motion and sensation, established the physiological correlative of psychological actions. By applying galvanic currents to definite portions of the brain, or by destroying certain areas, physiological experimenters caused movements of certain limbs and muscles. In itself the distribution of motor areas in the brain would be of little value to the psychologist, except that it proves to him the plurality of functions of the brain. When, however, we observe that the movements caused by excitation form the physical parallel of a mental action, we may arrive at the psychological function of a certain portion of brain by reducing the various faculties of the mind to their elements, and watching their physical expression. To arrive at the demonstration of centres of ideation: (1) we must observe the physical expressions of our thoughts and feelings; (2) we must take the limbs and muscles, which are affected by definite emotions, and see on what occasions they are made to move by central excitation. Thus we find that in a definite part of the frontal convolution (Ferrier's centre No. 7) the galvanic current had the effect of elevating the cheeks and angles of the mouth with closure of the eyes. On no other region could the same be effected. Darwin points out ("Expression of the Emotions," p. 202) that under the emotion of joy the mouth is acted on exclusively by the great zygomatic muscles, which serve to draw the corners backwards and upwards. The upper and lower orbicular muscles are at the same time more or less contracted. Duchenne and Sir Chas. Bell are of the same opinion, and Sir Crichton Browne, speaking of the general paralysis of the insane, says that in this malady there is invariably optimism, delusions as to wealth, rank, &c., and insane joyousness, while its very earliest physical symptom is trembling at the corners of the mouth. The effect produced by the galvanic current on Ferrier's centre No. 7 is thus shown to be the physical expression of the emotion of joy. Combe located there his "organ of cheerfulness" which he afterwards called "Hope"; and there is no doubt some relation between the effect of Ferrier's experiment and the result of Combe's observation. Prof. Sigmund Exner says the centres for the facial movements extend from the gyrus centralis anterior to the latter halves of the lower frontal convolutions, an area which corresponds with Gall's "centre for mimicry" (afterwards named "Imitation.") Most marked, however, is the harmony between the results of modern experiments and the observations made by the early phrenologists when we arrive at the demonstration of the "gustatory centre." Ferrier's experiments on the lower extremity of the temporosphenoidal convolution caused movements of the lips, tongue, and cheeks—indications of gustatory sensation. Looking up the *Edinburgh Phrenological Journal* (vol. x. p. 249), we find that many men claimed the discovery (in 1824) of the organ for gustatory sensation, as afterwards called "Gustativeness" or "Alimentiveness," and that they located this centre in exactly the same region. As this organ is difficult to be observed on account of the zygomatic arch and the temporal muscle, phrenology was much abused at the time. Prof. Ferrier's experiments on his centre No. 11, on the lower extremity of the ascending parietal convolution, resulted in retraction of the angle of the mouth. The action is that of the platysma myoides muscle, which, as Sir Chas. Bell ("Anatomy of Expression," p. 168) states, is strongly contracted under the influence of fear, and which he calls the muscle of fright. Phrenologists (Gall and Spurzheim) located in this region their organ of "Cautiousness," which they found largely developed in persons known for their timidity. Prof. Ferrier's centre No. 7 is said to cause "raising of the shoulders with extension of the arms," a movement which Darwin and Mantegazza refer to the expression of patience, submission, and the absence of any intention to resist. Gall's organ of "Veneration," which corresponds with this centre, is said to produce an instinctive feeling of respect, and when defective in children, Combe says, it has the effect of making them regardless of authority, prone to rebellion, and little attentive to command. Though the work, as described, is far from complete, it may have the effect of causing Gall's theories to be re-examined, and of pointing out a sure method for the demonstration of centres of ideation.

Mathematical Society, February 14.—J. J. Walker, F.R.S., President, in the chair.—Mr. H. F. Baker was admitted into the Society.—The following communications were made:—On the diophantine equation $y^2 + \left(\frac{dy}{dx}\right)^2 =$ square, Prof. Cayley, F.R.S.—Sur la transformation des équations algébriques, Signor Brioschi.—On projective cyclic concomitants or surface differential invariants, E. B. Elliott.—On secondary invariants, Prof. L. J. Rogers.—Remarks upon algebraical symmetry, with particular reference to the theory of operations and the theory of distributions, Major Macmahon.

Royal Meteorological Society, February 20.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—Report on the helm wind inquiry, by Mr. W. Marriott. The helm wind is peculiar to the Cross Fell Range of mountains in Cumberland, which runs from north-north-west to south-south-east. This range is high and continuous, and is not cut through by any valley. Cross Fell is 2900 feet above sea-level. From the top of the mountains to the plain on the west there is an abrupt fall of from 1000 to 1500 feet in about a mile and a half. At times when the wind is from some easterly point the helm forms over this district; the chief features of the phenomenon being the following: a heavy bank of cloud rests along the Cross Fell Range—at times reaching some distance down the western slopes, and at others hovering just above the summit; while at a distance of two or three miles from the foot of the Fell a slender roll of dark cloud appears in mid-air and parallel with the helm cloud; this is the helm bar. The space between the helm cloud and the bar is usually quite clear, while to the westward the sky is at times completely covered with cloud. The bar does not appear to extend further west than about the River Eden. A cold wind rushes down the sides of the Fell, and blow, violently till it reaches a spot nearly underneath the helm bars when it suddenly ceases. The observations that have been made in the district during the past three or four years show that the helm wind is not such a rare occurrence as it was popularly supposed to be, the bar having been observed on forty-one occasions in 1885, sixty-three in 1886, and nineteen in 1887. The phenomenon takes place usually when the sky to the eastward is covered with cloud.—An atmospheric sketch, by Mr. F. A. Velschow.—The drought in New South Wales in 1883–84, and rainfall at Corella, 1879–88, by the Ven. Archdeacon Wynne.

Royal Microscopical Society, February 13.—Annual Meeting.—Dr. C. T. Hudson in the chair.—The Report of the Council was read, showing an increase in the number of Fellows, and in the revenue of the Society. This will probably be the last annual meeting in the present library, which is required by King's College, and the Society will have to seek a new habitation.—Dr. Hudson delivered his annual address, taking as his subject, "Rotifers and their Distribution."

Paris.

Academy of Sciences, February 18.—M. Des Cloizeaux, President, in the chair.—On the vaccinal properties of pathogenic microbes transformed to simple saprogenic microbes destitute of all virulent properties, by M. A. Chauveau. These researches have been undertaken for the purpose of estimating

the value of certain facts supposed to throw some light on the natural history of micro-organisms in general, with special reference to the question of specific transformation. The main conclusion is that the charbon microbe entirely deprived of its virulence has not become the simple saprogenic microbe of ordinary fermentations set up in inorganic centres, for it has still preserved one of the most essential attributes that indicate the infectious nature of the pathogenic microbe; hence it has not undergone specific transformation. Such at least is the present inference, without prejudice to the question of possible ulterior metamorphoses of which *Bacillus anthracis* may be capable under the action of compressed oxygen or any other means. In a future communication it will be shown that at this stage the microbe has not even been deprived of the faculty of reverting to its virulent state.—On Egyptian blue, by M. F. Fouqué. The author has undertaken a fresh study of this pigment, which was discovered by Vestorius, of Alexandria, but which ceased to be made after the fall of the Western Empire. He finds its formula to be $CaO,CuO,4SiO_2$, consisting of 63·7 parts of silica, 14·3 of lime, 21·3 of copper oxide, with a trace of iron; specific gravity 3·04.—On two fossil Echinodermata from Thersakhan in Turkestan, by M. G. Cotteau. These specimens from the banks of the Sumbar, an affluent of the Attrek, are identical with the *Coraster vilanovæ* which abounds in the Upper Chalk of Alicante, Spain. Their presence in Turkestan at such a distance from the Pyrenees shows that at one time the Cretaceous seas occupied vast regions stretching eastwards to Central Asia and India.—Summary of the solar observations made at the Royal Observatory of the Collegio Romano during the second half of the year 1888, by M. P. Tacchini. Compared with the corresponding period for 1887 and 1886, the solar spots show a further decline in 1888, with a maximum of days without any spots. The protuberances have also decreased, but more irregularly, and at a less rapid rate.—On shooting-stars, by M. E. Minary. It is argued that the incandescence of these bodies cannot be explained by the transformation of motion into heat. The gases being perfectly elastic bodies, and in the upper atmospheric regions in an extremely rarefied state, heat cannot be produced by the shock of bodies endowed with great velocity and impinging on perfectly elastic molecules capable of receiving the motion and acquiring the velocity of those bodies; in this case the movement is communicated, not dissipated or transformed to heat. Had such transformation taken place, the velocity of the bodies on their trajectory would be progressively retarded, while the incandescence would be proportionately increased. But observation shows only luminous flashes, and more or less uniform velocities of translation at least for all the bodies that are not combustible. The reading of the paper was followed by some observations by M. Cornu, who remarked that the illumination of the trajectory of the shooting-stars might be attributed to a development or a discharge of static electricity without any considerable rise of temperature; as implied by the incandescence of detached particles of meteorites. This would agree with the spectral observations made on the shooting-stars, and would lend support to the view that certain cosmic phenomena, such as auroras, the zodiacal light, comets, solar protuberances, &c., are electric manifestations analogous to those that are so easily generated in rarefied gases.—On a general law relative to the effects of reversible transformations, by M. Gouy. It has been noticed that the effects produced by mechanical actions are often opposed to those actions (law of Lenz, thermic effects). Here M. Gouy establishes a general law, of which these facts form a particular instance, and which is applicable not only to direct mechanical actions, but also to a large number of reversible transformations.—Experimental studies on the dynamic and static elasticity of metallic wires, by M. E. Mercadier. As a complement to various researches in acoustics and thermodynamics, the author here determines the velocity of sound in metallic wires, first by directly registering their longitudinal vibrations, and then by deducing the velocity from the measurement of elastic expansions. His researches extend to copper, steel, platinum, aluminium, silver, and gold wire, varying in diameter from 0·5 to 1 millimetre.—On the rotatory power of crystallized chlorate of soda, by M. Ch. Eug. Guye. The results of these experiments agree fairly well with those obtained by M. Schuke for the visible parts of the spectrum. They may easily be reduced to a uniform temperature by employing the coefficient given by that physicist. These studies will be continued for the purpose of ascertaining whether the same coefficient is equally applicable to the ultra-violet radiations.—Tests for the mercaptans, by M. G. Denigès. Isatine, already used in sulphuric solution as a test for thiophene, is here shown to be also an excellent test for mercaptan.—On the origin of the eruptive rocks, by M. A. de Lapparent. From the constitution of the acid rocks—that is, those charged with silica—a fresh argument is drawn in support of the theory respecting the primordial fluidity of the globe.—Papers are contributed by M. M. Meslans, on the preparation and properties of the fluorides of propyl and isopropyl; by M. A. Lacroix, on the petrography of gneiss occurring in Ceylon and in Salem (Madras); and by MM. G. Weiss and A. Erckmann, on the optical properties of natural and false amber.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Natural Inheritance: F. Galton (Macmillan).—Cactus Culture for Amateurs: W. Watson (U. Gill).—Key to Lock's Elementary Trigonometry: H. Carr (Macmillan).—Key to Lock's Trigonometry for Beginners (Macmillan).—On Truth: St. G. Mivart (K. Paul).—The History of Ancient Civilization: edited by Rev. J. Verschoyle (Chapman and Hall).—Galileo and his Judges: F. R. Wegg-Prosser (Chapman and Hall).—The Coleopterous Fauna of the Liverpool District: J. W. Ellis (Liverpool, Turner).—Prodromus of the Zoology of Victoria, Decade xvii.: F. McCoy (Melbourne, Brain).—Das Klima des Aussertropischen Südafrika: Dr. K. Dove (Göttingen).—Proceedings of the Royal Society of Edinburgh, Nos. 126 and 127 (Edinburgh).—Report of the Marlborough College Natural History Society for Year ending Christmas 1888 (Marlborough).—Logic: R. F. Clarke (Longmans).—Practical Organic Chemistry: S. Rideal (Lewis).—Elementary Synthetic Geometry: N. F. Dupuis (Macmillan).—The Mineral Wealth of Queensland: R. L. Jack (Brisbane).—Basic Slag: C. M. Aikman (Edinburgh).—The Practical Use of the Spectroscope: J. Parry.—Zeitschrift für Wissenschaftliche Zoologie, Band 31–45 (Williams and Norgate).—Proceedings of the Boston Society of Natural History, vol. xxiii. Parts 3 and 4 (Boston).—Annalen der Physik und Chemie, 1889, No. 3 (Leipzig, Barth).—Quarterly Journal of the Geological Society, No. 177 (Longmans).—Journal of the Bombay Natural History Society, No. 4, vol. iii. (Bombay).—Kryptogamen-Flora von Schlesien, 3 Band, 5 Liefg. (Breslau).—Tōkyō Sūgaku Butsurigaku Kwai Kiji, Maki No. 4, Dai 2.—Die Natürlichen Pflanzenfamilien, Liefg. 26, 27, 28 (Leipzig, Engelmann).

CONTENTS.

THURSDAY, MARCH 7, 1889.

TOLLENS'S "CARBOHYDRATES."

Kurzes Handbuch der Kohlenhydrate. By B. Tollens. (Breslau: Maruschke and Berendt, 1888.)

THIS admirable *précis* of the chemistry of the carbohydrates is a model work of its kind. It is not merely a lucid account of this well-marked group of carbon compounds, but has the rare merit of preserving its facts and conclusions in their original guise—that is, as the offspring of research. Too often, the authors of treatises on experimental science, more especially of the genus text-book, are compelled to present their subject in such a way as to produce the impression that phenomena follow from laws, rather than that laws have followed from the phenomena which they generalize. This is no doubt justifiable, and in the end perhaps not seriously harmful, since the student is soon brought by his laboratory work to an appreciation of the perspective of his science, and to the correction of any superstitions which may have been engendered in his mind as to its origin and up-building. In the book before us, on the other hand, every fact is stamped and recorded as the contribution of a worker. A specialist, such as the author, engaged in active research in the field which he pauses, as it were, to describe, must write from the point of view of the worker; and hence it is that in the 330 short pages into which his account is condensed, we have over 1300 references to original memoirs. The impression produced, moreover, is that he has submitted this huge mass of experimental evidence to a searching examination, of which the matter of the book is the valid survival. It is to be hoped that the author's example will be generally followed. It is becoming less and less possible to keep pace with research in the many special branches into which chemistry is diverging. But if specialists will, as the author has done, unburden themselves to an imaginary interviewer, the task, which is laid upon us all, of keeping up with the progress of discovery and research will be both sweetened and lightened.

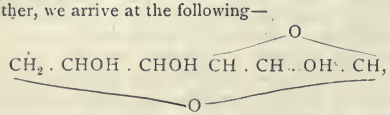
In proceeding to notice more particularly the contents of the work, we are struck, first, with its method of classification. The treatise is divided into two parts, the first dealing with the carbohydrates proper, together with such other comparatively inert compounds, *e.g.* arabinose, as stand in close connection with them; the second is an account of the derivative acids and their lactones ("saccharines").

The first section sets forth the general or typical properties and characteristics of the group, with an account of their origin in the plant, the evidence as to their molecular weight and constitution, their synthetical formation in the laboratory (phenose, acrose), their isolation in the pure state, their optical properties, and the various methods adopted for the determination of specific rotation. Then follows in outline the scheme of classification. The various groups under which the compounds are ranged are: (1) monosaccharides, or glucoses, $C_6H_{10}O_5$ (dextrose, lævulose, &c.); (2) disaccharides, or saccharoses, $C_{12}H_{22}O_{11}$ (cane-sugar, maltose, &c.); (3) polysaccharides—(*a*) crystalline (raffinose, $C_{36}H_{64}O_{32} \cdot 10H_2O$, and lactosin, $C_{36}H_{62}O_{31}$); (*b*) non-crystalline or *saccharocolloids*, &c., $C_6H_{10}O_5 \pm MH_2O$ (starch, inulin, gums, celluloses, pectone substances); (4) the somewhat miscellaneous group of substances which, although lacking some one or more of the group characteristics, are yet closely related to the carbohydrates—(*a*) in which $H : O = 2 : 1$ (arabinose, cerasinose, inosite, formose); (*b*) in which $H : O > 2 : 1$ (quercite, pinite, mannite).

Having thus forecast the order of treatment, the author plunges at once into the work of particular description. Beginning with dextrose, we have at the outset (*a*) a terse but minute account of the laboratory method of isolation from saccharose, also of the method of manufacture from starch; (*b*) certain physical properties of the anhydride and monohydrate, with solubilities and a table of specific gravities of aqueous solutions; (*c*) behaviour towards polarized light,—after a brief discussion of the variations between the numbers of different observers and their cause, we have the author's final selection of the formulæ, (1) for the anhydride $(\alpha)_D = 52{\cdot}5 + 0{\cdot}018796\,P + 0{\cdot}00051683\,P_2$, and (2) for the monohydrate $(\alpha)_D = 47{\cdot}73 + 0{\cdot}015534\,P + 0{\cdot}0003883\,P_2$, P being the percentage of substance in solution; (*d*) the results of heating at various temperatures; (*e*) actions of acids; (*f*) actions of alkalies; (*g*) action of nascent hydrogen (conversion into mannite); (*h*) action of the halogens and various forms of oxygen; (*i*) action of oxidizing (metallic) oxides; and (*k*) its several fermentations. Then follows a detailed account of the derivative compounds of dextrose: (A) with bases; (B) with negative radicles; (C) ethereal compounds; (D) hydrazine derivatives; (E) compounds with aromatic amines; (F) with metallic salts; (G) with hydrocyanic acid (conversion into normal heptoic acid). The author then gives a detailed account of analytical methods, *i.e.* (i.) identification by qualitative reaction; (ii.) estimation by polarization of the various oxidation methods (Cu, Hg, and Ag salts), and by fermentation. We have reproduced these heads, under which the chemistry of dextrose is treated, as characteristic of the method of the book. The typical saccharose, cane-sugar, is dealt with in even greater detail, the author giving a complete though brief account of the process of preparation from beet, with illustrated descriptions of the manufacturing plant, as also of the polarization instruments commonly employed for sugar estimation, viz. the Soleil-Ventzke-Scheibler, and that of Schmidt and Häusch.

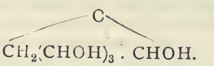
Of the amorphous polysaccharides or saccharocolloids, starch is treated at considerable length. The views of physiologists as to its origin in the plant are briefly discussed. Its resolutions by the various hydrolytic reagents are dealt with in detail, and due prominence is given to the results of the researches of O'Sullivan and of Brown and Heron. Special details are given of the methods of estimation of starch in farinaceous raw materials, with a description of the Lintner-Soxhlet apparatus for carrying out the acid hydrolysis. As the starch group is characterized by resolution into dextrose, so the inulin group, next described, appear to be poly-derivatives of lævulose. A third is composed of substances yielding galactose as a product of resolution (lævulan, galactan, Carragheen mucilage), while a fourth comprises such

gums and mucilages as yield various glucoses, and sometimes also arabinine, as products of hydrolytic resolution (gum-arabic, cerasin, bassorin, "wood gum"). Ascending in the scale of molecular complexity, the celluloses are next described; and lastly, in separate sub-sections, lignin, cork, and pectic derivatives. We have been interested in comparing the author's account of the latter group with those given under "Celluloses" in the new edition of Watts's "Dictionary." There is a close agreement in general method of treatment; and it is satisfactory to find that the problem of the constitution of the celluloses is being worked on such lines as would be laid down from a study of the general view of the carbohydrates which Prof. Tollens has given us. Neither of these acounts of cellulose suggests any probable constitutional formula for the unit group of which the celluloses may be regarded as poly-derivatives; but, putting the views of both authors together, we arrive at the following—

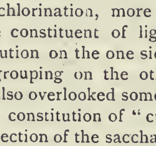


which we think will at least be found of service as a working hypothesis. In the section "Lignin," Prof. Tollens has overlooked some of the researches of recent years, which have, as the result of investigations of its hydrolysis and chlorination, more definitely identified the non-cellulose constituent of lignified tissue with a furfuralic constitution on the one side, and the tendency to an aromatic grouping on the other. In the section "Cork," he has also overlooked some of Fremy's interesting work on the constitution of "cuticular" substances The concluding section of the saccharocolloids comprises the pectic group of plant constituents, which are briefly but fully described.

The fourth section opens with an account of *arabinose*, which the author represents by the constitutional formula—



Formose, or polymerized formaldehyde, is not regarded as a true glucose. Carius's *phenose* is described; the evidence as to its composition, however, is very slender. The writer has, moreover, devoted some time to a repetition of Carius's research, but failed to obtain any product such as that described. *Inosite* is represented on the basis of Maquenne's researches, by the symmetrical formula—



Dambonite and *bornesite*, which are found in caoutchouc-yielding juices, are described as the mono- and di-methyl ethers of inosite, respectively.

The fifth section comprises the hexahydric alcohol mannite and its isomerides, dulcite, sorbite, and perseite, of which the typical mannite is fully described.

The second part of the work gives a detailed account of "substances closely related to the carbohydrates, and obtained for the most part from them, which are either acids or acid-anhydrides (lactones), and the molecule of which contains 6 C-atoms": (*a*) saccharinic acid, $C_6H_{12}O_6$, and its isomerides, iso- and *meta*-saccharinic acids, their lactone-anhydrides, $C_6H_{10}O_5$, "*saccharines*," and their oxy-derivatives, *saccharones*, $C_6H_8O_6$; (*b*) gluconic acid, $C_6H_{12}O_7$, and its isomerides, galactonic and arabinose carbonic and mannitic acids; saccharic acid, $C_6H_{10}O_8$, and its isomerides, mucic, iso-, meta-, and para-saccharic acids, with their lactonic and other derivatives.

Such is a brief account of the contents of the book before us. The main purpose is necessarily the theoretical treatment of the subject—from the point of view, that is, of pure chemistry; at the same time due prominence is given to technical considerations, and the physiological aspects of the subject are by no means excluded.

We do not believe that we are yet within measurable distance of a knowledge of the actual molecular constitution of the carbohydrates. We do not even think that the evidence for the C_6 formulæ of the simplest members is at all conclusive. One of the assumed criteria of this point—the calculation of molecular weight from the lowering of the freezing-point of solvents by the presence of compounds in solution, as proposed by Raoult—is not mentioned by Prof. Tollens. At a recent meeting of the Chemical Society, Messrs. Brown and Morris communicated the results of an investigation of the freezing-point of solutions of the simpler carbohydrates, which appear to confirm the generally accepted views. Nevertheless, it would be premature to pronounce at all positively, either as to this particular criterion, or as to the evidence generally on these points.

The chemistry of the carbohydrates brings us into contact with the syntheses and transformations of the organic world. The plant is, and will remain through all time, the chemist's ideal. Every contribution to the chemistry of the carbohydrates is a step towards a comprehension of the chemistry of elaboration. The number of workers in the field is relatively small, but will increase as the methods of investigation are rendered more precise. It is from this point of view that we commend this little work to the notice of all who have either a special or general interest in plant-chemistry.

BRITISH MOSSES.

British Mosses. By F. E. Tripp. 2 Vols. New Edition. (London: George Bell and Sons, 1888.)

THE fact that a new edition of this work has been called for shows that these elegant plants have a wide-spread circle of admirers, and that there are many seekers after a knowledge of their structure.

The introduction occupies thirty-eight pages. In Sect. 1, on the homes of mosses, we have a highly poetical description of the third day of Creation, and production of vegetation according to Genesis. This is followed by the natural distribution and habits of the mosses; and by charming word-pictures of woods and moorlands the authoress compels her readers to accompany her and see for themselves the beauties that await observation on every side and at every season.

Sect. 2, the characteristics of mosses, is written in

somewhat the same poetic strain, and glances at the leading aspects of mosses in natural scenery. Sect. 3, structure of mosses, gives a clear and succinct outline of the various organs of these plants, and their functions. Sects. 4 and 5, collecting and examining mosses, and uses of mosses, are the best in the book, and show the devotion of the writer to the study of this branch of botany. Then follows a synopsis of genera after Schimper's "Syn. Musc. Eur.," and at p. 45 we arrive at the description of species and plates, to which the remainder of the work is devoted.

The descriptions are all on one uniform plan, very short and under five heads—colour, stems, leaves, capsule, locality. The text is therefore somewhat monotonous and dry, while the essential points characteristic of the species are not always brought out; *e.g.* the Bryinæ are "Plants cellular, germinating from spores, with stems and leaves; fruit a capsule," which applies equally well to the Hepaticæ. With species this want of definiteness in description is apt to lead altogether astray. In other instances an erroneous term is introduced; thus *Trichostomum nitidum* is stated to have leaves "hairy at apex," *T. litorale* leaves "with short hair-points," whereas both have solid conical points, formed by the nerve.

The cell-structure of the leaves is a most important character, and is requisite both in descriptions and illustrations, but is not treated sufficiently in either. The 37 plates represent the plants of the natural size, and are very well coloured, so that the larger species may be readily recognized; but the leaves are not sufficiently magnified nor their structure sufficiently defined to render them sure guides, for the smaller species are too much alike, and the smallest of all, represented on Plate 5, it would puzzle any bryologist to discriminate.

Although for these reasons the work is not so helpful to the student as it might be, it forms an elegant table book. The paper is excellent, and the clear symmetric printing could hardly be surpassed.

OUR BOOK SHELF.

Catalogue of the Marsupialia and Monotremata in the Collection of the British Museum (Natural History). By Oldfield Thomas. (London: Printed by order of the Trustees, 1888.)

THIS is one of the new series of Zoological Catalogues of the British Museum, which, from their containing descriptions of all the known species of the group catalogued, form handy and excellent hand-books for the student, and serve for much more than records of the treasures of our British Museum.

This volume contains the descriptions of 151 species of Marsupials and 3 of Monotremes, in addition to descriptions of 12 well-marked varieties of the former and 2 of the latter order. Of this large total of 168, only 20 are not represented in the British Museum collection. The specimens amount to 1304 in all, of which 173 are preserved in spirits.

This is a very marked increase above the number in the list published in 1843, in which but 94 species were enumerated. Apart from the number of species represented in the collection, the value of these is greatly increased when they are "type" forms. In such forms, the British Museum is extremely rich, possessing 74, followed by the Paris Museum with 21, and then, at a long distance, by the Museums of Sydney and Leyden, with 8 each. In this Catalogue, probably for the first time, a double synopsis of each genus and species is given, in order to enable the student to identify a specimen either from its external characters, or from its skull alone. In order to make these latter synopses useful, explanations of the nomenclature and of the measurements are given.

The synonymy of the genera and species is worked out in very great detail, and in the case of the Monotremes we have in addition references to the literature bearing on the anatomy, embryology, &c., of the forms belonging to the order.

Although most of the species of Marsupials have been named within the last hundred years, and the greater number of them have names of quite recent date, yet the hasty descriptions of some authors have added much to the list of synonyms. In addition to the ordinary synonymy, Mr. Thomas has in most instances given references to the more important papers on the anatomy of the forms. These references make this Catalogue useful to the comparative anatomist as well as to the zoologist. To make such a list perfect would require much space, but, so far as we can judge, all the more important papers have been referred to; under *Phascolarctus cinereus*, we would add one on its anatomy by Prof. Macalister, in the *Ann. and Mag. Nat. Hist.*, 1872, vol. x., and one on the occurrence of a premaxilla-frontal suture in the skull, by Prof. Mackintosh (Proc. Roy. Irish Acad., n. s., vol. iii.).

We hope the day is not far distant when all the mammals in the British Museum collection will be catalogued in an equally accurate and effective manner.

Report of the Proceedings of the United States Expedition to Lady Franklin Bay, Grinnell Land. By Adolphus W. Greely. Vol. I. (Washington: Government Printing Office, 1888.)

EVERYONE knows, at least in its main outlines, the story of the Polar Expedition commanded by Lieut. Greely. Three years ago (NATURE, vol. xxxiii. p. 481) we reviewed the work in which he presented an interesting popular account of his experiences. The present volume contains the official Report, dated Washington, June 30, 1885, which Lieut. Greely addressed to the Chief Signal Officer of the United States army; and a singularly fascinating Report it is—all the more fascinating as no attempt is made to set forth the facts in a lively or picturesque style. The writer is so completely occupied with the events he records that he seems to have neither time nor inclination for any thought about the manner in which they should be presented. As appendices to the Report an immense number of documents relating to the Expedition are printed; and many of these are of considerable value, not only supporting the statements of the Report, but adding details which give freshness to the central narrative. The volume is enriched by an abundant supply of excellent full-page illustrations, illustrations grouped in plates, illustrations in the text, and maps and charts.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Origin of Coral Islands.

MR. MURRAY's concise explanation of the formation of coral reefs and islands presents advantages in more than one respect. It demands no *a priori* assumptions, but begins and ends with that which can be observed, while Darwin's theory requires the preliminary concession of subsidence, which never has been and never

perhaps can be observed. It must appear ungracious to question a theory that accords so completely with the natural history of coral islands, but even this theory requires a geological concession, and that is stability. Coral islands, it may be supposed, after all only differ from other oceanic islands in being crusted over with coral, so that we cannot see their original state, and the question is whether we can grant such long periods of stability to them, from our experience of other oceanic islands, which are free from coral and can therefore be observed. Nearly all oceanic islands are volcanic, and it is probable that their elevation coincides more or less with the period of volcanic activity somewhere along their line. It is obvious that coral islands are not formed during this phase, because no theory would then hold good; the peaks would grow through and carry up the coral, which might leave only such small traces of its existence as we find in a single spot in Madeira. It would not be unreasonable to suppose that if the expansive and elevating force were withdrawn the peaks would slowly subside, and that if there are some lines of elevation, there must be others of subsidence, unless the earth is as a whole growing in bulk. Darwin claims the existence of areas of subsidence, and that these are eminently favourable to coral growth, and it is quite apparent that if the Island of Madeira were to sink, as it has undoubtedly risen, its last appearance in a coral sea would be as an atoll. We shall never see the interior structure of a stationary or subsiding coral island, and can only look for a re-elevated example with a crust that has been protected from solution whilst dead and submerged, and yet not sufficiently so to mask the core.

In submitting geological considerations I am not questioning any of Mr. Murray's observations, which are in every way admirable, though it does appear to me doubtful whether atolls could increase outwards in deep water on their own talus, in face of the dissolution of dead coral that is claimed to take place in the interior of the lagoons, and yet more so in deeper water.

J. STARKIE GARDNER.

The Sun's Corona, 1889.

A GENERAL statement of the successes of the Western Eclipse Expeditions on January 1 has already appeared in NATURE. More photographs of the corona were taken than ever before—many of them indifferent and worthless, but an unusually large number of great excellence. The best that I have so far seen were taken with 5-inch telescopes, by Mr. W. H. Pickering at Willow, and by Captain R. S. Floyd at Lakeport, both in California. The latter's lens was newly made by Clark, on the Stokes-Pickering plan, convertible from optical to photographic use by reversing the crown lens.

Until the photographs can be well collated, there is little use in presenting them; and the difficulties arising in this work are by no means easy to meet.

With the drawings, however, the case is different. These were made in great abundance, and I have received sufficient responses to my printed instructions to afford very satisfactory conclusions as to the appearance of the corona. The state of the sky was practically everywhere favourable throughout California, Nevada, Idaho, Wyoming, Montana, Dakota, and Manitoba.

The instructions for sketching the corona were printed in three sections: (1) drawings of the corona as a whole; (2) drawings with small telescopes of the filaments about the solar poles; (3) sketches of the outlying streamers along the ecliptic. These latter were made with the assistance of an occulting disk, set up at such distance from the eye of the observer that it would subtend an angle of 65′. It was supposed that disks of this size, being much larger than those used on any previous occasion, would hide very nearly all the inner corona, and leave the eye free to follow the faint outer filaments to their farthest limit. The magnitude and brilliancy of the inner corona, however, were such as to convince me that the disks might better have been one-fourth larger.

From the best of all the drawings now available, of the three classes, Mrs. Todd has prepared the accompanying sketch of the corona. This was done without knowledge of the details shown on any of the photographs, and it may be taken as an index of the sort of results which may be derived from the co-operative plan of figuring the optical corona. It is also instructive in studying the differences between the optical and the photographic corona.

DAVID P. TODD.

Amherst College Observatory, February 22.

[No sketch was received with this letter.—ED.]

The Meteoric Theory of Nebulæ, &c.

THERE would appear to be a difficulty in the theory of the meteoric constitution of nebulæ,[1] &c., which, as far as I am aware, has not been mentioned.

It is, namely, the fact that some gas—probably part of it permanent—exists in the nebula along with the moving masses in translatory motion. Making allowance for the relatively small effect of gravity on the gas, due to the diffuse distribution of the matter, and consequently having regard to the probable tenuity of the gas; it has nevertheless, I find, been estimated by Joule ("Scientific Papers," vol. i. p. 539) that meteors are first observed at a height of 116 miles in the earth's atmosphere. He estimates that 0·0003 of a grain of air is contained in a column of air one mile long, and one square foot in cross section at that height. This, I find by calculation, amounts to 1/1000 millionth of an atmosphere in round numbers as to density.

So that if in some nebulæ the gas had something like this small density, the bodies, or masses moving in translatory motion according to the kinetic theory, would (if their velocity were at all comparable to that of those colliding in the earth's atmosphere) behave as meteors, or inflame; and so apparently be rapidly converted into gas. Even if they did not inflame; no doubt the heat consequent on friction would be considerable. It might be suggested, perhaps, that the mass of these bodies in some nebulæ may be so great that they do not lose their translatory motion rapidly, even if they leave a luminous track. In any case it is evident that this stage of evolution is not a lasting one, and, to my mind, it seems that it is less permanent than is perhaps generally supposed.

I find that Mr. G. H. Darwin, in his paper in the Philosophical Transactions, 1889, above alluded to, suggests the hypothesis that the "metallic rain" generated by the condensation of the incandescent vapour of iron could "fuse with old meteorites whose surfaces are molten." It seems to me that the rate of translatory motion, calculated by him at $5\frac{1}{2}$ kilometres per second, is scarcely allowed for here. How, it may be asked, could such "metallic rain" fuse on bodies colliding against it at this velocity? Some are moving at a less velocity, no doubt; but some are moving at a greater.

The temperature equivalent to this value for translatory motion ($5\frac{1}{2}$ kilometres per second) is, I find, 36,000° C. (about); *i.e.* this would be the temperature if the translatory motion alone were entirely converted into heat. Clausius has calculated, I believe, that in a gas the ratio of the whole energy (which includes translational and vibrational energy of molecules) is to the vibrational energy alone as the specific heat at constant pressure is to that at constant volume.[2] If this be the case, a very large proportion of the translatory motion is resolved into internal motion—that motion which emits the waves of heat analyzed in the gas. Must not the same be true of meteoric masses: or is not the principle (ratio) independent of the *scale*, or number of molecules clustered about a centre, and moving as one lump in the motion of translation? In some complex gases, at least fifty to sixty molecules may be clustered about a centre to form a lump. Then if more (as in a meteorite) are so clustered, it appears that the same must hold true, as regards subdivision of the energy between translatory motion and vibratory motion (heat). If so, by the great temperature equivalent of the translatory motion (viz. 36,000° C. above estimated), the meteorite would rapidly be dissociated into separate molecules by the subdivision of the energy according to the above principle—just as the more firmly united constituents of the lumps (*i.e.* compound molecules) of gases would be dissociated, even if moving at but a fraction of the above translatory velocity.

Is it supposed perhaps that the length of path between encounters (giving time to cool?) in meteorites constituting nebulæ, prevents this? This point is not apparently gone into in Mr. Darwin's paper. But if the meteoric mass has time (nearly) to cool down, or lose, by radiation into stellar space, the heat generated at each successive collision, then it would seem that the translatory motion would be somewhat rapidly lost by con-

[1] I allude specially to Mr. G. H. Darwin's paper, "On the Mechanical Constitution of a Swarm of Meteorites," of which an abstract appeared in NATURE of November 22 and 29, 1888 (pp. 81 and 105). The paper is contained in full in the Philosophical Transactions, vol. clxxx., 1889.

[2] It may be curious to observe that, if a meteoric swarm whose mass equalled that of the sun, were contained within an impenetrable envelope, whose radius equalled the radius of Uranus's orbit (nearly), the mean density of the meteoric swarm would be one five-millionth of an atmosphere only; which represents a fair "vacuum" of a Sprengel pump. This degree of distribution of matter may be represented by a centimetre cube of iron placed at the corner of a cube 139 metres in the side.

version into heat (wasted in space); and this, again, is an indication of the relatively small permanence in such a system, before pointed out as a probable fact. If there is not much free gas in a nebula, the heat radiated by the meteoric masses into space will be great, because unobstructed by the gas. If, on the other hand, there is much free gas in the nebula, it will fritter the translatory motion down by friction into heat. A translatory motion whose temperature equivalent (36,000° C.) is from ten to twenty times more than sufficient to volatilize the moving masses, if utilized, could scarcely exist for a lengthened epoch, or this would seem to be an unnatural state of things.

If the meteoric masses had a mean length of path at all comparable in relative scale to that of a gas at normal density; such as, for instance, if the mean path were (merely for illustration) 1000 times the diameter of the meteorite; then it is evident that the whole system—by a translatory motion of $5\frac{1}{2}$ kilometres per second—would be resolved into gas in a few minutes or even seconds of time. The question then becomes, as it seems, How far does lengthening the mean path diminish the tendency to resolution into vapour by allowing time to cool between the encounters? or some mechanical relations might possibly be demonstrated here from elements[1] or physical data determinable apparently. S. TOLVER PRESTON.

Paris, February.

Upper Wind Currents over the North Atlantic Doldrums.

THE following observations were taken on board the steamship *Araucania* on her voyage from Liverpool to Valparaiso in December last:—

From the Cape Verde Islands down to 9° N. lat. the surface wind was steadily north-east, but the low clouds came as persistently from south-east, and the middle or high layers from south-west.

About 5° N. the wind worked gradually through east to south-east, and we experienced no calm doldrum, nor even a belt of variable winds. From here to the equator the surface wind remained south-east, while the low clouds came from between south and south-east, but the middle and high layers still passed from south-west.

From the line till about 10° S., while the surface wind continued to blow from south-east, the high cirrus moved from the north-west.

The circulation of the atmosphere, indicated by these observations, is very different from that described by myself in your columns on two former occasions. On one, while traversing the same track as now, only in the month of July 1885; and on another while going from Cape Verde to Cape Town in December of the same year, I found the highest current over the doldrums coming from the east. Now there was no doldrum at all, and though there were 200 miles of latitude between the place where the last south-west highest current and the first north-west highest current were observed, it seems somewhat improbable that there was a narrow belt of high-level east winds between these two currents from some point of west.

It may be noted that cirrus came from the south-west for about 300 miles of southing over the south-east trade, and that a low current from south-east blew over both trades from 6° S. to 13° N. RALPH ABERCROMBY.

Straits of Magellan, January 15.

The Giant Earthworm of Gippsland.

IN the last issue of NATURE (p. 394) I observe in an article upon *Megascolides australis* that a supposition is expressed that very large earthworms will be found to occur in South America as well as in other continents. It may be of a little interest to mention that I found near the town of Manaos, in Amazonia, in the year 1874, an earthworm that measured 30 inches in length by $\frac{3}{4}$ inch in greatest breadth. When found, in the early morning, it was quite fresh, though newly dead, being somewhat crushed near one end, probably by some passer-by in the darkness. Unfortunately the worm spoiled in the rum in which I attempted to preserve it. JAMES W. H. TRAIL.

University of Aberdeen, February 27.

Weight and Mass.

PROF. GREENHILL seems to have overlooked the fact that my letter in NATURE of February 7 (p. 342) related entirely to procedure in teaching. I merely stated that as the result of experience I have found it absolutely necessary to use terms strictly in the senses assigned to them by definition, and not to use the same term in two senses. I find that it conduces to clearness and accuracy to use the word "pound," for example, only in the sense of a certain quantity of matter, and to use the phrase "weight of a pound" when speaking of the force of gravity on that quantity of matter.

With the ordinary expressions used by engineers when addressing engineers or other persons who, presumably, are able to distinguish between the different senses in which the same term or phrase is used, I have no quarrel whatever, and must decline Prof. Greenhill's invitation to express an opinion as to the accuracy of the phrases which he quotes from NATURE.

University College, Bangor, February 25. A. GRAY.

The Formation of Ice.

IN connection with the discussion on the formation of ice in crystals, it might be worth while to record that on December 6, 1861, in a slight frost, I saw some in the process of formation in a trough of water. There were three thin pieces of ice in it, two irregular, but the third a beautiful star, 4 or 5 inches in diameter, having six feather-like rays which were branched twice or thrice, in all cases at an angle of 60°. Also, two days before, when the water in the trough was frozen over, I observed in it six-rayed stars several inches in diameter very slightly raised above the rest of the surface. T. W. BACKHOUSE.

Sunderland, March 2.

ROTIFERA AND THEIR DISTRIBUTION.[1]

IT is no longer possible, I think, for your President to give, as the substance of his address, a summary of the most important improvements of the microscope, and of the most remarkable results of microscopical research, which have been recorded in the preceding twelve months.

All this is now so fully and so admirably done in your own journal, by your energetic Secretary and his able colleagues, that your Presidents will most probably, in future years, have to follow the excellent precedent set by Dr. Dallinger, and choose for the subject of their address some topic directly springing from their own special studies. For, on an occasion like this, each President would wish to give the Society the best he can, and it is clear that this best must be sought for among matters of which he has a special knowledge.

Unfortunately, an accident, which befell me early last year, not only robbed me of the pleasure of being present at several of your monthly meetings, but also produced consequences that compelled me to put my microscope aside; and, as I had not long before finished my share of the "Rotifera," I feared at first that I had lost the power of pursuing any new investigations, just at the very time when I had published the results of all my old ones.

There is, however, still a portion of my subject with which I am familiar, and which, I believe, has not as yet been touched upon by anyone; and I venture to

[1] It is said that "The total energy of agitation in an isothermal adiabatic sphere is half the potential energy lost in the concentration from a condition of infinite dispersion" (NATURE, Nov. 29, 1888, p. 107). This is apparently the analogue of the ratio of Clausius, somewhat differently expressed, viz. the ratio between the two parts of the energy, translational and vibrational (internal motion), applicable to a rigid body, and calculated at $\frac{2}{3}$ *a priori* by Maxwell. I would venture one remark here. It appears evident that if the mean thermal equivalent of *half* the potential energy lost were all accumulated in the meteorites, they would be volatilized. If, on the other hand, part of this thermal equivalent were dissipated in space by radiation, the meteorites could not possess their natural equivalent of thermal energy due to the translatory motion, and consequently it would seem that in the continued effort towards the equalization of these two forms of energy (translatory and thermal), the translatory motion would with tolerable rapidity degrade down to a value which could no longer support the weight of the superincumbent material. This would be another argument for the small degree of permanence of such a system.

[1] Address delivered at the Annual Meeting of the Royal Microscopical Society, by Dr. C. T. Hudson, President, on February 13, 1889.

hope that I may make it interesting to you. It relates to what may be called the foreign Rotifera; that is to say, to those Rotifera which have not as yet been found in our islands. One would naturally like to know what proportion these foreign species bear to the British; whether there are any families or genera entirely absent from the British fauna; whether there appears to be any law of distribution among the Rotifera; and how far it is possible to account for the existence of the same species in places which are thousands of miles apart. But many of the numerous memoirs, from which information on these points is to be derived, are only to be found, scattered widely, in various European periodicals; and so are difficult to be procured; while, of those that have been published separately, the best are rare.

Under these circumstances I thought it not improbable, that the members of our Society might be glad to know, that the task of studying and condensing these memoirs had been, in the main, accomplished; and that I am able now to present them with some of the results.

In the first place, I made a list of all the known species, and marked against each the various localities in which it has been found. It was curious to see, as the table grew, how certain well-known Rotifera were picked out by their rapidly advancing scores, till at last about fifty typical Rotifera were separated from the rest; while, of these, a smaller group enjoyed the further distinction of having a very wide range, not only in latitude and longitude, but also in altitude.

The same table showed at a glance that Great Britain decidedly outstripped all other countries in the number of its recorded species, having quite two-thirds of the whole. Nor was this all, for the Rotifera seemed, like trade, to follow the flag, and to haunt the British colonies just as if they were British ships.

The reason, for this curious pre-eminence of British Rotifera, is clearly seen when we notice how those species are distributed, which have as yet been found in one country only. There are about 240 such species, and of these no fewer than 173 (that is to say, more than two-thirds) are peculiar to Great Britain. It is, of course, obvious that this apparent selection of Great Britain as the fatherland of the Rotifera is simply due to the greater energy, industry, and skill with which the search for new species has been pursued in this country. It is, however, very remarkable that the naturalists of Great Britain should, in late years, have added to the Rotiferous fauna two and a half times as many species as the naturalists of all other countries put together have done; and this highly honourable result is mainly due to members of your own Society, and especially to my deeply-lamented colleague and dear friend, the late Mr. Philip Henry Gosse, F.R.S.

After I had seen how greatly the value of the recorded distribution of the Rotifera was affected by what I may term the "personal equation," I at first feared that I should obtain little else from my tables than a well-merited tribute to the energy of British naturalists. Further inspection, however, showed other points that are well worth your notice.

In the first place, my lists showed that Germany, Switzerland, and Hungary come next in order to Great Britain, in the total number of species that each records; and I have only to mention the names of Ehrenberg, Leydig, Cohn, Grenacher, Zacharias, Eckstein, Plate, Imhof, Perty, Bartsch, Vejdovsky, Zelinka, not to say many others, to make it obvious that the result is due, not to the real distribution of the species in these countries, but to the comparative skill and industry of their naturalists.

Next, my table shows clearly that in all cases a considerable number, and in some the great majority, of the above-named fifty typical Rotifera, range throughout Britain, France, North and South Germany, Denmark, Switzerland, Hungary, and Russia; so that we may reasonably conclude that a considerable proportion of the 450 known species would probably be found in almost any part of Europe, if they were diligently searched for. Here, for instance, is a list of thirty well-known Rotifera, all of different genera, and all recorded in at least five of the above eight European countries:—

Floscularia ornata	Diglena catellina
Stephanoceros Eichornii	Mastigocerca carinata
Melicerta ringens	Rattulus lunaris
Limnias ceratophylli	Dinocharis pocillum
Lacinularia socialis	Scaridium longicaudum
Philodina roseola	Salpina mucronata
Rotifer vulgaris	Euchlanis dilatata
Actinurus neptunius	Cathypna luna
Asplanchna helvetica	Monostyla cornuta
Triarthra mystacina	Colurus uncinatus
Hydatina senta	Metopidia lepadella
Notommata aurita	Pterodina patina
Proales decipiens	Brachionus urceolaris
Furcularia forficula	Anuræa aculeata
Eosphora aurita	Notholca striata.

Besides, many of the Rotifera are very tolerant of climate, and appear to be able to live anywhere that they can get food. For instance, *Rotifer vulgaris* is to be found all over Europe, and at all heights; thriving under moss, near the top of the Sidelhorn, and on the Tibia, at an altitude of 9000 feet above the sea. It has been met with also in Nubia, on the slopes of the Altai Mountains in Siberia, in Ceylon at the top of Adam's Peak, in Jamaica, and in the Pampas of La Plata. *Brachionus pala* has nearly as great a range; for it has been found in many parts of Europe, in Egypt, at the Cape of Good Hope, in Siberia, Ceylon, Jamaica, and New Zealand. Besides these, *Diglena catellina*, *Hydatina senta*, *Actinurus neptunius*, and a few others, have all been met with in different quarters of the globe. But the distribution of the Rotifera presents us with other facts quite as curious as these. For not only are European species to be found ranging over Asia and Africa, but America, and even Australia and New Zealand, in spite of their ocean belts, possess the same familiar creatures; and, moreover, seem to have hardly any peculiar to themselves. Here, for example, is a list of Rotifera that have been found in Sydney by Mr. Whitelegge, and in Queensland by Mr. Gunson Thorpe:—

Floscularia ornata	Conochilus volvox
,, campanulata	,, bullata (*n. sp.*), *T.*
,, cornuta	Asplanchna Brightwellii
,, Millsii	,, ebbesbornii
,, coronetta (*var.*), *W.*	Cephalosiphon limnias
Melicerta ringens	Actinurus neptunius
,, conifera	Rattulus tigris
Œcistes crystallinus	Notommata centrura
,, janus	Euchlanis triquetra
Limnias ceratophylli	Dinocharis pocillum
,, annulatus	,, triremis (*n. sp.*), *W.*
,, cornuella	Brachionus militaris
Lacinularia socialis	Anuræa cochlearis
,, pedunculata (*n. sp.*), *W.*	Pedalion mirum.

Mr. Thorpe has also found what seems to be a swimming Floscule, with a forked foot and a dorsal eye; as well as a new *Noteus* or *Brachionus*, with a strangely unsymmetrical lorica, bearing ten spines in front, and three behind. Who would ever have imagined that, in a sea-girt continent, at the opposite side of the globe—in a land whose fauna and flora are so strange as those of Australia—we should find that twenty-four out of thirty recorded species were British; and that, of the remaining six, one (*Floscularia Millsii*) had a habitat in the United States?

The United States, too, Jamaica, and Ceylon all reproduce the same phenomenon, though on a reduced scale; so that the question at once arises, How could these

minute creatures, who are inhabitants of lakes, ponds, ditches, and sea-shore pools, contrive to spread themselves so widely over the earth? Take, for instance, the case of *Asplanchna ebbesbornii*, which till quite lately had but one known habitat, viz. a small duck-pond in a vicarage garden in Wiltshire. The very same animal has been found by Mr. Whitelegge in the botanical gardens at Sydney, New South Wales. No doubt, in time, it will be found elsewhere also; but how, or when, did it pass from the one spot to the other?

Again, there is the strange Floscule, *F. Millsii*, a Rotiferon apparently linking together the genera *Floscularia* and *Stephanoceros*, and which has been found almost simultaneously by Mr. Whitelegge at Sydney, and Dr. Kellicott at Ontario. The possibility of its journeying between two such points seems quite as hopeless as that of *Ashplanchna ebbesbornii's* passing from New South Wales to Wiltshire.

And such cases are numerous. How did *Hydatina senta* and *Brachionus pala* get to New Zealand? or *Notops brachionus* and *Rotifer vulgaris* to the top of Adam's Peak, and the Pampas of La Plata? Again, there is *Pedalion mirum*: since I first found it, in a pond at the top of Nightingale Valley, at Clifton, it has been met with in four or five other places in England, including a warm water-lily tank at Eaton Hall; but, till quite lately, in no other country. Now I have just received a letter from Mr. Gunson Thorpe, telling me that he has found it swarming in a pool on a rocky headland in Queensland.

You have, no doubt, long ere this anticipated the solution of the puzzle, and see clearly enough that living creatures, to whom a yard of sea-water is as impassable a barrier as a thousand miles of ocean, could only have reached or left Australia, New Zealand, Jamaica, Ceylon, &c., in the egg; not the soft, delicately shelled, and quickly hatching summer egg, but the ephippial egg, which is protected by a much harder and thicker covering, which is constructed so as to bear without injury a long absence from the water, and which hatches, so far as is known, some months after it has been laid.

But this explanation still requires to be explained. The case of the free-swimming Rotifera is simple enough. They are most of them to be found, at some time or another, in small shallow pools, and their eggs either fall to the bottom of the water, or are attached to the small confervoid growth on the stones in it. Such pools frequently dry up, leaving the ephippial eggs to wait for the rainy warm weather of next year. Then comes boisterous weather, and the dusty surface of the exposed bottom of the pool is swept by a wind, which raises the dust high into the air, ephippial eggs and all. For these latter are minute things; few exceeding one three-hundredth of an inch in length, and many even half that size. Once raised in the air, I see no reason why they should not be driven by aërial currents, unharmed, half round the globe, falling occasionally in places where water, temperature, and food are alike suitable.

The dust of the eruption at Krakatão, which gave us such wonderful sunsets and green moons in 1883, travelled from the Sunda Isles to England in three months; and so the ephippial eggs of *Asplanchna ebbesbornii*, and other Rotifera, may have traversed the distance from England to Australia, and yet have been capable of hatching at the end of the journey.

It may perhaps seem a fanciful notion to account for the stocking of the ponds at Sydney by eggs carried thousands of miles in the air, but several well-known facts warrant the hypothesis. The tops of our houses, the heights of the Alps, the slopes of the Siberian mountain ranges, are haunts of the Philodines, which, being an exceptionally hardy race, have accommodated themselves to living in damp mosses at the edge of a glacier, or in a gutter which now holds a mere handful of stagnant water, now is a racing current, and now a dusty leaden basin, glowing under a blazing sun. No doubt eggs of all sorts of species fall on the same spots, but only to perish under trials that none but a Philodine could survive.

How various are the species whose eggs are thus wafted up by the air has been well shown by Mr. J. E. Lord, who has given a list of no fewer than forty-five species (contained in twenty-nine genera) that he found, in the course of twelve months, in the same garden pond. It was, however, admirably situated for catching whatever there was to be caught, for it lay in a flat plot of ground, where there was an entire absence of trees and shade, so that its surface was fully exposed to every wind that blew.

The eggs, of course, must often fall on unsuitable places, and be carried past suitable ones, and this accounts for the capricious appearances of Rotifera in some well-watched ponds, and for the frequent disappointments of the naturalists who visit it. To this aërial carriage of the eggs is also due the otherwise perplexing fact that, when any rare Rotiferon is found in one spot, it is frequently found at the same time in closely neighbouring ponds and ditches, even in such an unlikely hole as the print of a cow's foot filled with rain, but not at all in more promising places at some distance off.

Admitting, then, this fitful shower of eggs as proven, we at once see another way in which they may readily travel to distant lands. For it is quite possible that now and then they may fall on the cargo of an outgoing ship. Here they might lie safely in cracks and creases till, the journey being over, the knocking apart of packing-cases and the shaking of wrappers would set them afloat again, to drop down, it may be, into the Botanical Gardens of Sydney, the shore-pools of Ceylon, or the ponds of Jamaica. In fact, these Rotifera would have really done what I have already pointed out that they seemed to do—they would have followed the flag.

The eggs of the tube-makers, however, and of such Rotifera as live only in the clear waters of lakes and deep ponds, present a greater difficulty, for their eggs either lie within their tubes, or are attached to growing weeds, or fall down to a bottom which lies covered all the year round with several feet of water. The wind and sun here cannot be the only means of dispersion. Aquatic birds, and dogs, are probably assisting agents. The birds, as they swim among the water-plants, must frequently set free the eggs from the tubes of the Rhizota, as well as those which adhere to Confervæ, Potomagetons, and water-lilies, and so get them attached to their feathers. Then away they fly, carrying the eggs to some far distant lake, or shaking them off into the air with the flapping of their wings.

In confirmation of this idea, I may mention that the well-known naturalist, Mr. John Hood, of Dundee, who has added so many remarkable species of Rhizota to our Rotiferous fauna, informs me that the Scotch lakes most prolific in new and rare species are those which are visited annually by wild fowl from the North. Prof. Leidy also informs me that his collector, Mr. Seal, noticed sand-pipers haunting the duck-pond where he found an *Asplanchna* very similar to *ebbesbornii*, and that he thought that "these birds were especially instrumental in distributing the lower forms of aquatic life." I may also add that on one occasion I found in a temporary rain puddle, barely a yard across, a living ciliated ovum of *Plumatella repens*. Of course the puddle itself contained no adult forms, and the ovum must have been brought by some bird the distance of at least half a mile. The twin polypes were already partially developed inside the ovum, and it is curious that so delicate a thing should have borne this transport safely.

Dogs probably play a humbler part in the dispersion of the Rotifera; but they cannot help taking some part in it, by intercepting, as they swim, eggs that are slowly sinking

to the bottom, or by brushing off, on to their coats, eggs which have been already caught by the weeds; for the ephippial eggs are frequently armed with hooks or spines, which make them adhere easily to a pond-weed or to a hairy coat, and yet would not prevent a dog's vigorous shake, after his bath, from sending them flying into the air, or on to the dust, where sun and wind would do the rest.

Perhaps one of the most curious illustrations of this aërial conveyance of Rotiferous eggs is the account of *Callidina symbiotica*, which we owe to Dr. Carl Zelinka. It was in the depth of last winter that I read his interesting memoir, concerning a new *Callidina* that he had discovered inhabiting the little green cups on the under surfaces of the leaves of a scale-moss (*Frulliana dilatata*). As I knew that this plant grew on the elms of our Clifton promenade, I started off at once, on the rather forlorn hope of finding some living specimens of the new Rotiferon. When I arrived at the promenade I passed patch after patch of the scale-moss, hoping in vain to find something more promising than the withered liver-coloured stuff which alone was to be seen on the tree-trunks. At last I gave up further search, and pulling off a scrap of what looked like old ragged carpet, I carried it home. There I put a bit of it into a watch-glass, covered it with water, and gently teased it out with needles, till I found an under frond that had some pretension to being green. This I transferred to a glass cell, and placed it under the microscope with the cups turned towards me; and it was with no little pleasure that, in about a quarter of an hour, I saw first one *Callidina*, and then another, stretch its proboscis out of a cup, unfurl its wheels, and begin to feed.

No wonder that these *Philodinadæ* are to be found everywhere when they can bear to be frozen alive in the cell of a plant, or wasted by a midsummer sun in a leaden gutter!

Some chance breeze must have first wafted a *Callidina's* egg on to the scale-moss, just after a shower, when the whole plant was wet, and the little green cups were filled with water. The young *Callidina*, when hatched, could not have desired a better home. The rainfall, on an elm, flows down its furrowed bark in tracks as constant as those of a river and its tributaries; and the growth of the *Jungermann* follows these tracks. Every shower fills the spaces between its flat layers of overlapping leaves with water; and the lower layers, sheltered by the upper, retain for a long time water enough for the *Callidina* to creep about or swim in. And when, at last, the sun and air have dried up the water, the creature retreats into its green cup, which presents so small an aperture to the air, and is so fenced round with thick juicy cells, that the contained water is almost certain to hold out till the next shower. If it does not, the *Callidina* is still content; it becomes conscious of the coming crisis, draws in its head and foot, rounds its trunk into a ball, secretes round itself a gelatinous covering, and waits for better times.

But the Rotifera owe their wide dispersion not only to the ease with which their eggs are blown from one place to another, but also to their powers of endurance, and to their marvellous capacity for adapting themselves to new surroundings. A Philodine may say with Howell, "I came tumbling out into the world a true cosmopolite." I have already noticed how the *Philodinadæ* will endure such extremities of heat, cold, and dryness as Nature inflicts on them; but she does not put their full powers to the test, for, when time is given to them to don their protective coats, they can bear a heat gradually advancing to 200° F., or a fifty days' exposure to a dryness produced over sulphuric acid in the receiver of a good air-pump. Ehrenberg tells us that, whereas he killed *Volvox globator* with one electric shock, it took two of the same intensity to kill *Hydatina senta*; and that *Rotifer vulgaris* will swallow laudanum and yet "be lively," adding that a solution of cantharides seemed "to give it new life." The same irrepressible creature will flourish in water containing a perceptible quantity of sulphuric acid, while *Asplanchna priodonta* will swim about actively for twenty-four hours in a weak solution of salicylic acid, and *Synchæta pectinata* will do the same in chromic acid. The great majority of the fresh-water species die when dropped into sea-water, but some will bear sudden immersion in a mixture of one part sea-water to two fresh. We should not be surprised, therefore, to find not only that there are thirty-four known marine species of Rotifera, but that seventeen of these species are to be met with alike in salt-water and in fresh.

The following is the list of Rotifera found in salt or brackish water; those marked with a star are also the inhabitants of fresh-water:—

Floscularia campanulata*	Colurus amblytelus
Melicerta tubicolaria*	,, caudatus*
Rotifer citrinus*	,, dactylotus
Synchæta baltica	,, pedatus
,, tremula (?)*	,, uncinatus*
Pleurotrocha leptura (?)*	Mytilia tavina
Notommata naias*	Pterodina clypeata
Proales decipiens*	Brachionus Bakeri*
Furcularia forficula*	,, Mülleri
,, gracilis*	Notholca striata*
,, Reinhardti	,, spinifera
Diglena catellina*	,, inermis
,, grandis*	,, scapha*
Distemma raptor	,, thalassia
,, marinum	Anuræa valga*
Rattulus calyptus	,, biremis
Monostyla quadridentata	Hexarthra polyptera.

Although this is doubtless a very imperfect list, still it is sufficient to show how these fresh-water animals are slowly spreading into the tide-pools on the sea-shore. Some may have commenced their change of habitat in the field drains which are periodically invaded by the brackish waters of a tidal river. It was precisely in such a locality that I first found *Brachionus Mülleri*, in water only faintly salt, and at a height of 30 feet above the Severn. Ditches of this kind are to be found all down the Avon; from the highest point, that the tide reaches, to its mouth. As they approach the Severn their water becomes more and more brackish, and the preponderance of marine species in them more pronounced; so that it is easy to see how the descendants of a fresh-water Rotiferon, passing slowly down the river-side from ditch to ditch, may in course of many generations come to endure the sea itself.

In other cases the air-borne eggs may have dropped into the pools, of every degree of brackishness, which usually skirt the shores of our river estuaries. It is in such places, on the Scottish shore, that Mr. John Hood has found so many new marine species, and where no doubt so many more are yet to be found.

But the most noteworthy point about the above list is that the number of distinct genera is so great. One would rather have expected to find but four or five genera hardy enough to endure salt water; and yet here are no fewer than nineteen genera for the thirty-four known marine species; and of these latter, seventeen species are yet in the transitional state, inhabiting alike salt waters and fresh. Still more curious is it to find that all the four orders are represented; and that *Rhizota*, *Bdelloida*, and *Scirtopoda* have each furnished a contingent to the marine forms, as well as the more frequent *Ploima*. It is, of course, rather startling to hear that *Melicerta* and *Floscularia* are to be found inhabiting sea-water; but I know of no reason why any doubt should be thrown on Dr. Weisse's record of having so found them on the sea-shore at Hapsal.

The capacity of the Rotifera for adapting themselves to new surroundings is shown by a mere enumeration of the

strange places in which they are found. For these fresh-water creatures, the common inhabitants of lakes and ponds, are to be found in brackish ditches, sea-pools, the mud of ponds, the dust of gutters, in tufts of moss, on the blades of wet grass, in the rolled-up leaves and in the cups of liver-worts, in the cells of *Volvox*, the stems and sporangia of *Vaucheria*, in vegetable infusions; on the backs of *Entomostraca*, on their abdominal plates, on their branchial feet; on fresh-water fleas, wood-lice, shrimps, and worms; in the viscera of slugs, earth-worms, and Naiades; and in the body-cavities of *Synaptæ*.

But the great variability of every part of the external and internal structure of the Rotifera points to their fitness for playing the parts of cosmopolites. See how in *Floscularia* and *Stephanoceros* the head and its appendages are so developed that they dwarf all the rest; how in *Apsilus* the trunk predominates; while in *Actinurus* both head and trunk become appendages of a huge foot. The corona diminishes continually from the large complex organs of *Melicerta*, *Hydatina*, and *Brachionus*, down to the furred face of *Adineta* and the tuft of *Seison*, and vanishes altogether in *Acyclus*. The antennæ can be traced from long infolding or telescopic tubes, furnished with setiferous pistons, special muscles, and nerves, through a succession of shorter and simpler structures, till they become mere pimples or even setiferous pits in the body surface. The skin is hardened into a perfect lorica in *Brachionus*, is partially hardened in *Dapidia*, is merely tough in *Mastigocerca*, and is soft and quite unarmed in *Notommata*. The appendages of the body in Pedalion rise almost to the dignity of crustaceous limbs, for they have joints, and are worked by opposing pairs of muscles, passing across their cavities from point to point. In *Asplanchna* these appendages become stumpy projections, and the muscles, though still passing freely across the body-cavity, are reduced to threads. In *Triarthra* the appendages become chitinous spines; and at last, when we reach *Adineta*, *Taphrocampa*, and *Albertia*, we find that we have passed from a Rotiferon closely resembling a Nauplius-larva to one that is a simple worm.

The internal structure is just as plastic. The characteristic trophi exhibit a series of striking changes as we pass from one genus to another. In one direction the change is due to the degradation of the mallei, in the other to that of the incus; and in both this degradation is pushed so far, that the changing parts may be said almost to disappear. For in *Brachionus* and *Euchlanis* the mallei are well developed; in *Furcularia*, mere needle-shaped curved rods: in *Asplanchna*, so evanescent that it is hardly possible to find them in an animal killed by pressure.

By another set of changes, the rami are in their turn reduced almost to evanescence; becoming feeble loops in *Stephanoceros*, and in *Floscularia* two membranes attached to the unci.

Changes, great in degree, if not in variety, occur also in the excreto-respiratory system. For the contractile vesicle, which fills quite half the body-cavity in some *Asplanchnæ*, dwindles down in various species till it seems to vanish in *Pterodina* and *Pedalion*; while in one abnormal form, *Trochosphæra*, the connection between the lateral canals and the contractile vesicle is snapped, and the latter becomes an appendage of the cloaca only.

The nervous system, wherever it has been made out, is indeed always on the same plan; but its central organ, the nervous ganglion, is, in *Copeus* and *Euchlanis*, a great cylindrical sac stretching from the head below the mastax; while in *Floscularia* it shrinks into a small star-shaped body between the eyes and the organ of taste.

The alimentary and reproductive systems are those which vary the least; but even here the difference, in proportionate size, is very great between the stomachs of *Sacculus* and *Synchæta*, and also between the ovaries of *Asplanchnopus myrmeleo* and *Asplanchna priodonta*.

But not only do most of the external parts and internal organs vary in turn almost to vanishing, but these variations are not in any way simultaneous. The result is, that we find an organ, of a form characteristic of one family or genus, occurring in a species that belongs to another. Thus, for instance, the trophi of the *Melicertadæ* appear in *Pompholyx*, one of the *Triarthradæ*. Nay, more; it is easy to point out Rotifera that bear some striking characteristics of two or three other genera, or even of two or three other families. *Microdon clavus*, for example, has the central mouth and double ciliary wreaths of one of the *Rhizota*, the eye of a *Notommata*, the trophi of a *Diglena*, and the foot of a *Monostyla*. Again, *Pterodina patina* has the corona of *Philodina*, the lorica and transversely wrinkled retractile foot of *Brachionus*, the foot-ending of a young Rhizotan, and the mastax of the *Melicertadæ*. Then there is Mr. Thorpe's new Australian Floscule, which swims freely like one of the *Ploïma*, has the buccal cup and wreath of *Floscularia*, the dorsal eye of *Notommata*, and the body and forked foot of *Proales*.

To sum up, we may say that in the female Rotifera, the corona, head, foot, toes, appendages of the trunk, antennæ, eyes, and contractile vesicle vary down to almost absolute extinction; while, if we include the male in our survey, we must add that even the whole of the alimentary tract may disappear also. Moreover, the characteristics of the various groups interlace in so many ways that no organ—nor, indeed, any combination of two or three organs—can be relied upon to determine with certainty an animal's true position.

Two conclusions are, in consequence, irresistibly forced on us: the first, that the Rotifera, from *Pedalion* to *Albertia*, are related by descent; the second, that their curious habitats, wide dispersion and great variations in their structure are due to causes that have been at work for a very long period of time.

One other fact has also been made clear in this review—namely, that the British Rotifera give a very fair idea of the whole class. No doubt there are many foreign species, and some of these are very remarkable, and of great interest; but the greater number fall readily enough into the divisions that contain our own species.

And indeed it is a fortunate thing that we can here, at our own doors, study so many typical forms from life. For what books or drawings can give us the delight which we derive from observing the animals themselves?

To gaze into that wonderful world which lies in a drop of water, crossed by some atoms of green weed; to see transparent living mechanism at work, and to gain some idea of its modes of action; to watch a tiny speck that can sail through the prick of a needle's point; to see its crystal armour flashing with ever-varying tint, its head glorious with the halo of its quivering cilia; to see it gliding through the emerald stems, hunting for its food, snatching at its prey, fleeing from its enemy, chasing its mate (the fiercest of our passions blazing in an invisible speck); to see it whirling in a mad dance to the sound of its own music, the music of its happiness, the exquisite happiness of living,—can anyone, who has once enjoyed this sight, ever turn from it to mere books and drawings, without the sense that he has left all Fairyland behind him?

THE DARKNESS OF LONDON AIR.

A GREAT deal has been written at various times upon the subject of London fogs.

The constitution of these London fogs has been carefully gone into by several well-known men of science, from time to time; and the results obtained are of very great interest, as they prove, amongst other things, that during the winter London air has an unusually large amount of

carbonic acid in it. Dr. W. J. Russell found from experiments made in the City of London a few years ago, that on one day the carbonic acid had increased to 14·1 parts in 10,000 of air—that is, there was more than three and a half times the average amount present.

The question will naturally be asked, whether we cannot check this increase of carbonic acid in London air; and, in reply, it may be said that we can; partly by stopping the enormous volumes of sooty smoke belched out daily from hundreds of thousands of chimneys in the metropolis; and, partly, by having more open spaces, &c.

It is well known that the crowding of many people and animal life of all sorts, upon a small area of land, increases the production of carbonic acid, whether it be from respiration or from coal-burning; and it is almost indisputable that the latter produces the well-known black fogs and yellow fogs.

That these black fogs and yellow fogs—to which London and other large towns are liable in the winter—have a most injurious effect upon human beings and all animal life, and vegetation also, cannot be doubted, since we see the death-rate largely increased during the prevalence of black fogs; and of late years the more delicate plants, and indeed the strong ones, both at the Botanical Gardens, Regent's Park, and at Kew, have suffered much from the same cause.

Two things are to be briefly shown in this paper: (1) the general thickness and density of the atmosphere over London during the winter; and (2) the amount of artificial light used during the prevalence of black or dark yellow fogs in various parts of London.

SKETCH MAP OF A PORTION OF LONDON:—MEASURING POINTS SHOWN BY BLACK DOTS.

In connection with this subject various observations were taken in London during the winter of 1887–88, and the results are given below.

Before proceeding further, we may note that London was very free from fogs during the winter of 1887–88—a fact which was probably, to a certain extent, due to the light rainfall, and the unusual dryness, in consequence, of the great plain or valley of the Thames. This was the case not only in London, but in Manchester, Leeds, Liverpool, and elsewhere.

(1) As regards the general thickness and density of London air. The sketch plan of a portion of London will show that Primrose Hill—which is about 219 feet above the sea-level—was the point selected for (let us say) measuring from.

Three lines, which embraced the measuring points fixed upon, were taken over London, these lines being respectively the south-west line to St. Mary Abbot's Church, Kensington; the south line to the Clock Tower, Houses of Parliament; the south-east line to St. Paul's Cathedral. One more line was taken towards the country: this we will call the north line to St. John's Church, Hampstead. Intermediate measuring points were taken on all these lines, church spires or towers, &c., being selected as the most conspicuous objects that could be seen from Primrose Hill.

The distance of all these intermediate measuring points

from Primrose Hill, and their names, are given in Table I. The *total* number of times that each of the chosen points was seen during the five months selected is also given.

It will be observed that during the 152 days which make up the five months selected, Christ Church, Lancaster Gate, and St. Mary Abbot's Church, Kensington, on the south-west line; the Clock Tower, Houses of Parliament, on the south line, and the Scotch Church, Regent's Square; and St. Paul's Cathedral, on the south-east line, were *never once* seen.

When it is known that on any ordinary fine day during the late spring, summer, and early autumn, you can see right across London, on any one of the selected lines, it will be easy to realize how thick the air over London is during the winter.

It may be noted that when you could see as far as St. John's Church, Hampstead, it would, as a rule, have been possible to see much further, but since there was no point beyond Hampstead which could be taken as a measuring point, it was impossible to record the distance.

(2) As regards the amount of artificial light used in various parts of London owing to the prevalence of dark fog. The observations given below were made, with the assistance of various friends, during the winter of 1887-88, and give the approximate result in hours.

TABLE I.—*The distances given in this Table are all measured approximately from Primrose Hill.*

	South-west line to St. Mary Abbot's Church, Kensington.				South line to the Clock Tower, Houses of Parliament.				South-east line to St. Paul's Cathedral.					North line to St. John's Church, Hampstead.	
Distance from Primrose Hill.	½ mile.	¾ mile.	2 miles.	3 miles.	¼ mile.	¾ mile.	1¾ mile.	3 miles.	½ mile.	1 mile.	1½ mile.	1¾ mile.	3¼ miles.	¾ mile.	1½ mile.
Name of Measuring Point.	South end of Ormonde Terrace.	St. John's Chapel, Marylebone.	Christ Church, Lancaster Gate.	St. Mary Abbot's, Kensington.	North side of Zoological Gardens.	North side of Botanical Gardens.	All Souls' Church, Langham Place.	Clock Tower, Houses of Parliament.	St. Mark's Church, Regent's Park.	St. Matthew's Church, Oakley Square, N.W.	Clock Tower, St. Pancras Station.	Scotch Church, Regent's Square.	St. Paul's Cathedral.	St. Peter's Church, Belsize Park.	St. John's Church, Hampstead.
November 1887	11	13	0	0	10	14	0	0	19	6	0	0	0	12	9
December 1887	18	13	0	0	17	13	0	0	25	4	1	0	0	8	11
January 1888	18	8	0	0	17	9	1	0	25	1	0	0	0	6	2
February 1888	5	22	0	0	3	24	0	0	22	3	2	0	0	3	21
March 1888	31	31	0	0	31	30	1	0	30	1	0	0	0	8	23
Total number of times each point was seen... ...	83	87	0	0	78	90	2	0	121	15	3	0	0	37 [1]	66

On nine days you could not see 100 yards. On four days you could not see 5 yards.

A few districts out of many others are given in Table II.

TABLE II.—*Observations taken in London.*

District in London in which the register was kept.	1887.		1888.			Total number of hours during which artificial light was used.
	Nov.	Dec.	Jan.	Feb.	March.	
Fenchurch Street, E.C.	13½	1¾	29	0	3½	47¾
Southwark Street, S.E.	27¼	1½	20½	0	2¼	51½
Grosvenor Gardens, S.W.	17¾	0	35	0	2½	55¼
Oakley Square, N.W.	19½	5	13	0	3	40½
New Cavendish Street, W. ...	23½	2	26	0	1½	53
Notting Hill Gate, W.	19	0	15	0	2¼	36¼
Homerton, E. ...	34¼	24¼	38¼	4¾	11½	113

The manner in which the observations (given in Table II.) were made will be seen in detail from Table III., which was kept by Mr. E. Liddell at the College, Homerton, E.

This table is given because Homerton suffered more from darkness than any other part of London, owing, it cannot be doubted, to the large number of factories in the neighbourhood. It will be seen later on in this paper that Leeds suffered more from darkness than the other provincial towns selected, and this was due probably to a great extent to the same cause.

From Table II. it will be seen that January was the worst month for dark fogs, the average for each of the districts given being 27 hours of darkness per month.

London is not much worse than our large provincial towns in respect to dark fogs. Table IV. gives the results of observations made in several towns.

It will be seen from Table IV., that dark fog was general in the tabulated towns during the month of January. It is said that Manchester, of late years, has been unusually free from dark fogs, owing to the fact that a very large number of mills have been moved out, so as to escape the heavy town rates, &c. The ordinary white fog has also been reduced, probably through thousands of acres of the wet morass lands on the west side of Manchester having been well drained recently. This being

[1] On thirty-six days this point could not be seen.

the case, something might be done to improve the drainage of the marshes to the east of London.

Presuming that dark fogs are principally due to smoke, —and Sir Douglas Galton, in a paper read at the Parkes Museum in 1887, on "The Cause and Prevention of Smoke," declared that black (or dark) fog was *entirely* the result of smoke, while Dr. Marcet attributes the density of London fog *mainly* to smoke—it is clear that we want legislation to intervene, and to extend the Metropolitan Acts of Parliament, viz. those of 1853 and 1856, and instead of allowing these Acts to deal only *partially* with factory smoke, to cause them to be applied to every house in London.

TABLE III.—*Fog Observations from November* 1, 1887, *to March* 31, 1888.

NOTE.—Two spaces will be found below for every day during the five months. When, owing to the prevalence of fog, artificial light is used on a particular day, put a × in the *first* space assigned to that particular day; and in the *second* space put the quarters of an hour or number of whole hours and quarters during which light is used.

	November.		December.		January.		February.		March.	
	Denote days by ×.	Duration of time in hours.	Denote days by ×.	Duration of time in hours.	Denote days by ×.	Duration of time in hours.	Denote days by ×.	Duration of time in hours.	Denote days by ×.	Duration of time in hours.
1	...	...	×	1½						
2	...	...	...	...	...	...	×	4¾		
3	...	...	...	...	×	1				
4										
5										
6										
7										
8	...	...	×	2¼						
9	...	...	...	...	×	1				
10	...	...	×	2	×	8				
11	...	...	×	5½	×	8				
12	...	...	×	4	×	5½				
13	...	...	×	1½	×	4				
14	...	...	...	...	×	2¼				
15										
16	×	9¼								
17										
18	×	8								
19	×	2½								
20	×	1¼	...	...	×	4½				
21	×	6¼	...	...	×	2				
22	×	2¼	...	...	×	½				
23	...	...	...	...	...	...	...	...	×	6[1]
24	×	1½								
25										
26	...	...	...	...	...	...	...	...	×	1[2]
27	...	...	...	...	...	...	...	...	×	4½[3]
28	×	1								
29	×	2¼								
30	...	...	...	...	×	1				
31	...	...	×	7½						
		34¼		24¼		38¼		4¾		11½

Grand total = 113 hours.

TABLE IV.—*Observations taken in Provincial Towns.*

Name of Town in which the register was kept.	1887.		1888.			Total number of hours during which artificial light was used.
	Nov.	Dec.	Jan.	Feb.	March.	
Leeds	18½	12	47	4	0	81½
Liverpool	6½	8¼	26¼	3¾	0	44¾
Manchester ...	16	20	37	0	3	76

[1] Fog not dense on March 23, but very dark.
[2] Darkness, rather than fog, from 2.45 to 3.40.
[3] Fog and darkness.

It is not necessary to quote any figures here, to prove how the death-rate in London rapidly increases during the prevalence of smoke fogs, as everyone knows it too well. But we may give an extract from the *Gas World* to show the enormous and quite unnecessary cost of these smoke fogs.

The *Gas World* says that, "during the foggy days which were experienced between the 16th and 24th of November 1887, the Gas Light and Coke Company sent out to its customers in London no less than 710,251,000 cubic feet of gas; that to manufacture this quantity 71,000 tons of coal must have been carbonized, and that the total value of the gas, without the consideration of the by-products, is £106,000. During the nine days, therefore, the public paid the Gas Light and Coke Company no less than £490 per hour for *artificial light*."

This calculation, it should be observed, does not include the amount supplied by other Gas Companies in London during the same short period of fog.

W. HARGREAVES RAFFLES.

ELECTRICAL STRESS.[1]

THE subject of the discourse was brought before the members of the Royal Institution some years ago by Mr. Gordon. In the interval a considerable amount of work has been done upon it, both in England and Germany, and many experiments have been devised to illustrate it. Some of the more striking of these, though of great interest to the student, are rarely or never shown in courses of experimental lectures. The lecturer and Mr. C. V. Boys, F.R.S., last year devised a set of apparatus which has made the optical demonstration of electrical stress comparatively easy, and most of the results obtained by Kerr and Quincke can now be demonstrated to audiences of a considerable size. Before discussing this portion of his subject the lecturer introduced it by an explanation of principles on which the experiments are founded.

Magnetic lines of force can easily be mapped out by iron filings, but the exhibition of electrical lines of force in a liquid is a more complex matter. In the first place, if two oppositely electrified bodies are introduced into a liquid which is a fairly good non-conductor, convective conduction is set up. Streams of electrified liquid pass from the one to the other. The highly refracting liquid phenyl thiocarbamide appears to be specially suitable for experiments on this subject. If an electrified point is brought over the surface a dimple is formed which becomes deeper as the point approaches it. At the instant at which the needle touches the liquid the dimple disappears, but a bubble of air from the lower end frequently remains imprisoned in the vortex caused by the downward rush of the electrified liquid from the point. It oscillates a short distance below the point, and indicates clearly the rapid motions which are produced in the fluid in its neighbourhood. When the needle is withdrawn a small column of liquid adheres to it. This effect is, however, seen to greater advantage if a small sphere about 5 mm. in diameter is used instead of the needle-point. When this is withdrawn a column of liquid about 5 mm. high and 2 mm. in diameter is formed between the sphere and the surface. A similar experiment was made by Faraday on a much

[1] Abstract of a Lecture delivered at the Royal Institution on February 15, by Prof. A. W. Rücker, F.R.S.

larger scale with oil of turpentine, and he detected the existence of currents, which are in accord with the view that the unelectrified liquid flows up the exterior of the cylinder, becomes electrified by contact, and is repelled down its axis. In view of this explanation, and the movements assumed can be clearly seen in the phenyl thiocarbamide, the performance of the experiment on a small scale is not without interest. The possibility of the formation of such violent up-and-down currents in so small a space must depend upon a very nice adjustment between the properties of the liquid and the forces in play. It is obvious that such movements of the liquid must be a disturbing element in any attempt to make the lines of electric force visible.

Again, if a solid powder be suspended in a liquid into which electrified solids are introduced, it tends to accumulate round one of the poles. This subject has been investigated by W. Holtz. Sometimes the powder appears to move in a direction opposed to that in which the liquid is streaming. Sometimes two powders will travel towards different poles.

If powdered antimony sulphide be placed in ether, it settles at the bottom of the liquid, and if either two wires insulated with glass up to their points, or two vertical plates be used as electrodes, on exciting them slightly the solid particles arrange themselves along the lines of force. If the electrification be increased, they cluster round the positive pole. On suddenly reversing the electrification by means of a commutator, they stream along lines of force to the pole from which they were previously repelled. Other methods of obtaining the lines of force have been devised. They can, for instance, be shown by crystals of sulphate of quinine immersed in turpentine.

The tendency of the lines of force to separate one from the other was illustrated by Quincke's experiment. A bubble of air is formed in bisulphide of carbon between two horizontal plates. It is in connection with a small manometer, and when the plates are oppositely excited, the electrical pressure acting at right angles to the lines of force, being greater in the liquid than in air, compels the bubble to contract.

Kerr's experiments depend upon the fact that, since the electrical stress is a tension along the lines of force, and a pressure at right angles to them, a substance in which such a stress is produced assumes a semicrystalline condition in the sense that its properties along, and perpendicular to, the lines of force are different. Light is therefore transmitted with different velocities according as the direction of vibrations coincides with, or is perpendicular to, these lines; and the familar phenomena of the passage of polarized light through crystals may be imitated by an electrically stressed liquid.

The bisulphide of carbon used must be dry, and, to make the phenomena clearly visible, it is necessary that the light should travel through a considerable thickness. Thus, to represent the stress between two spheres, elongated parallel cylinders should be used, the axes of which are parallel to the course of the rays of light. These appear on the screen as two dark circles. Between crossed Nicols, the planes of polarization of which are inclined at 45° to the horizontal, the field is dark until the cylinders are electrified, when light is restored in the space between them.

If parallel plates with carefully rounded edges, and about 2 millimetres apart, are used, the colours of Newton's rings appear in turn, the red of the third order being sometimes reached. If one plate is convex towards the other, the colours of the higher orders appear in the middle, and travel outwards as the stress is increased. The experiments may be varied by using two concentric cylinders, or two sheets of metal bent twice at right angles to represent a section through a Leyden jar. In the first case a black cross is formed; and in the second, black brushes unite the lower angles of the images of the edges of the plates. By the interposition of a piece of selenite, which shows the blue of the second order, two of the quadrants contained between the arms of the cross become green, and the others red. In like manner the horizontal and vertical spaces between the inner and outer coatings of the "jar" become differently coloured.

There are several phenomena connected with the stress in insulators which present considerable difficulties. Thus in a solid it is found impossible to restore the light between crossed Nicols by a uniform electrical field. That the non-uniformity of the field has nothing to do with the phenomenon in liquids, though at first disputed, is now generally admitted. It may be readily proved by means of a Franklin's pane, of which half is pierced into windows. The glow is much weakened by thus removing part of the uniform field, though it is thus made much less uniform.

Again, though most dielectrics when placed in an electric field expand, the fatty oils contract. Prof. J. J. Thomson has recently pointed out that this indicates that another set of strains are superposed upon those assumed in the ordinary explanations of these phenomena, and by which they may be neutralized or overcome.

In experiments with carbon bisulphide it is necessary to take every precaution against fire. For this purpose the cell which contains the liquid should be immersed in a larger cell, so that if—as sometimes happens—the passage of a spark cracks the glass the liquid may flow into a confined space. This should stand in a tray with turned-up edges, and an extinguisher of tin plate should be at hand to place over the whole apparatus. No Leyden jars should be included in the electrical circuit. The difficulties which formerly arose in the exhibition of experiments in statical electricity owing to the presence of moisture in the air of a lecture-room are now immensely reduced by the Wimshurst machine, which works with unfailing certainty under adverse conditions. A new and very beautiful machine was kindly lent by Mr. Wimshurst for the purposes of the lecture.

NEW BUILDINGS AT CAMBRIDGE FOR PHYSIOLOGY AND ANATOMY.

THE energy and success of the Cambridge teachers of science are once more demonstrated by the proposal to build new laboratories, with a large lecture-room, for anatomy and physiology, and a museum and dissecting-room for human anatomy, on a scale commensurate with the importance of the medical and biological school. The present physiological laboratories, which ten years ago were a great advance upon the mere make-shift arrangements that had previously done duty, are now disagreeably overcrowded. At present, Prof. Foster's elementary class is attended by between 190 and 200 students; and the several advanced classes have from twenty to thirty-five students. In the laboratory there are now only places for ninety students of histology; but accommodation has been provided for about seventy more in a temporary building attached to the museum. Inasmuch as the students of the elementary class must all go through the histological course, lasting throughout three terms, it is evident that they can be accommodated only by relays, and that in order to accommodate the advanced students, who have no proper places of their own, much crowding must take place, whereas the advanced students' work-places ought not to be disturbed, as these students need opportunities for continuous work. For chemical physiology there are only eight places available, and there is one fairly large room for physical physiology; there is no adequate lecture-room.

The proposed buildings have once been deferred, plans having been prepared in 1884; but it is hoped that the

delay may have led to the presentation of a better and more complete plan. The details of the scheme would be too long for us to give; but the result will be to provide an excellent new building extending for 190 feet along Corn Exchange Street, in continuation of the east front of the present buildings for physiology and comparative anatomy, and occupying the whole distance between them and the Corn Exchange. Besides rooms for teachers and demonstrators, aquaria, and preparation-rooms, there will be a new class-room in which 140 additional students for histology can be accommodated. There will be a demonstration-room in which about fifty students at a time can be shown experiments which now have to be omitted owing to the want of such a room. By rearrangement of rooms additional accommodation will be given to chemical physiology and to research, while rooms will be available for advanced students to work without interruption from the elementary classes.

The new lecture-room will be in the middle block, between the anatomical and physiological buildings; internally it is to measure 40 by 45 feet, by 25 feet high to the wall-plate, above which will be an open queen-post trussed roof, with sky-lights in the sides. There will also be a large window in the east gable. This lecture-room will accommodate 240 students, for more than which number it is not yet considered necessary to provide; although if the school continues to expand till it reaches the dimensions of the Edinburgh School, which is not impossible, a still larger lecture-room will ultimately be required. But the present proposal will give a room far superior to the room now in use, both for anatomy and physiology.

The northern block, for human anatomy, has about 70 feet of frontage, and contains, in addition to offices, Professor's and articulating-rooms, &c., a museum 40 feet by 60, lighted by windows in three walls, and 17 feet high, admitting of the construction of a gallery. Above the museum is a dissecting-room of rather larger area, well lighted.

The estimated cost is, for physiology, £4755; lecture-room, £3338; human anatomy, £5872: total, £13,965. The report and plans are to be discussed on Saturday next, and we hope they will be promptly carried out, as the anatomical buildings at present in use are painfully inadequate, and physiology is also urgently in need of better accommodation.

NOTES.

THE subject of the Croonian Lecture to be delivered before the Royal Society during the present year will be "Preventive Inoculation." The lecture will be delivered by M. Roux, and will be founded on observations made in the Pasteur Institute. It is hoped that M. Pasteur will be present at the lecture.

MR. EADWARD MUYBRIDGE, of Philadelphia, who by arrangement with the Managers of the Royal Institution had agreed to give a discourse after Easter on "The Science of Animal Locomotion in its Relation to Design in Art" (illustrated by the zoopraxiscope), a subject of great novelty and interest, has kindly consented to deliver it on Friday evening, the 22nd instant, Dr. Edgar Crookshank being compelled, through illness, to defer his discourse on "Microbes," which was to have been delivered on that evening.

To meet the expressed wish of the members, the Council of the Mineralogical Society has resolved that two additional general meetings shall be held in London during the current year; the first has been fixed for Tuesday, March 12, and the other for Tuesday, June 25. The general meetings still to be held in London during the year will thus be on the following Tuesdays: March 12, May 7, June 25, November 5 (anniversary). The meetings will be held on the premises of the Geological Society, Burlington House, Piccadilly, at 8 p.m.

ON Monday, March 11, Mr. William Jago will begin, at the City and Guilds of London Institute, a course of ten lectures on "Bread-making." The lectures will be delivered on Monday and Thursday evenings at 7.30. The special object of the course is to give, in the simplest possible manner, instruction to practical working bakers as to the nature of the changes which occur during the manufacture of bread.

IN spite of the enthusiasm evoked in Norway by the success of the Nansen Expedition, the national subscription opened to defray the cost has been but poorly responded to. In consequence, Herr Gamél, of Copenhagen, whose munificence enabled the Expedition to start at all, has offered to contribute the balance wanting.

WE regret to have to record the death of the Rev. John George Wood, author of "Common Objects of the Sea-shore" and many other popular works on natural history. He died on Sunday, while on a visit to Coventry, from an attack of peritonitis. Mr. Wood was in his sixty-second year.

THE death is announced of Dr. Johannes Brock, lately Professor of Zoology at Dorpat University. He was well known by his scientific journey to the Indian Archipelago, undertaken with the pecuniary help of the Berlin Academy. He died at Göttingen, where he had been appointed Professor of Natural Science.

DR. J. SOYKA, Professor at the German University at Prague, and formerly at the University of Munich, shot himself during a fit of melancholia, on February 23. He was the author of works on Bacteria.

LAST week, in answer to a question put by Mr. Mundella, with regard to the aid to be granted by the Government to provincial Colleges, the Chancellor of the Exchequer made the following statement:—"A vote for provincial Colleges has been put down in the Estimates for 1889–90. The Government have found considerable difficulty in deciding what Colleges should be entitled to share in it, and in what proportions and on what conditions it should be distributed between them. They have accordingly appointed a small Committee to make particular inquiries and advise them on these points. The Committee will sit at an early date, and its deliberations are not likely to be prolonged. Upon receiving its report the Government will settle the scheme of distribution. The sum voted will, of course, be available for the Colleges which are entitled to share in it during the coming financial year."

THE Owens College is one of the Manchester institutions which benefit by the will of the late Mr. John Rylands. He has bequeathed to it £10,000.

SOME time ago the Coast Fishing Section of the German Fisheries Society established a zoological station at Ditzum, on the Dollart, where researches on the fauna of the German Ocean were carried on during the summer months. The Society are now making arrangements to keep the station open during the whole year.

A BIOLOGICAL STATION, chiefly for the promotion of the fisheries, is to be established in Denmark, at a cost of £2000, with a yearly subsidy of £480.

THE Fisheries Exhibition which has just been opened in St. Petersburg is the first Exhibition of this kind that has been held there. It will remain open till the end of April.

ON February 20, about 10 p.m., a remarkably brilliant meteor was seen in and around Stavanger, on the west coast of Norway. It radiated in the south-east, and, going in a westerly direction, burst about 35° above the horizon, without any detonation, but leaving a long trail behind. Its light was a dazzling white.

On January 22, about 2 a.m., an earthquake shock was felt at Hönefos, in Central Norway. This was followed by another, and by a few more at intervals, but faint in character.

A number of houses were destroyed by the earthquake that occurred at Fleurier, in the Jura Mountains, on February 13 last.

In the *American Meteorological Journal* for December last, General Greely, Chief Signal Officer, contributes an interesting article on "Average Velocities of Low-area Storms and Upper Air Currents in the United States." The author shows that the decrease in velocity of the former is regular and unbroken from February to June, and that the increase is nearly as regular to February again. He expresses the important opinion that the average movement of low-area storms in the United States bears a definite relation to the velocity of upper air currents; and in support of this, a chart is given showing a remarkable accord between the mean hourly velocity of low-area storms and the mean velocity of the upper air currents from 1881–87. Prof. M. W. Harrington contributes a useful article in the shape of a translation of a simple demonstration of the deflection of horizontal motion due to the earth's rotation, from Dr. Günther's "Lehrbuch der Geophysik," without the use of higher mathematics. In the January number, Mr. A. L. Rotch gives a very complete account of the organization of the meteorological service in France since the first establishment of weather telegraphs by Leverrier at the Paris Observatory in 1856, together with descriptions of the instruments and methods now employed. Since 1887 the meteorological service has been separated from the astronomical work, and has been under the able direction of Prof. E. Mascart. The observing-station of the Central Office is the Parc St. Maur Observatory, nearly ten miles south-east of Paris.

In the latest Report of the Bombay Chamber of Commerce, it is stated that it was decided by the Government last May to abolish the office of Meteorological Reporter of Western India, and substitute for it that of Reporter at a reduced salary, who would work under and through the Meteorological Office at Simla. The Chamber, thinking that this alteration would be detrimental to the shipping in the port by stopping the system of storm-warnings that had been carried on for some years, petitioned the Government against the proposed change, and advocated the establishment of additional coast and inland signal stations to aid in the daily forecasts. The Chamber also pointed out in its petition that its members have, for a number of years, printed bi-weekly weather reports, which were indispensable in the absence of a Government daily weather chart and report. The Superintendent of the Meteorological Department of the Government of India was sent to Bombay to make inquiries, but his visit only resulted in the closing of the office in the month of August. Since that time the head of the Telegraph Department has superintended the forecasts. The scheme now about to be tried is that, in consideration of a small monthly payment by the Chamber of Commerce and the Port Trust, a daily telegraphic weather report chart and storm warnings and a bi-weekly crop report will be supplied to each member of the Chamber.

Mr. R. Andree has lately been collecting information as to the use of signals by primitive peoples, and the facts he has brought together—summarized in *Science*—are interesting and suggestive. American Indians use rising smoke to give signals to distant friends. A small fire is started, and, as soon as it burns fairly well, grass and leaves are heaped on the top of it. Thus a large column of steam and smoke rises. By covering the fire with a blanket, the Indians interrupt the rising of the smoke at regular intervals, and the successive clouds are used for conveying messages. Recently, attention has been called to the elaborate system of drum-signals used by the Cameroon negroes, by means of which long messages are sent from village to village. Explorations in the Congo basin have shown that this system prevails throughout Central Africa. The Bakuba use large wooden drums, on which different tones are produced by two drum-sticks. Sometimes the natives "converse" in this way for hours; and, from the energy displayed by the drummers, and the rapidity of the successive blows, it seemed that the conversation was very animated. The Galla south of Abyssinia have drums stationed at certain points of the roads leading to the neighbouring States. Special watchmen are appointed, who have to beat the drum on the approach of enemies. Cecchi, who observed this custom, designates it as a "system of telegraphs." The same use of drums is found in New Guinea. From the rhythm and rapidity of the blows, the natives know at once whether an attack, a death, or a festival is announced. The same tribes use columns of smoke or (at night) fires to convey messages to distant friends. The latter are also used in Australia. Columns of smoke of different forms are used for signals by the inhabitants of Cape York and the neighbouring island. In Victoria, hollow trees are filled with fresh leaves, which are lighted. The signals thus made are understood by friends. In Eastern Australia, the movements of a traveller were made known by columns of smoke, and so was the discovery of a whale in Portland Bay.

A very important series of vapour-density determinations have been made by M. Alphonse Combes, which appear to decide the much-discussed question of the valency of aluminium. It will be remembered that a few months ago, as noticed in these columns (vol. xxxviii. p. 624), Profs. Nilson and Pettersson, of Stockholm, published the results of a most conclusive series of experiments upon chromic chloride, showing that at the lowest available temperature the density of the chloride corresponds so closely to the formula $CrCl_3$, as to preclude the possibility of the existence of molecules of Cr_2Cl_6 in the gaseous state. This decisive result in the case of chromium, following as it did after the experiments of Prof. Victor Meyer and Dr. Grünewald upon ferric chloride, which also resulted in showing that the formula $FeCl_3$ represented the only stable molecular condition, appeared to indicate that the metals of this group are really triads, and that the double formulæ Cr_2Cl_6 and Fe_2Cl_6 must be abandoned. This conclusion was further strengthened by the fact that still earlier determinations of the vapour-densities of the chlorides of indium and gallium by Prof. Meyer, Profs. Nilson and Pettersson, and M. Friedel, had yielded conclusive results, pointing to the formulæ $InCl_3$ and $GaCl_3$. In the case of aluminium, however, the evidence has been by no means so decisive. As shown by Dr. Young, in an admirable *résumé* of all the experimental data bearing upon this question in Nature (vol. xxxix. p. 198), determinations of the vapour-density of aluminium chloride by Profs. Nilson and Pettersson showed that from 440° C. the density gradually diminished, until at about 800° it arrived at the value corresponding to $AlCl_3$, and then remained constant for about 200°, until, in fact, it began to break up with liberation of free chlorine. On the other hand, Messrs. Friedel and Crafts, in a beautifully graduated series of experiments, found that at 218°, 31° above the boiling point, the density corresponded almost exactly to the formula Al_2Cl_6, and remained practically constant to 400°. More recently, Messrs. Roux and Louïse have found that, at temperatures near their boiling-points, methide and ethide of aluminium possess densities corresponding to the formulæ $Al_2(CH_3)_6$ and $Al_2(C_2H_5)_6$; these values, however, do not remain constant for any sufficient interval of temperature, and so are by no means conclusive. At this interesting moment M. Combes brings forward his experiments upon a new compound, acetyl acetonate of aluminium, $[Al(C_5H_7O_2)_3]_n$, a

substance readily obtained perfectly pure as a white crystalline solid, melting at 193°–194°, and vaporizing unchanged at 314°–315°. Two consecutive density determinations by Victor Meyer's method in an atmosphere of nitrogen and at the temperature of boiling mercury, which is only 45° above the boiling-point of the substance, yielded numbers corresponding to the molecular weights 325·5 and 324·2. The molecular weight of $Al(C_5H_7O_2)_3$ is 324·5; so that, even at this comparatively low temperature, just as in the case of chromium chloride, the triad is the only possible formula. There was no trace of decomposition, the pure white crystals being found re-formed and chemically unchanged after the experiment. From this result it appears pretty conclusive that aluminium does behave like chromium, iron, indium, and gallium, and that, although in the case of the chloride, molecules of the composition Al_2Cl_6 may for a brief space exist, yet the most stable molecules of aluminium compounds in general are those in which the metal plays the part of a triad.

IN some notes on a voyage to the Greenland Sea in 1888, published in the *Zoologist* for March, Mr. Robert Gray gives some curious particulars with regard to the contents of the stomachs of several hooded seals, *Cystophora cristata*, shot on July 9. While most were empty, one was packed full with a bluish mud or ooze, in which were embedded the crystalline lenses of two eyes belonging probably to some small species of fish, and the remains of one Crustacean common at the surface (*Themisto*). The stomachs of three other seals contained mud alone. "With regard to the presence of mud in these animals' stomachs," says Mr. Gray, "while considering the depth of the water too great (in this instance 200 fathoms; in another, 1100) to permit the bottom being reached, the only explanation I am able to offer is that the substance must be swallowed in small quantities by the seals along with their ordinary food (Crustaceans living at the surface), and that, owing to its indigestible nature, it accumulates in course of time in the stomach. These seals are occasionally observed disappearing under the ice, for the purpose, I believe, of feeding on the immense number of Crustaceans which are known to accumulate there. Many of the ice-fields bear on their surface, immediately under a superficial coating of snow, cargoes of mud (apparently of an alluvial origin). During the process of melting, the mud may accumulate on submerged tongues or ledges of the ice, and thus become the retreat of numbers of Crustaceans, which, as they are devoured by the seals, are swallowed along with a small quantity of the mud. Some such explanation must, I think, be conceived."

A PAPER on the occurrence of Pallas's sand-grouse (*Syrrhaptes paradoxus*) in Ireland was read some time ago before the Royal Dublin Society, by Dr. Robert Scharff, and has now been printed. The immigrations of the bird began in Ireland at the end of May and lasted to the middle of July, when they ceased until the end of November. It is difficult to say, with any degree of accuracy, how many specimens found their way to Ireland; but Dr. Scharff thinks that in the various flocks which were seen there may have been about one hundred birds. A far larger number, however, may not have been observed. "Following their instinctive desire to explore the extreme west," says Dr. Scharff, "hundreds may have perished in the waves of the Atlantic, thus putting a stop to their enterprising spirit."

AN interesting paper on the Coreans was read lately by Mrs. E. R. Scidmore, an American lady who, in 1887, visited, as a guest, Judge Denny, the foreign adviser to the King of Corea. Wisps of straw and bits of cloth hang at the doorways to keep off evil spirits; and these, according to Mrs. Scidmore, are the only signs of worship seen about Seoul. The Coreans have the worship of ancestors, as the Chinese; and a trace of the old dragon-worship must order their toleration of snakes, as it is impossible to get a Corean servant to kill the snakes that drop from the mud roof and slip out of the flues of the kaugs that warm the floors of the houses. Until the arrival of the American physicians, the king and queen had an army of necromancers and wizards in attendance upon them, and a form of Shamanism was practised upon the sick. They were consulted also in matters of State policy.

A SUPPLEMENT to the first volume of the *Internationales Archiv für Ethnographie* has been issued. It consists of a careful and interesting monograph, by Dr. Otto Stoll, on the ethnology of the Indian tribes of Guatemala. He has much that is valuable and interesting to say about the social organization of these tribes, their religious ideas and customs, and their skill in various industrial arts. Two admirably coloured plates accompany the essay, and three illustrations are given in the text.

THE January and February numbers of *Mathesis*, a Belgian mathematical journal, have just been issued as a single number of seventy pages, which are wholly taken up with a French translation of the "supplementary chapter" of Dr. Casey's "Sequel to Euclid" (pp. 165–248, fifth edition). With the double part the editors present a copy of M. Vigarié's useful "Premier Inventaire de la Géométrie du Triangle," to which we lately drew attention.

THE proprietors of the Castle Mail steamers have issued a guide "to the land of gold and diamonds, and the places touched at by their various steamers." The book is called "South Africa, and how to reach it by the Castle Line." The author is Mr. Edward P. Mathers.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Miss L. C. Hart; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mrs. Margaret Allison; an American Black Bear (*Ursus americanus* ♂) from North America, presented by Messrs. Hugh Williams and Basevi, Lieutenants R.N.; a Common Fox (*Canis vulpes*), British, presented by the Lord Tredegar; a Short-eared Owl (*Asio brachyotus*), captured at sea, presented by Mr. R. Phillips; a Common Blue Bird (*Sialia wilsoni*) from North America, presented by Commander W. M. Latham, R.N.; an Axolotl (*Siredon mexicanus*) from Mexico, presented by Mr. E. Evelyn Barron; nine Moorish Geckos (*Tarentola mauritanica*) from the South of France, presented by Masters F. and O. Warburg; a Manatee (*Manatus australis*) from Guiana, deposited; two Tui Parrakeets (*Brotogerys tui*) from Brazil, four White-breasted Gallinules (*Gallinula phœnicura*) from India, purchased; an Unarmed Trionyx (*Trionyx muticus*) from North America, received in exchange.

OUR ASTRONOMICAL COLUMN.

SOLAR ACTIVITY IN 1888.—The behaviour of the various orders of solar phenomena, spots, faculæ, and prominences, during the past year has shown most conclusively that the minimum must now be very near at hand, and it may with confidence be expected to fall either towards the end of the current year or else early in 1890. The spots especially have shown unmistakable signs that the trough of the eleven-year curve is nearly reached, for they have been few in number, small in size, and low in latitude, and there have frequently been considerable intervals in which no spots have been seen at all. The remarkable depression of October 31 to December 9, 1886 (see NATURE, vol. xxxv. p. 445) has in some respects indeed not been equalled during 1888, but there has been no such long period of unbroken quiescence since the minimum of 1879 as that recorded in last October, when in the three weeks October 4–24 not a single spot was seen, whilst there were but three days showing spots in the thirty-seven from September 29 to November 5. Other spotless or nearly spotless periods in 1888 were January 23–30, February 4–17, March 1–8, March 24–31, April 6–15, April 30 to May 10,

May 24 to June 8, June 30 to July 12, July 18 to August 7, and December 22–31. And not only were there these long and numerous breaks in the spot manifestations, but when spots were seen they were almost always small in size and few in number. On not a single day in the year did the total spotted area amount to $\frac{1}{1200}$ of the surface of the visible hemisphere; on only eight days did it exceed $\frac{1}{2000}$. The mean daily spotted area for the year amounted only to about 9 parts in 100,000, or almost precisely the same as in 1877. In January there was a feeble but fairly sustained display of activity up to the 22nd; there was a similar but less lasting manifestation at the end of February, and again about the middle of March. April was very quiet, but May 11–23 yielded a fair show of spots, May 14 giving the largest area of the year. August 28 to September 9 was also a fairly active time; but the most prolific month as to entire spotted area, though not as to number of spots, was November, following immediately after the longest period of entire quiescence. The last ten days of last year, and the first two months of the present, have been exceedingly barren.

A rough tendency has manifested itself in the past as in previous years, for quiet intervals to follow each other at the distance of half a synodic rotation of the sun, indicating a preference of the spots for a few favoured longitudes. In latitude, the spots have continued to be more numerous in the southern hemisphere—a condition of things that has prevailed, on the whole, ever since the dying away of the great spot of November 1882. Generally speaking, the spots have lain close to the equator, not often rising above 5° or 6° of latitude in the northern hemisphere, and 9° or 10° in the southern; but the same curious pulsation shown in the great eleven-year cycle has been also visible in these minor oscillations, and whenever there has been anything like an outburst, there has also been an effort to ascend to higher latitudes. Thus, the greatest display in the northern hemisphere, that of November, lay in lat. + 11°; whilst a part of the outbreak in the southern hemisphere in September reached lat. − 16°.

The monthly numbers for spots and faculæ given by Prof. Tacchini, in the *Comptes rendus*, vol. cvi. No. 18, vol. cvii. No. 6, and vol. cviii. No. 7, are as follows, and may be compared with those given for previous years in NATURE, vol. xxxiii. p. 398, vol. xxxv. p. 445, and vol. xxxvii. p. 423:—

	Relative frequency of days without spots.	Sun-spots. Relative frequency.	Relative size.	Mean daily number of groups.	Faculæ. Relative size.
January	0·21	2·70	11·17	1·30	14·13
February	0·74	2·30	5·91	0·48	11·09
March	0·61	1·70	6·22	0·48	14·57
April	0·39	1·65	4·31	0·89	13·65
May	0·54	2·50	18·77	0·46	7·20
June	0·42	3·71	4·18	0·79	17·52
July	0·68	1·10	3·45	0·42	15·81
August	0·46	1·86	13·71	0·68	14·29
September	0·04	4·80	40·12	1·68	27·80
October	0·80	0·68	1·12	0·08	5·72
November	0·41	3·12	21·88	0·77	9·12
December	0·44	2·44	10·64	0·56	10·22

The foregoing table shows that the faculæ have not by any means varied simultaneously with the spots, and that their diminution as compared with 1886 and 1887 has been but slight. They showed, however, a very noticeable development during the secondary maximum of September, whilst the prominences, on the other hand, fell off considerably both in September and November, but attained their greatest development during the year about March and April, when the spot activity was decidedly feeble. This diversity of behaviour shows that the connection between the spots and hydrogen prominences is less intimate than it has sometimes been stated to be. Nevertheless, the prominences also afford very distinct evidence of a continued decline, as the following figures, given by the Rev. S. J. Perry in the *Observatory* for March, will show:—

	Mean height of chromosphere. ″	Mean height of prominences. ″	Mean extent of prominence arc. ° ′
1886	8·05	24·78	13 36
1887	8·13	23·86	9 29
1888	8·06	20·96	6 46

The highest individual prominences recorded by Prof. Tacchini were 2′ in height, and were seen on January 10 and February 7.

The magnetic variation has also shown a decline during the year in fairly faithful parallelism to the sun-spots; indeed, the accordance has, according to Dr. R. Wolf, been closer in 1888 than in 1887. The following are the numbers he gives in the *Comptes rendus*, vol. cviii. No. 2:—

1888.	Wolf's relative numbers (Zurich). r.	Δr.	Variation in magnetic declination (Milan). v. ′	Δv. ′
January	13·0	− 0·1	3·03	−0·68
February	7·0	− 8·7	3·02	−0·67
March	6·3	+ 3·6	7·11	+0·12
April	3·9	− 3·6	8·27	−1·06
May	7·8	− 9·4	8·48	−0·82
June	6·5	− 9·8	9·27	−0·28
July	1·9	−24·3	8·58	−1·67
August	1·9	−19·2	9·17	+0·10
September	7·8	+ 0·9	7·31	+1·23
October	2·0	− 3·5	6·32	+0·29
November	12·9	+ 8·4	2·18	−0·89
December	9·9	−10·6	1·76	−0·47
Mean	6·7	− 6·4	6·21	−0·40

The formula $v = 5'·62 + 0·045r$, which Dr. Wolf has established for Milan, would give $v = 5'·92$ if $r = 6·7$, whilst the observed value of v was $6'·21$, a difference of $0'·29$. The difference between the observed and computed values was $0'·40$ for 1887.

COMET 1889 *a*.—This object, discovered by Mr. Brooks on January 14, appears to be lost. Prof. Holden, writing on January 30 to the editor of the *Astronomische Nachrichten*, states that both Mr. Barnard and Prof. Swift had carefully searched for it with the Lick instruments, but without success.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *MARCH* 10–16.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 10

Sun rises, 6h. 26m.; souths, 12h. 10m. 21·6s.; sets, 17h. 54m.: right asc. on meridian, 23h. 23·8m.; decl. 3° 54′ S. Sidereal Time at Sunset, 5h. 8m.

Moon (at First Quarter on March 9, 18h.) rises, 10h. 41m.; souths, 18h. 53m.; sets, 3h. 9m.*: right asc. on meridian, 6h. 7·2m.; decl. 21° 54′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury	5 39	10 28	15 17	21 40·7	14 15 S.
Venus	7 18	14 52	22 26	2 5·6	16 54 N.
Mars	7 12	13 46	20 20	0 59·9	6 4 N.
Jupiter	3 14	7 10	11 6	18 22·5	23 2 S.
Saturn	14 16	21 54	5 32*	9 9·2	17 38 N.
Uranus	20 43*	2 8	7 33	13 19·3	7 40 S.
Neptune	8 55	16 38	0 21*	3 51·8	18 30 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Mar.	h.	
13	11	Mercury at greatest elongation from the Sun, 28° west.
14	6	Saturn in conjunction with and 1° 0′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
W Tauri	4 21·7	15 51 N.	Mar. 12,		*M*
R Canis Majoris	7 14·5	16 11 S.	,, 11,	19 57	*m*
			and at intervals of	27 16	
S Geminorum	7 36·4	23 43 N.	Mar. 14,		*M*
S Cancri	8 37·6	19 26 N.	,, 11,	18 38	*m*
T Cancri	8 50·3	20 16 N.	,, 14,		*M*
R Ursæ Majoris	10 36·8	69 22 N.	,, 15,		*m*
R Crateris	10 55·1	17 44 S.	,, 16,		*m*
U Coronæ	15 13·7	32 3 N.	,, 11,	0 32	*m*
S Serpentis	15 16·5	14 43 N.	,, 11,		*M*
T Vulpeculæ	20 46·8	27 50 N.	,, 10,	21 0	*m*
			,, 11,	23 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 10,	5 40	*m*
			,, 13,	5 40	*m*
δ Cephei	22 25·0	57 51 N.	,, 10,	20 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Between Lynx and Auriga	98° ...	46° N.	
Near ν Virginis	175 ...	10 N. ...	Slow; bright.
,, κ Cephei	300 ...	80 N. ...	Slow; bright.

GEOGRAPHICAL NOTES.

M. ROLLAND, a French naturalist, charged with an official mission to Madagascar, has sent in his Report to the Minister of Public Instruction. M. Rolland sums up his geographical observations by remarking that, notwithstanding its apparently simple contour, the topography of Madagascar is exceedingly complex. Behind the line of lagoons which border the coast, and which, except that the water is salt, remind one of the *étangs* of Languedoc, the hills begin to rise, and increase in height towards the interior. Behind these, again, the mountains rise by stages to a height of over 6500 feet. The surface is cut up by innumerable ravines, at the bottom of which are torrents, which rush on their way towards the Indian Ocean. This chain forms the backbone of the island, and consists mainly of Primary and crystalline rocks. When it is crossed, the Mozambique Channel is reached. The two slopes, east and west, are very unequal in extent. The former, which M. Rolland has explored to a considerable extent, occupies more than one-third of the total area of Madagascar. A broad valley, that of the Mangoro, runs north and south, parallel to the great central chain and the coast. Unfortunately, the Mangoro is not navigable, even for canoes. The two other most important rivers are the Manangoro and the Mangataka; and these three rivers, with innumerable streams, render this part of the island one of the best-watered regions on the globe. The climate varies considerably from one zone to another. On the east coast the temperature oscillates between 13° and 30° C.; on the west coast, it never descends below 17°; in Imerina province it ranges from 5° to 25°. M. Rolland refers in some detail to the well-known characteristics of the fauna of Madagascar, and to the abundance of mineral treasures, especially iron, copper, and lead; but, he states, the natives carefully conceal the localities of the beds.

LIEUT. VANS AGNEW has undertaken a journey to the Upper Salween and South-Eastern Tibet, with the object of attempting the solution of the problem of the course of the Lu River—whether to the Irawadi or the Salween—propounded by General J. T. Walker in his paper read to the Royal Geographical Society on April 25, 1887. The Council of the Society have sanctioned a contribution of £100 towards the expenses of the expedition. Lieut Vans Agnew leaves India for the Salween in the course of the present month.

AT the February meeting of the Berlin Geographical Society Dr. A. Schenck read a report on his recent journey in Nama Land and Herero Land, South-West Africa. He showed that the whole country between Walfisch Bay and the Orange River is—in consequence of the purely mechanical decomposition of the prevailing granitic rock, which is taking place under the great daily variations of temperature, causing in many places the disintegrated surfaces to be eaten away in the form of a crust—covered over with a sea of sand and granitic shingle, from which the highest elevations stand out like islands. The country is not suited for agricultural colonies. The coast and the interior stand in contrast with regard to the season of rainfall. While on the coast the rain falls mostly in winter, the rainfall in the interior occurs only in summer, and nearly always in the form of thunder-showers, which, as Dr. Schenck believes, are caused by the condensation of the moisture-laden air, which is brought to this part by the warm, humid, north-east winds from the more equatorial regions of Africa, through coming into contact with the cool south-west winds blowing from the coast to the interior. As to the configuration of Great Nama Land, Dr. Schenck gives the following notes. After the hilly coast-region between Angra Pequena and Aos is passed, a broad valley-like depression is reached, filled up with drift-sand. East of the depression the country ascends and forms a stony, desolate plain, out of which rise isolated peaks or longer mountain-chains running in a north and south direction. The whole of this district, as far as Aos, forms a connected mountain system composed of ancient rocks, granite, and gneiss, which has been buried by the sand from which the higher parts stand out. Beyond Aos the traveller enters upon the steppe region, which is divided into detached plateau districts. Beyond Aos and the river-bed of the Goägib, on which the station of Bethanien is situated, the Huib plateau stretches away to the north, as far as the region of Khuias, and to the south to a point a few miles north of the Orange. A long series of table-mountains, resembling in form truncated cones, mark the western escarpment of this plateau; the former are composed of granite and gneiss, and are covered with limestone and sandstone, horizontally laid down. East of Bethanien, and corresponding with the line of a long geological fault, is the escarpment of another plateau; it is about 5000 feet in height. It descends to the Great Fish River on the east; on the other side of the river, the plateau character of the country is continued to the Karas Plateau, which extends into the brush steppe of the Kalahari. Further details concerning this interesting region will be found in the March number of the Proceedings of the Royal Geographical Society.

THE FORCES OF ELECTRIC OSCILLATIONS TREATED ACCORDING TO MAXWELL'S THEORY. BY DR. H. HERTZ.[1]

II.

Note by the Translator.

IT is to be noted that Hertz follows the French system of wave-lengths and periods. Had I noticed this before the diagrams went to the engraver, I would have altered it, and interpreted his T as ½T, &c., throughout. As it is, I have left them everywhere as in the original. My elaborate attempt to evade a literal translation of *Doppelpunkt* was quite unnecessary. Prof. Karl Pearson has sent me a reference to Maxwell's definition of "double-point" in vol. i. Art. 129, first edition of "Electricity and Magnetism."—O. J. L.

In order now to ascertain the distribution of force for the remaining parts of space we may use graphic representation, constructing for definite times the lines of electric force, viz. the curves Q = const., for equi-distant values of Q.

Since Q itself is the product of two factors, of which one depends only on r, the other only on θ, the construction of these curves presents no great difficulty.

We decompose every value of Q for which we want the curve into two factors in different ways; we determine the angle θ for which $\sin^2 \theta$ is equal to the one factor, and by means of an auxiliary curve that value of ρ for which the function of ρ contained in Q is equal to the other factor; we thus get as many points as we please of the curve. When one attempts to carry out the construction one perceives many small processes which it would be prolix to detail here. We will content ourselves with examining the results of such construction, as exhibited in Figs. 1, 2, 3, 4.

These figures represent the distribution of force at the times $t = 0$, $\frac{1}{4}T$, $\frac{1}{2}T$, $\frac{3}{4}T$; and also, by suitable inversion of the arrows, for all future times which are similar multiples of $\frac{1}{4}T$. At the origin is shown, in the correct aspect and about of the right proportional size, the arrangement by which in our earlier experiments the oscillations were excited.

The lines of force are not indicated right up to the picture because our formulæ regard the oscillators as infinitely short, so in the neighbourhood of a finite oscillator they are insufficient.

Let us begin a study of the figures with Fig. 1. Here, when $t = 0$ the radiation is in the condition of its strongest development, but the poles of the straight oscillator are not electrically charged—no lines of force start thence. Such lines of force begin, however, now from the time $t = 0$ to start out from the poles; they are inclosed in a sphere which expresses the value $Q = 0$. In Fig. 1 this sphere is indeed still vanishingly small, but it enlarges itself quickly, and by the $t = \frac{1}{4}T$ it fills already the space R_1 (Fig. 2). The distribution of lines of force inside the sphere is approximately of the same kind as correspond to a static electric charge on the pole. The speed with which the spherical surface $Q = 0$ spreads out from the origin is at first much greater than $\frac{1}{A}$ [or "v"]; in fact, the latter velocity would only correspond to

[1] Translated and communicated by Dr. Oliver Lodge. Continued from p. 404.

the distance given in the figure as $\frac{1}{4}\lambda$ for the time $\frac{1}{4}$T. At infinitesimal distance from the origin the velocity of outspreading is indeed infinite.

This phenomenon it is which we represented in the old mode of expression by saying that along with the inductive action travelling with the velocity of light there was superposed an electrostatic force travelling with infinite speed.

We properly express this phenomenon in terms of our present theory when we remark that fundamentally the self-forming waves do not arise solely from processes occurring at the origin, but are influenced by the condition of the whole surrounding medium, which latter, according to Maxwell's theory, is the true seat of the energy. However this may be, the surface Q = 0 expands with a velocity which gradually reduces to $\frac{1}{A}$, and by the time $t = \frac{1}{4}$T it fills the space R_2 (Fig. 3). By this time the electrostatic charging of the pole is at its greatest development; the number of lines of force which start thence attains its maximum value.

With further increase of time no fresh lines of force protrude from the poles; rather, those already produced begin to withdraw back into the conductor, there to vanish as lines of electric force, their energy, however, being converted into magnetic energy.

Hence occurs a singular behaviour which in Fig. 4 ($t = \frac{3}{8}$T) is plainly to be recognized, at least in its beginning. The lines which have furthest removed themselves from the origin get

Fig. 1.

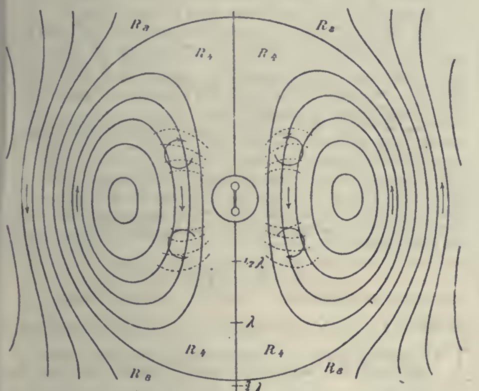


Fig. 2.

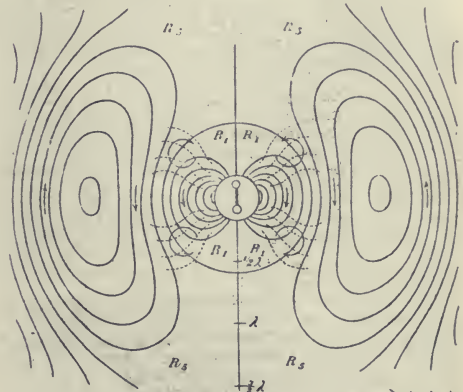


Fig. 4.

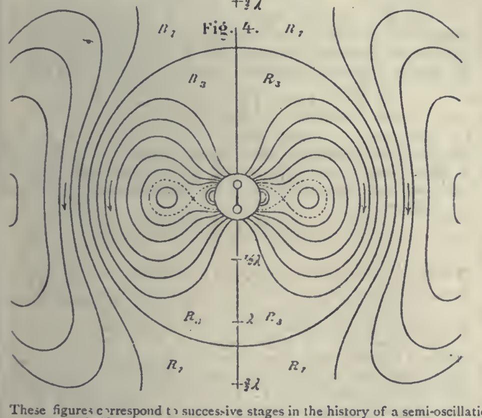
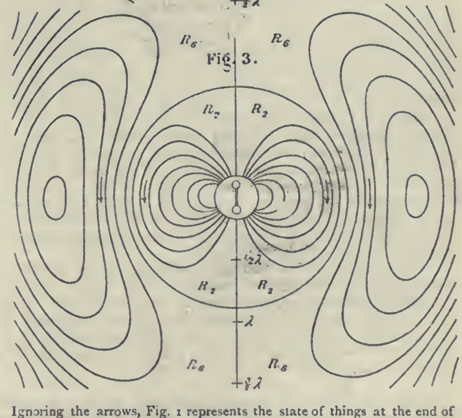


Fig. 3.



These figures correspond to successive stages in the history of a semi-oscillation. Ignoring the arrows, Fig. 1 represents the state of things at the end of every half-period; Fig. 3 at the end of every quarter-period; Figs. 2 and 4 represent one-eighth and three-eighths of a complete oscillation respectively. I do not feel clear about the correctness of the straight arrows in Fig. 1.

drawn together by the stress with a lateral inflexion; this inflexion approaches nearer and nearer to the axis of z; and then a self-closed portion detaches itself from each of the outer lines of force, which automatically spread out into space, while the residue sink back into the conductor.

The number of receding lines is just as great as the number of originally expanding lines; their energy, however, is necessarily diminished by the energy of the detached portion. This loss of energy corresponds to the radiation into space. In consequence of it the oscillation must soon come to rest unless some impressed forces restore the energy lost to the source. Meanwhile we have regarded the oscillations as undamped, and thus implicitly understood the existence of such forces.

In Fig. 1, to which we can now return for the time t = T, since we can imagine the arrows inverted, the detached portions of the lines of force fill the space R_4, while the lines starting from the poles have completely vanished. But new lines of force break out from the pole, and compress the lines whose early history we have followed into the space R_5 (Fig. 2).

It needs no further explanation now to follow these lines into the spaces R_6, R_7, and R_8. More and more they transform themselves into a pure transverse wave motion, and lose themselves as such in the distance. One would get the best picture of the play of force if one made a series of drawings for still smaller time-intervals, and examined them with a stroboscopic disk.

A closer consideration of the figures shows that the direction of the force changes from instant to instant for such points as lie either in the axis of z or in the plane xy. If we represent the force at a point, therefore, in the customary way by a line, the end point of this line oscillates, not indeed in a straight line, but in an ellipse. In order to see whether there are points for which this ellipse approximates to a circle, in which, therefore, the forces go through all the directions of a windrose without important change of magnitude, let us superpose two of the representations expressing times which differ by $\frac{1}{2}$T ; for instance, Figs. 1 and 3, or 2 and 4.

For the points we seek, the lines of the one set must plainly cut those of the other system orthogonally, and the distances of the lines of the one figure must be equal to those of the other. The small quadrangles formed by the superposition of the two systems must therefore be squares for the sought points.

There may be now remarked, in actual fact, a region of the kind sought: it is represented in Figs. 1 and 2 by circular arrows, whose directions give at once the direction of the rotation of the force. The dotted lines are inserted for convenience; they belong to the line system of Figs. 3 and 4.

One finds, moreover, that the force exhibits the behaviour here described, not only at the specified points, but also in the whole strip-formed region which, spreading out from those points, forms the neighbourhood of the z axis. Nevertheless, the magnitude of the force decreases so quickly in these directions, that only in the points above-mentioned can its singular behaviour be important.

The system of forces now described and required by theory can be quite well recognized in an incomplete observation, not hitherto indicated by theory, which I formerly described (*Wied. Ann.* xxxiv. p. 155, 1888). One cannot, indeed, explain everything about those experiments, but one can get the main points correctly.

By both experiment and theory the distribution of force in the neighbourhood of the oscillator is chiefly an electrostatic distribution. By both experiment and theory the force spreads out chiefly in the equatorial plane and decreases in that plane at first quickly, afterwards slowly, without being zero at a mean distance. By both theory and experiment the force, in the equatorial plane, in the axis, and at great distances, is of constant direction and varying magnitude, while at intermediate points it changes its magnitude but little and its direction much. The correspondence between theory and those experiments only breaks down in this, that at great distances, according to theory, the force remains always normal to the straight line through the source, while by experiment it appears to be parallel to the oscillator. For the neighbourhood of the equatorial plane where the forces are strongest this follows from the equations too, but not for directions which lie between the equatorial plane and the axis. I believe that the error is on the side of experiment. In these experiments the direction of the oscillator was parallel to both the main walls of the laboratory, and the component of the force which was parallel to the oscillator might be thereby strengthened in proportion to the normal components.

I have therefore repeated the experiment with a different arrangement of the primary oscillator, and found that with certain arrangements the result corresponds with theory. I did not attain an exact result, but found that at great distances, and in regions of small intensity of force, disturbances due to the boundary of the space available were already too considerable to permit a safe verdict.

While the oscillator is at work, the energy vibrates in and out of the spherical surfaces surrounding the origin. More energy goes out, however, through any spherical surface during an oscillation than comes back; and indeed the same excess quantity goes through all spherical surfaces. This extra quantity represents the loss of energy during the period of swing due to radiation. We can easily calculate its value for a spherical surface whose radius, ρ, is so great that it is permissible to employ a simplified formula. Thus the energy going out of the spherical zone between θ and $\theta + d\theta$ in the time dt will be—

$$2\pi\rho \sin\theta\, \rho d\theta\, dt . \frac{P}{4\pi A} (Z \sin\theta - R\cos\theta).$$

Putting into this the values of Z, P, and R, which are proper for great distances, and integrating from $\theta = 0$ to π, and from $t = 0$ to T, we get, as the energy going out through the whole sphere during every half complete swing,—

$$\tfrac{1}{3}E^2l^2m^3nT = \frac{\pi^4E^2l^2}{3\lambda^3}.$$

Let us try to obtain an approximate estimate of the amount of this corresponding to our actual experiments. In those we charged two spheres of 15 centimetres radius in opposite senses up to a spark length of 1 centimetre about. We may estimate the difference of potential between these spheres as 120 C.G.S. electrostatic units, so each sphere was charged to half this potential, and its charge was therefore E = 900 C.G.S. units.

The total store of energy which the oscillator originally possessed amounted to 60 × 900 = 54,000 ergs, or 55 centimetre-grammes. The length of the oscillators, moreover, was 1 metre approximately, and the wave-length was about 480 centimetres.

So the loss of energy in half a swing comes out about 2400 ergs. It seems, therefore, that after eleven half-swings one-half of the energy must have gone in radiation. The quick damping which the experiments made manifest was therefore necessitated by radiation, and could not be prevented even if the resistance of conductor and spark were negligible.

A loss of energy of 2400 ergs in 1·5/100,000,000 of a second means a performance of work equal to 22 horse-power. The primary oscillator must be supplied with energy at at least this rate if the oscillation is to be permanently maintained at constant intensity in spite of the radiation. During the first few oscillations the intensity of the radiation at about 12 metres distant from the vibrator corresponds with the intensity of solar radiation at the surface of the earth.

(To be continued.)

GENERAL EQUATIONS OF FLUID MOTION.

THE general equations of the motion of a fluid can all be comprehended in a single form, which seems to be deserving of special notice.

Taking the ordinary notation, u, v, w, for the velocity-components at any point, P, of the fluid at any instant, and denoting the components of vortical spin at the point by ω_1, ω_2, ω_3, the usual Cartesian equations can be at once put into the form—

$$\frac{du}{dt} + \frac{d}{dx}\left(\tfrac{1}{2}q^2 + \int\frac{dp}{\rho}\right) + 2(w\omega_2 - v\omega_3) = X,$$

and two analogues, q being the resultant velocity. If through the point P we draw any curve whatever, the direction-cosines of whose tangent are l, m, n, and multiply the above and its two analogues, respectively, by l, m, n, we obtain by addition the equation—

$$\frac{d\dot{s}}{dt} + \frac{dU}{ds} + 12\Delta = S \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

in which $\dot{s}$ stands for the component of velocity along the tangent to the curve, $U = \frac{1}{2}q^2 + \int\frac{dp}{\rho}$, S = component of external force-intensity along the tangent, and Δ is the volume of the tetrahedron formed by the vector drawn at P to represent q, the resultant velocity, the vector drawn to represent Ω, the resultant vortical spin, and the vector representing a unit length along the tangent to the curve at P. (Strictly speaking, the notation $\dot{s}$ is not a good one, but it is the best that presents itself.)

This equation (a) is that which I propose, as typical of all fluid motion, and as including all the special Cartesian equations in current use.

Some simple results follow at once for the case of steady motion. Thus, if we integrate (a) between any two points, A, B, of the curve,

$$U_B - U_A + 12\int\Delta ds = \int S ds \quad . \quad . \quad . \quad . \quad (1)$$

where U_B and U_A are the values of U at B and A.

Now, in particular, if the curve drawn at P is a stream-line, $\Delta = 0$ at every point of it; also, if the curve is a vortex-line, $\Delta = 0$ at every point, and we have the simple result,

$$U_B - U_A = \int S ds \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

a result which has long been known for a stream-line, but, apparently, not so long known for a vortex-line. It holds also for an infinite number of curves that can be drawn through P, all lying on a certain surface, as is pointed out by Lamb ("Motion of Fluids," p. 173), the surface in question being formed of a network of stream- and vortex-lines. That such surfaces exist in the fluid when the external forces have a potential, is proved most satisfactorily by taking the integral of (α) along a circuit through P, of which a part consists of stream-line and a part of vortex-line; but into the details of this we need not enter.

I observe, also, that this equation (2) holds for the portion of any curve whatever connecting any two points, A, B, on a network surface, although this curve does not lie on the surface.

Another point to which I would call attention is an analytical expression of the state of non-vortical motion. The physical expression has, of course, reference to the non-rotation of the three principal axes of the little ellipsoid into which, at each instant, a small sphere is deforming. The analytical expression of the fact takes usually the form that there is a velocity potential, *i.e.* $\frac{du}{dy} = \frac{dv}{dx}$, with two Cartesian analogues. Here, again, I would suggest a single equation, having no reference to special axes. This equation is simply

$$\frac{d\dot{s}}{d\sigma} = \frac{d\dot{\sigma}}{ds} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\beta)$$

where s and σ denote arcs of *any two curves whatever* drawn at the point P, and $\dot{s}$ and $\dot{\sigma}$ the component velocities of the fluid along them.

It is obvious that these contain the whole three of the usual Cartesian expressions. The proof is very easy.

Cooper's Hill. GEORGE M. MINCHIN.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The following Examiners in Natural Science have been appointed for the Honour Examinations:—Mr. J. V. Jones and Mr. A. L. Selby (Physics); Prof. McLeod and Mr. V. H. Veley (Chemistry); Prof. Milnes Marshall and Mr. W. Hatchett Jackson (Morphology); Prof. Sanderson and Prof. Schäfer (Physiology); Prof. Boyd Dawkins and Prof. Green (Geology).

The conditions of tenure of the Burdett-Coutts Geological Scholarship are to be altered, so as to make it necessary for the holders to devote themselves to Geology, and to work with the Professor.

Scholarships in Natural Science are announced for competition at Merton and at New College. The examination begins on July 2.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 21.—"The Influence of Bile on the Digestion of Starch. (1) Its Influence on Pancreatic Digestion in the Pig." By Sidney Martin, M.D., B.Sc., British Medical Association Scholar, and Dawson Williams, M.D. (From the Physiological Laboratory, University College, London.)

The experiments of the authors have shown that if pig's bile be added to a solution of starch with pancreatic extract the digestion goes on with greater rapidity than without the bile. The rapidity of digestion is increased with the addition of quantities up to 4 per cent. of dried bile (equivalent to at least 30 per cent. of fresh bile). The rapidity was tested by noticing when the iodine reaction of starch had disappeared. On further research, it was found that this property of the bile depended on the bile salts (hyoglycocholate of sodium). The increased rapidity of digestion was well seen if 0·6 to 2 per cent. of bile salts were added to the digestive mixtures.

It was also found that not only was the change of starch into dextrine hastened, but also the change into sugar; and that the amount of dextrine and sugar formed when bile-salts wer present was one-fifth more than when they were absent. Fo the methods used in estimating the amount of dextrine and sugar, the original paper must be consulted.

"The Innervation of the Renal Blood-vessels." By J. Rose Bradford, M.B., D.Sc., George Henry Lewes Student. Communicated by E. A. Schäfer, F.R.S. (From the Physiological Laboratory of University College, London.)

The research was undertaken in order to map out the origin, cause, and nature of the renal nerves in the dog more accurately than had hitherto been attempted. The method employed consisted in exciting the roots of the spinal nerves, and observing simultaneously the effects produced on the general blood-pressure and on the volume of the kidney, the latter being investigated by means of Roy's oncometer. The anæsthetics used were chloroform and morphia. The general results were shortly as follows:—

No efferent vasomotor fibres were found in the posterior roots.

The efferent vasomotor fibres for the blood-vessels of the kidney leave the cord in the anterior roots of the nerves, extending from the second dorsal to the second lumbar. The renal nerves are, however, most abundant in the tenth, eleventh, twelfth, and thirteenth dorsal nerves.

In individual cases, however, there may be small variations in the number of fibres going on the one hand to the kidney, and on the other hand to the other abdominal viscera.

When quick rates of excitation are used, only contraction of the kidney and increase of general blood-pressure are observed, *i.e.* the vaso-constrictor fibres are excited.

With slow rates, however, expansion of the kidney with no increase of blood-pressure occurs, *i.e.* the vaso-dilator fibres are stimulated.

Hence the renal vessels not only receive constrictor fibres, but also dilator, and these are also most abundant in the eleventh, twelfth, and thirteen dorsal nerves.

Similarly when the peripheral end of the divided splanchnic nerve is excited with slow rates, a fall of blood-pressure is observed instead of the rise seen with quick rates.

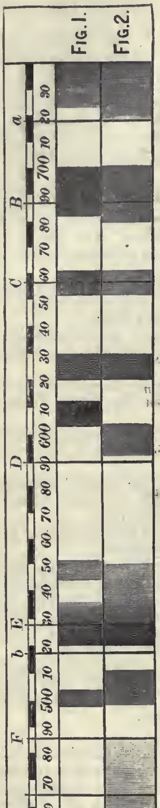
Hence the splanchnic contains not only vaso-constrictor fibres for the abdominal vessels, but also vaso-dilators.

The results of reflex excitation can be summed up shortly by saying that the excitation of an afferent nerve causing a rise of blood-pressure is accompanied by a renal contraction, unless the nerve is one of what may be called the renal area. In this case the rise of blood-pressure is accompanied, as a rule, by either a renal expansion or else by a mixed kidney effect.

The main conclusion of this communication is the demonstration of dilator fibres in the splanchnic and in the renal nerves, and also the fact that these vaso-dilator fibres reach the kidney by the same paths as the constrictor fibres.

Chemical Society, February 7.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Researches on the constitution of azo- and diazo-derivatives; compounds of the naphthalene-β-series (continued), by Prof. R. Meldola, F.R.S., and Mr. G. T. Morgan.—The action of nitric acid on anthracene, by Mr. A. G. Perkin. Hitherto, only anthraquinone and nitro-anthraquinones have been obtained by treating anthracene with nitric acid; the author, however, finds that nitro- and dinitro-anthracene can readily be prepared by the action of nitric acid upon anthracene if care is taken at once to decompose any nitrous acid which may be formed.—The preparation of glyceric acid, by Dr. Lewkowitsch.—The relation of cobalt to iron as indicated by absorption-spectra, by Dr. W. J. Russell, F.R.S., and Mr. W. J. Orsman, Junr. It is well known that when examined spectroscopically, some coloured metallic compounds are found only to produce a general absorption, but from previous observations it seemed possible to the authors that in some cases at least this might be resolved into bands by employing more powerful chemical agents than are generally used in such cases; experience had indicated that the chloride is usually the most suitable salt, and that it should be dissolved in chlorhydric acid and the liquid saturated with hydrogen chloride, also that, if possible, ether should be taken as solvent. Applying these views to iron, it was found that ferric chloride gave a banded spectrum strikingly similar to that of cobalt chloride. Irons of all kinds were examined: pig-iron, commercial cast-iron, and various manufactured articles; steel in the form of

wire, needles, and knives; and a number of specimens of reputed pure iron, viz. Demidorff's sheet-iron, a sample of which was kindly given to the authors by Mr. Crookes, electro-deposited iron, and some ancient Indian iron from Prof. Roberts-Austen, and iron prepared by the late Dr. Matthiessen. Also a large number of iron ores—hæmatite Elba ore, Welsh bog ore, micaceous ore, ordinary spathic ore, a spathic ore found in cryolite, for which the authors have to thank Dr. Müller; Giderite, pyrites from the chalk, wolfram and rouge. Iron was also separated from the ignited residue of blood. All the specimens examined gave the same result. Fig. 1 represents the bands seen in a solution of cobalt chloride to a scale of wave-lengths; the three most refrangible bands are easily photographed, but are not visible to the eye under ordinary conditions. The iron spectrum (Fig. 2) in general appearance closely resembles the cobalt spectrum, but the band which in cobalt is at 605 is slightly shifted nearer the blue, as shown in the diagram; there appears also to be a shift in the 501 band, but in the opposite direction. It was found that ether always dissolves out of the ferric chloride a substance which gave a band of extraordinary intensity, exactly agreeing in position with the 530 band in the cobalt spectrum; further, that on increasing the strength of the ethereal solution, other bands became visible, agreeing with the bands observed in the strong chlorhydric solution of ferric chloride, and differing only in the case of the 690 and 655 bands, which in the ethereal solution were nearer the blue. Fig. 2 is the spectrum observed in a solution of iron in chlorhydric acid, peroxidized by any ordinary means. For a variety of reasons the authors believe that this spectrum (Fig. 2) does not arise from the presence of cobalt in the iron. In the first place, there is a constant difference between the two spectra, as shown in the position and appearance of the band at 597. A trace of cobalt dissolved along with the iron gives the same spectrum as pure cobalt dissolved in chlorhydric acid. Again, on gradually increasing the strength of a pure cobalt chloride solution, the bands in the red are the first to appear, and the band at 530 is not visible until the general absorption has crept up as far as 580, completely blocking up the red end of the spectrum; in an ethereal solution from iron, on the contrary, this 530 band is the first to appear, and the bands in the red only become visible in comparatively strong solutions. Ether extracts the band-giving substance from the ferric chloride with great ease; but it abstracts nothing from the cobalt chloride. Again, on dissolving iron in chlorhydric acid, no bands are visible, and so long as the iron is in the ferrous state even ether extracts no band-giving substance; but on converting the ferrous into ferric chloride by nitric acid, or potassium chlorate, &c., the band-yielding substance is at once apparent. A known weight of Mr. Crookes's Demidorff iron was converted into chloride and dissolved in a known volume of ether, and the intensity of the bands compared with those given by cobalt chloride dissolved in a similar bulk of chlorhydric acid; it was found that approximately it required a weight of cobalt equal to that of the iron to give bands of similar intensity. Prof. J. Norman Lockyer, F.R.S., said that some years since, in a paper communicated to the Royal Society, he had suggested that there were many different molecular groupings of the same element possible, and that spectrum analysis would disclose these: if the same molecular grouping were demonstrated in several substances, then undoubtedly there was a common constituent. If the bands described by the authors represent a substance common to iron and cobalt, it should be possible to obtain spectroscopic evidence of its presence at some temperature on volatilizing the metals; although he had not fully studied cobalt and nickel comparatively, he had, in fact, found that under certain special conditions some of the spectroscopic appearances were common to both, and in such a marked degree as to render it improbable that they were caused by impurities. Dr. Perkin referred to the non-appearance of bands in an alcoholic solution of purpurin and their appearance in an ethereal solution, as an illustration of the influence of the solvent. Prof. Armstrong remarked that the slight shift of the bands which had been referred to did not necessarily indicate that different substances were primarily the cause of the absorptions, as it is well known that such effects were observed on employing different solvents; the absorbing substance might in the one case be held in combination more firmly than in the other; this view was in harmony with the statement that ether did not extract the band-yielding subtance in all cases. Dr. Russell in reply said that not the spectrum as a whole, but only one of the bands was shifted. His view was that the solvents had broken up the substance into a finer state.—Note on methyl fluoride, by Dr. N. Collie. Methyl fluoride assumes the critical state at 44°·9 C. and at a pressure of 47,123 mm. This pressure is probably slightly too high, owing to a trace of air, and the temperature too low. The error in pressure probably does not exceed 1500 mm., and of temperature 0°·2 C.—The nitration of naphthalene-β-sulphonic acid, by Prof. H. E. Armstrong, F.R.S., and Mr. W. P. Wynne. According to Cleve three isomeric α-nitro-β-sulphonic acids are produced on nitrating naphthalene-β-sulphonic acid; the chlorides of which melt respectively at 169°, 140°, and 125°. The authors find, contrary to the view put forward provisionally by Cleve (*Ber. der Deut. Chem. Gesells.*, xxi. 3275), that the first compound is a heteronucleal derivative and corresponds in constitution with the dichloronaphthalene melting at 63°·5. All attempts to obtain the sulphochloride of intermediate melting-point have been unattended with success.—Action of bromine and chlorine on the salts of tetrethylphosphonium, by Prof. O. Masson and Mr. J. B. Kirkland.—Preparation of the salts of triethylsulphine, tetrethylphosphonium, and analogous bases, by the same.



Linnean Society, February 21.—Mr. Carruthers, F.R.S., President, in the chair.—Mr. George Murray exhibited a fossil Alga, *Nematophycus Logani*, Carr.—Mr. G. C. Druce exhibited some rare British plants from Scotland, amongst which were *Calamagrostis borealis*, *Ranunculus acris*, var. *pumilus*, and *Bromus mollis*, var. *decipiens*.—Prof. Marshall Ward exhibited a sclerotium of a Fungus produced from a Botrytis spore, and explained the method by which it had been obtained.—A paper was then read by Mr. F. Townsend, M.P., on *Euphrasia officinalis*, with a description of a new sub-species, and a discussion followed, in which the President, Mr. J. G. Baker, and others took part.—In the absence of the author, a paper by Mr. C. T. Druery, on sexual apospory in *Polystichum angulare*, was read by the Botanical Secretary, Mr. B. D. Jackson, upon which remarks were made by Mr. Murray and Dr. D. H. Scott.—Mr. Murray then gave the substance of a paper on a new genus of Green Algæ, proposed to be named *Boodlea*, and in so doing made some instructive observations on the affinities and distinguishing characters of allied genera. The paper was criticized by Messrs. A. W. Bennett, Reay Greene, and D. H. Scott.—In continuation of his researches upon the eyes of insects, Mr. B. T. Lowne gave an admirable exposition of the structure of the retina in the blow-fly, illustrated by preparations under the microscope, and some excellent photographs.

Geological Society, February 15.—Annual General Meeting.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The Secretaries read the reports of the Council and of the Library and Museum Committee for the year 1888. The Council stated

that they had once more to congratulate the Fellows upon the prosperous state of the Society's affairs. The report of the Library and Museum Committee, after enumerating the additions made to the Society's Library and collections during 1888, referred briefly to the work done in the Museum, in the way of cleaning and putting it in order.—The President then presented the Wollaston Gold Medal to Prof. T. G. Bonney, F.R.S.; the Murchison Medal to Mr. William Topley, F.R.S., for transmission to Prof. James Geikie, F.R.S.; the Lyell Medal to Prof. W. Boyd Dawkins, F.R.S.; the Bigsby Medal to Mr. J. J. Harris Teall; the balance of the proceeds of the Wollaston Fund to Mr. A. Smith Woodward; the balance of the Murchison Geological Fund to Mr. Grenville A. J. Cole; and the balance of the proceeds of the Lyell Geological Fund to M. Louis Dollo.—The President read his Anniversary Address, in which, after giving obituary notices of Mr. W. Hellier Baily, Mr. H. Carvill Lewis, Vice-Admiral T. A. B. Spratt, Viscount Eversley, Mr. John Brown, Mr. W. Ogilby, and other deceased Fellows, together with notices of the Foreign Members and Correspondents of the Society who had died since the last anniversary meeting (Prof. Gerhard Vom Rath, Prof. T. Kjerulf, Prof. Giuseppe Meneghini, and Prof. Giuseppe Seguenza), he noticed the papers which had been published by the Society during the past year. The remainder of the address consisted chiefly of a discussion of the work of the International Congress from its commencement to the last meeting in London in 1888, and dwelt upon the influence which such meetings exercise upon the progress of geological science, quite apart from any formal resolutions which may be arrived at by the members.—The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President: Dr. W. T. Blanford, F.R.S. Vice-Presidents: Dr. John Evans, F.R.S., Prof. T. McKenny Hughes, Prof. J. W. Judd, F.R.S., Prof. J. Prestwich, F.R.S. Secretaries: Mr. W. H. Hudleston, F.R.S., Mr. J. E. Marr. Foreign Secretary: Sir Warington W. Symth, F.R.S. Treasurer: Prof. T. Wiltshire. Council: Prof. J. F. Blake, Dr. W. T. Blanford, F.R.S., Prof. T. G. Bonney, F.R.S., Mr. James Carter, Dr. John Evans, F.R.S., Mr. L. Fletcher, Dr. A. Geikie, F.R.S., Prof. A. H. Green, F.R.S., Rev. Edwin Hill, Mr. W. H. Hudleston, F.R.S., Prof. T. McKenny Hughes, Prof. J. W. Judd, F.R.S., Major-General C. A. McMahon, Mr. J. E. Marr, Mr. E. T. Newton, Prof. J. Prestwich, F.R.S., Mr. F. W. Rudler, Prof. H. G. Seeley, F.R.S., Sir Warington W. Smyth, F.R.S., Mr. W. Topley, F.R.S., Rev. G. F. Whidborne, Prof. T. Wiltshire, Rev. H. H. Winwood.

Zoological Society, February 19.—Dr. St. George Mivart, Vice-President, in the chair.—Mr. Sclater exhibited specimens of the eggs and chicks of the Hoatzin (*Opisthocomus cristatus*) from a series collected by Mr. R. Quelch in British Guiana, and called attention to the extraordinary development of the wings in the chick, in reference to the statement that these organs are used like hands for climbing-purposes.—Mr. Sclater exhibited heads and skins of a new Antelope obtained by Mr. H. C. V. Hunter, in Eastern Africa, which he proposed to call *Damalis hunteri*, after its discoverer.—Sir E. G. Loder, Bart., exhibited and made some remarks on a skeleton of the Rocky Mountain Goat (*Haplocerus montanus*).—Dr. Günther exhibited a mounted specimen of Thomson's Gazelle (*Gazella thomsoni*), and pointed out its complete distinctness from Grant's Gazelle (*Gazella granti*). The specimen in question had been obtained in Masai-land by Mr. H. C. V. Hunter.—Mr. R. Lydekker read a paper on the skull of *Lytoloma*, an extinct genus of Chelonians allied to *Chelone*.—Mr. R. Lydekker pointed out the characters of an apparently new species of *Hyracodontotherium*, based on specimens from the phosphorites of Bach, near Lalbengue, in France.—Dr. A. Günther, F.R.S., described some new fishes from the Kilima-njaro district in Eastern Africa, based on specimens obtained by Mr. F. J. Jackson during his recent expedition into that country. He also exhibited a dried specimen of a fish obtained by Mr. H. C. V. Hunter from one of the crater-lakes in the same district, which he referred to a new genus and species of Chromidæ, proposed to be called *Oreochromis hunteri*.—Dr. Günther also exhibited a pair of horns of an Antelope obtained many years ago in the interior of Southern Central Africa, which were remarkable for their length and gentle backward curvature, with only a very slight twist near the tips. He referred these horns to a new species, proposed to be called *Antilope triangularis*.—Dr. Günther read some notes on a Bornean Porcupine, which he had formerly described as being without a tail, and named *Trichys lipura*. It now appeared that some specimens of this animal possessed a long and slender tail, but that other characters would necessitate the retention of the genus as distinct from *Atherura*.—Mr. F. E. Beddard read a paper directing attention to certain points in the anatomy of the Accipitres with reference to the affinities of *Polyboroides*. This form was shown to belong to the Falconidæ, and to have no real affinities with *Serpentarius*.—Sir Walter Buller read a paper on a species of Crested Penguin from the Auckland Islands, based on a specimen lately living in the Society's Gardens, which he proposed to call *Eudyptes sclateri*.

Anthropological Institute, February 26.—Dr. J. Beddoe, F.R.S., President, in the chair.—Mr. Francis Galton exhibited a new instrument for testing the delicacy of perception of differences of tint; also an instrument for telling reaction time. Both instruments will be exhibited in the Paris Exhibition.—Major C. R. Conder, R.E., read a paper on "The Early Races of Western Asia."

EDINBURGH.

Royal Society, February 4.—The Rev. Prof. Flint, Vice-President, in the chair.—Prof. T. R. Frazer read a paper on the natural history, chemistry, and pharmacology, of *Strophanthus hispidus*.—Mr. John Aitken exhibited and described his improved apparatus for counting the dust particles in the atmosphere.—Prof. Rutherford read a paper by Dr. G. N. Stewart, on the electrotonic variation in nerve with strong polarizing currents.

February 18.—Dr. Thomas Muir, Vice-President, in the chair.—Prof. Crum Brown communicated a paper by Mr. Alex. Johnstone, on the prolonged action of sea-water on pure natural magnesium silicates.—A paper by Dr. A. B. Griffiths on the so-called liver of *Carcinus mænas* was also read.—Dr. Muir communicated a paper by Mr. Alex. M'Aulay, Melbourne, on the differentiation of any scalar power of a quaternion, and a note by Prof. Tait on Mr. M'Aulay's paper.—Prof. Crum Brown read an account by Mr. Albert Campbell of the change in the thermo-electric properties of Wood's fusible metal at its melting-point.—Prof. Brown also read a paper by Mr. Frank Beddard on the anatomy and physiology of *Phreoryctes*.

PARIS.

Academy of Sciences, February 25.—M. Des Cloizeaux, President, in the chair.—Note on the question, whether their original infectious properties can be recovered by pathogenic microbes, which have apparently preserved nothing beyond the power of vegetating outside the living animal organism, by M. A. Chauveau. In continuation of his recent communication (*Comptes rendus*, cviii. p. 319), the author here describes some experiments which show that, in *Bacillus anthracis* apparently deprived of all infectious virulence, this virulence may be as easily restored as the simply diminished virulence is renovated in M. Pasteur's attenuated microbes. It results generally from these studies that in losing or recovering their virulence pathogenic microbes undergo no specific transformation. These physiological metamorphoses are merely an extension of the law well known to botanists that the conditions of culture may modify not only the form, but also and specially the functions of plants.—On some points in the theory of the sextant, by M. Gruey. The points here discussed are (1) the possibility of constructing the sextant with a single glass, which is decided in the affirmative, a means being indicated by which the practical inconvenience of such an instrument may be obviated; (2) the use of the transparent part of the small glass. This is suppressed by some, preserved by others, and M. Gruey considers that it is in fact useless.—On a question in the doctrine of probabilities, by M. E. Mayer. A solution is here proposed of M. Bertrand's 57th problem, dealing with the case of two players with equal chances and equal capital, and the probability of one ruining the other in a given number of throws.—Remarks on the conductivity and mode of electrolysis of concentrated sulphuric acid solutions, by M. E. Bouty. The main object of these experiments is to measure the molecular conductivity of sulphuric acid at or about the temperature of 0° C. An attempt is also made to determine the coefficients of temperature α and β in the formula—

$$C_t = C_0(1 + \alpha t + \beta t^2).$$

—On the electro-chemical measurement of the intensity of currents, by M. A. Potier. Arguments are advanced to show that the electrolytic measurement of intensity cannot be regarded

as rigorously accurate except on the condition of the electrodes presenting no trace of polarization. This condition is generally supposed to be strictly complied with when the electrodes are formed of molten metals; but the present researches prove that such is not always the case.—On the reciprocal influence of two rectangular magnetizings in iron, by M. Paul Janet. A piece of iron being magnetized in a given direction by a given magnetic force, the author inquires whether this magnetic state becomes modified by the establishing or interrupting a fresh magnetic current perpendicular to the first.—On drops of mercury as electrodes, by M. Ostwald.—A correction as regards the action of sulphurous acid on the alkaline thiosulphates, by M. A. Villiers. In a previous note (*Comptes rendus*, cvi. pp. 851 and 1354) the author described the sodium salt of a new oxy-acid of sulphur as obtained by the action of sulphurous acid on the sodium thiosulphate, and as having the formula $S_4O_8Na_2$. But he has since discovered that this salt contains two atoms of hydrogen, so that its formula is $S_4O_8Na_2H_4 = S_4O_6Na_2.2H_2O$; that is to say, it is hydrated tetrathionate of soda.—On the valency of aluminium, by M. Alphonse Combes. The vapour-density of $Al(C_5H_7O_2)_3$ at 360° in an atmosphere of nitrogen was found to be 11·25, agreeing with the above formula. Its valency at this comparatively low temperature therefore shows its analogy with indium and other triad elements.—Combination of mannite with the aldehydes of the fatty series: ethylic acetal, by M. J. Meunier. Two processes are described, by means of which the ethylic acetal of mannite may easily be prepared. The combination of mannite with an aldehyde of the aromatic series (benzoic aldehyde) has already been studied. It now appears that an acid solution of mannite, mixed with equal molecular weights of acetic and benzoic aldehydes, yields ethyl acetal, and not an acetal resulting from the simultaneous combination of the two aldehydes.—M. A. Haller describes the preparation of some new neutral and acid ethers of the camphols, and also gives an easy process for the separation of camphor and camphol.—M. Aimé Girard reports the results of some protracted experiments on the cultivation of the potato in France, with a view to the selection of the best tubers, and a more abundant yield of starch-producing roots.—M. G. Hayem studies the causes of the fatal effects resulting from the transfusion of blood between animals of different species, and more especially from the injection of dogs' blood in the rabbit.—The porphyritic rocks of Cavenac, near Saint-Pons, are described by MM. P. de Rouville and Auguste Delage; and those of the Forez district by M. U. Le Verrier.—M. Ed. Piette gives an account of some human and animal remains representing a transitional epoch between Quaternary and modern times, recently discovered by him in a cave on the left bank of the Arize.

Astronomical Society, February 6.—M. Flammarion in the chair.—M. Guiot, of Soissons, sent observations of Uranus made with the naked eye, and of Neptune with an opera-glass.—M. Schmoll showed diagrams of solar activity during 1888. He had noted 190 days without spots. M. Bruguière placed the minimum at 1888·8. MM. Lihou and Jacquot sent some remarks on the same subject.—M. Flammarion read a paper on γ Arietis, calling attention to the remarkable relative fixity of the two components. His measures at Juvisy gave 8″·51 and 359°·1.—M. Ch. Moussette made some remarks on the lunar eclipse of January 17.—General Parmentier read a note on the planetoids discovered in 1888, and showed that they confirmed the classification of those bodies which he published a few years ago.—M. Gunziger exhibited some Thompson's disks, and showed their utility for drawing and accurately placing sun-spots.

STOCKHOLM.

Royal Academy of Sciences, February 13.—Sir Joseph Lister was elected a Foreign Member of the Academy.—Prof. Wittrock gave an account of the present state of the Bergian Garden belonging to the Academy.—An examination of some Algæ referred to the genus Adenocystis, Hooker fil. et Harvey, by Prof. F. R. Kjellman.—Contributions to the flora of Medelpad, by Dr. L. M. Neuman.—Report on investigations relating to the flora and fauna of the peat-bogs of Scania, by Herr G. Andersson.—Report on investigations relating to the Ascomycetes, especially the coprophilous, of Öland, by Herr C. Starbäck.—A special case of the problem of three bodies, by Prof. Gyldén.—On Odonatæ collected during the Swedish Expedition to Yenisei in 1876, by Dr. F. Trybom.—*Ichneumones pneustici*, by the late Lector A. E. Holmgren.—An experiment with an electric spark and a small flame, by Dr. C. A. Mebius.—Prof. Nilsson gave an account of the researches of Dr. Krüss on cobalt and nickel.—On the singular points of the common algebraic differential equations, by Dr. J. Möller.—On maximi and minimi convergents of a certain class of distinct integrals, by Herr C. B. Cavallin.—On naphtoë acids, &c., by Dr. Ekstrand.—On the b^1-b^2-brom-naphthalin-sulphon acid, by Herr Forsling.—On the reaction of the fuming sulphuric acid on a^1-b^1-chlor-naphthylamin and on a^1-b^1-chloracetnaphthalid, both combined with hydrochloric acid, by Herr P. Hellström.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Mémoires de la Société de Physique et d'Histoire Naturelle de Genève, tome xxx. Première Partie (Genève).—History of the Linen Hall Library, Belfast: J. Anderson (Belfast).—Glimpses of Feverland: A. P. Crouch (Low).—Ueber den Einfluss der Festsitzenden Lebensweise auf die Thiere: Arnold Lang (Jena, Fischer).—Lehrbuch der Vergleichenden Anatomie, Erste Abthg.: Arnold Lang (Jena, Fischer).—Darwinism and Politics: D. G. Ritchie (Sonnenschein).—New South Wales, 1887, Report of the Minister of Public Instruction (Sydney, Potter).—Annual Report of the Department of Mines, N.S.W., for the Year 1887 (Sydney, Potter).—New South Wales Australian Museum, Report of Trustees for 1887 (Sydney, Potter).—New South Wales Report on Technical Education: E. Combes (Sydney, Potter).—Molekularphysik, Zweiter Band: Dr. O. Lehmann (Leipzig, Engelmann).—Histologische Beiträge, Heft 2: E. Strasburger (Jena, Fischer).—Ueber die Hypothese einer Vererbung von Verletzungen: Dr. A. Weismann (Jena, Fischer).—Intracellulare Pangenesis: H. de Vries (Jena, Fischer).—The Best Forage Plants fully described and figured: Drs. Stebler and Schröter; translated by A. N. McAlpine (Nutt).—Index of Publications on Methods of Communication in the Field, and on Torpedo Warfare: R. von Fischer-Treuenfeld (Alabaster).—Electricity in the Service of Man, Part 1, edited by R. Wormell (Cassell).—The Asclepiad, No. 21, vol. 6: Dr. B. W. Richardson (Longmans).—Note on the Lapps of Finmark: Prince Roland Bonaparte (Paris).—La Nouvelle-Guinée, 3rd Notice—Le Fleuve Augusta; 4th Notice—Le Golfe Huon: Prince Roland Bonaparte (Paris).—Himmel und Erde, Heft 6 (Berlin, Paetel).—Beiblätter zu den Annalen der Physik und Chemie, 1889, No. 2 (Leipzig, Barth).—Verhandlungen des Naturhistorischen Vereines, Fünfte Folge. 5 Jahrgang, Zweite Hälfte (Bonn, Max Cohen).—Geological Magazine, March (Trübner).

CONTENTS.

THURSDAY, MARCH 14, 1889.

THROUGH THE HEART OF ASIA.

Through the Heart of Asia: Over the Pamir to India. By Gabriel Bonvalot. Translated from the French by C. B. Pitman. (London: Chapman and Hall, 1889.)

THIS is a translation of the account of a very remarkable journey taken in the year 1886 by M. Bonvalot through Central Asia. It is not the first time that M. Bonvalot has traversed that region, for in the years 1880–82, accompanied by M. Capus, and starting from Moscow, he entered Turkestan from Siberia, explored part of Bokhara and the mountains of Kohistan and Chitral, and returning home by Samarcand and Bokhara, descended the Amu-daria to Khiva, and finally crossed the desert of Ust-Urt in the depth of winter. That journey, however, only embraced the country north of the Oxus, and the present one was undertaken to complete the exploration of Central Asia. Shortly speaking, the latter journey—that of which this work treats—began at Batum, whither MM. Bonvalot and Capus had gone by ship from Marseilles, and continued through the Caucasus along the south-west shore of the Caspian Sea into Persia, thence into Bokhara. At Chitral, on the borders of Afghanistan, they were detained as prisoners by Ishak Khan, of whom we have heard so much lately. He was then in revolt against Abdurrahman, and his captives were not released till the Indian Government interfered on their behalf.

There is very much to interest us in M. Bonvalot's account of the feelings of the native tribes in that vast region towards England and towards Russia, but that we shall pass over, merely pointing out what is most important from a geographical or ethnological point of view in the work. At Adshi-Cabul, which is on the south-west shore of the Caspian, he says that the population is Turkish, though the country is Tartar. The men are tall, with good features, and are plainly a mixture of Caucasian and Persian. Their dress differs from that of other Mahometans in that the tight-fitting upper garments with long skirts are not frequently seen; they seldom reach below the knee, and are often open at the breast. The head-dress is higher and broader at the top than the usual Mahometan head-dress, and in place of high boots they wear broad slippers or sandals, with very large wooden heels and curled-up toes. The feet are bare, and are covered with woollen socks, which have fantastic coloured figures worked on them. It was the festival of the New Year when the travellers arrived at Adshi-Cabul, and the whole place was *en fête*. The Mahometans of the region on such occasions dye their hands, beard, and hair a bright red, with henna. The Turkish language was the only one spoken there, but a few stages further on it seemed as if a new world was reached, where the Russian tongue, Russian fashions, Russian cattle and horses, were everywhere. This, it appears, was due to the descendants of a body of Russian settlers, called Malakanes, who had left their homes on account of their religion, but it is not quite clear in what respect they differed from the orthodox Russian Church. The country—that is, the region north-west of Resht—is wonderfully fertile, but the inhabitants suffer terribly from fevers. The waters swarm with duck, teal, herons, and cormorants; eagles and foxes abound there.

Further on, still on the shores of the Caspian, may be seen "all the birds of creation." Generally speaking, the ground held by the Tartars is badly cultivated: the men are incorrigible idlers, lying in the fields while the women sow the seed; and in harvest-time, instead of gathering in the crops themselves, they sell them as they stand in the fields to the mountaineers for about a third of their value. Though there is an abundance of good timber at hand, they live in wretched hovels with thatched roofs, or in huts made of reeds, with a thin facing of mud, and in the coldest weather prefer to lie shivering in their huts to gathering the firewood which lies almost at their hand. They are badly clad, and, as has been said, suffer terribly from the fevers that are so prevalent. A little further south, towards the confines of Persia, though the month was March, the foliage and the flowers were marvellous: wild pomegranate trees and wild briars growing under acacias and beeches were well covered with leaves; anemones, violets, and daisies bloomed everywhere. At Chifa Rud, near Resht, Talichi, a peculiar Turkish dialect is spoken, but some of the people also use Guilek. Rice and fish, of which there is abundance, are the food of the inhabitants, bread being unknown. From Teheran the travellers journeyed to Meshed, travelling almost due east, and skirting the Great Salt Desert, which presented the appearance of heaps of sand that had been lately washed by the waves of the sea. The journey through the mountains at this part was very trying. The sun was scorching, and there was not the slightest shelter to be had. The River Tchaï flows through the valley which they traversed, and was the only thing in motion in the whole region. The waters of the river are salt. Dehinemek (the "Salt Village") consists of only a few houses, and the inhabitants of it are as idle as those on the border of the Caspian, cultivating just enough to feed their families and to supply passing pilgrims. Here, as in the whole country, everything seems covered with salt. It is seen on the walls of the houses and on the banks of the rivers, and and the water one drinks is very salt. There are many travelling saltpetre-makers to be found in this region, who in summer go from place to place wherever they can find materials to work upon. Their mode of operation is a very rough and ready one. Holes in the earth serve as vats and boilers, and below these are placed ovens. Abundance of brushwood supplies them with material for their fires. They collect from the surface of the earth heaps of a compost of salt and animal manure. This is soaked for twenty-four hours in water, then filtered, and then boiled for twenty-four hours, cleansed, and placed in the sun, so that the water may evaporate. An ordinary workman can make about fifty pounds in a day, and this he sells at the rate of a penny a pound. The workers appear quite contented with their lot, and the industry is preserved in their families for generations.

Around Bostan and Shahrud, numbers of gypsy encampments were met. The inhabitants said they were natives of Seistan, and gained their living by making brass wire and working metal. Each family pays a small yearly contribution to the chief of the Shah's footmen.

They are constantly on the move, and make their tents or huts so lightly that they can dismantle them at a moment's notice. They resemble the natives very much in features, and have the same filthy habits, but they are more swarthy in complexion, and much thinner. With regard to the Turkomans, they are, or rather were—for they have ceased their depredations—the terror of the Persian pilgrims and traders. M. Bonvalot thinks that they have received a bad name which they do not deserve. They are frank, gentle, hospitable, affable, and truthful; while their victims, the Persians, are the incarnation of deceit and lying. The Turkomans, he says, have all the good qualities of the Turks, a race which has been more calumniated than any other, but from which some of the more highly esteemed European nations might with advantage learn many things. From Samarcand they journeyed to the Amu-daria, whence, completing an irregular curve, they returned to Samarcand. Upon leaving Sari-jui, they came up with a number of Uzbegs, who were coming down from the summer encampments in the mountains to their winter-quarters in the valleys. The men of the tribe ride in front, driving the cattle and horses before them, and the younger children look after lambs, goats, and calves. Some are bareheaded; others wear a dirty turban. Though they are filthy in their habits, they are wonderfully strong. Their dwellings are strong, but easily taken to pieces. As a rule, the Turkoman is not a nomad, like the Uzbeg; he has not very many cattle, so that it is not necessary for him to move to different pastures with the changes of the seasons. In the valley of the Surkhan they met many encampments of gypsies, who appear to lead the same sort of life all over Central Asia. Their tents are of the most primitive kind—two poles, with a piece of cloth spread over them. From this to Regar the country is very rich, well watered, abounding in rice-fields, but, like the other parts of Central Asia where there is a copious rainfall, fevers abound. At Kara-tagh, besides English, French, Russian, and Indian goods, various products of the district itself are sold in the market, as, for instance, rice, barley, wheat, grapes, and dried apricots. In the towns there are many blacksmiths and saddlers, and a few potters, who are very skilful in making and enamelling cups and dishes. Strange to say, the occupation which has the most numerous representatives is that of druggist. A funeral procession amongst the Uzbegs, who regard death as a blessing and not as an evil, is thus described:—

"A small stream which crosses the road is salt, and we notice a group of men ascending its course with very rapid steps. They are carrying something on a stretcher, but, instead of going as slowly as they can, like bearers of a dead body in the West, they march at full speed, as if they were conveying some one who was very ill, or had met with an accident, to the doctor's. They go across the fields, and stumble in their hurry. They talk loudly, as if they were quarrelling, and there is not the least trace of sadness upon their faces. There are about ten of them, and they are dressed as plainly as usual. . . . In advance are several men with poles, which will be used for forming a vault on the grave, and behind comes an aged mollah, leaning upon a stick."

The travellers reached Samarcand in December, and were about to abandon the idea of going further, when General Karalkoff suggested that they should try to enter India by Pamir. It was worth trying, for nobody had ever before thought of attempting such a thing in winter. The undertaking was a great one, and they took every care in their outfit before setting out. Boots made of double felt with leather soles, with strips of skin protecting the seams, were made for them. Long stockings, made of felt, reaching up to the thigh, lined trousers with leather trousers over them, were the protections for the legs. The body was covered with two garments, one of them made of Kasghar sheepskin with the wool left on. A sheep-skin cap covered the head and ears, and a huge hood of the same material enveloped the whole head and face, with the exception of the eyes. With regard to the food supply, the more remarkable portions were immense numbers of small slabs of bread soaked in fat and baked twice over; mutton boiled down, salted, and placed in bladders; roasted millet seed, and dried apricots. The journey really began at Osh on March 6, and for over a month the whole party underwent the severest privations; the goal, however, was reached at last. We have not space to say much about this journey over the Pamir, one of the most remarkable feats of travel ever accomplished, but a few extracts in M. Bonvalot's own words will show what the enterprising explorer and his companions went through:—

"March 13. . . . The variations of temperature are very great, for at 9 a.m. the thermometer marks 75° F. in the sun and 10° F. below freezing in the shade, while at 2 p.m. it is nearly 100° F. in the sun and 3° below freezing in the shade; at 6 p.m., there are 18° of frost, while at 9.20 p.m. the glass is several degrees below zero."

"March 17 (when crossing the Alai). We are all of us exhausted and out of breath, devoid of all strength, and nearly blind. We have splitting headaches and a feeling of suffocation. One man is stretched out on his back, close beside his horse, which is lying on its side; another man is asleep as he stands with his head resting against the saddle; a third is whipping his poor horse, to the tail of which he clings like a drowning man to a buoy. Some of the men were bleeding from the nose, and so were the horses, the blood freezing as it trickled down their muzzles, and looking like ruby stones. . . At 6 in the morning there were 75° of frost."

On March 22 they reached an altitude of 15,000 feet on Kizilart. On March 27, when at the height of 15,700 feet—that is, at the summit of the pass of Kizil-Djek—a terrific snow-storm burst on them.

"March 30. . . . At 2.20 a.m., with the moon still so luminous that I could distinguish objects inside the tent, I go out to look at the thermometer, and find that the mercury has vanished. It has evidently been frozen. Thinking that I may be mistaken, I show the instrument to Capus, and we light a candle, the result being that we find the mercury really has frozen up, and is no bigger than a leaden pellet."

About the beginning of April they approached human habitations, and here, near the source of the Oxus, they found numbers of monuments to the dead:—

"The tumuli are built south-west to north-east, so that the dead may have their faces turned towards the holy city. They extend around four mausoleums made of earth, almost double the height of an *oui* (felt tent), and with a frontage of about 30 feet. The cupolas are pointed, and the architecture very simple, as there are no materials handy to attempt anything ambitious. Moreover, if a higher building had been erected, the wind,

which is the terror of the Pamir, would soon have brought it to the ground. At the four corners of the largest of the mausoleums a rude attempt has been made to carve pigeons. At the end of the humbler tombs are some stones sunk in the earth. Some of them have a sort of railing round them, formed of stakes bound together by wooden cords."

The numerous illustrations are excellent, and the translation is well done, but it is a pity that Mr. Pitman has not given the English names of places. Many readers will not recognize in French forms names which in English forms are perfectly well-known.

THE TESTING OF MATERIALS OF CONSTRUCTION.

The Testing of Materials of Construction. By William Cawthorne Unwin, F.R.S., M. Inst.C.E. (London: Longmans, Green, and Co., 1888.)

IN a volume of about 480 pages, Prof. Unwin has condensed probably all our knowledge of the strength of materials used in construction. The work is unique in its way, there being no other equally trustworthy, as far as we are aware, in existence.

Engineering structures of to-day are designed with due regard to the strength of their individual parts, and each part is so proportioned that it will safely carry its load, yet not be of greater strength than is necessary for this special purpose. In the Forth Bridge, for example, the strains in each member are calculated to a nicety, and the section of metal is duly proportioned. This could not be done if we did not know exactly the actual strength of the various materials used, as well as their behaviour under varying conditions of load less than the breaking load of the material. It is therefore necessary that these data should be obtained by using the most perfect apparatus obtainable, and that the experiments should be made by persons who have given much time and thought to the subject. Bearing this in mind, we gladly welcome Prof. Unwin's work; his name is well known among engineers as one thoroughly able to write such a work successfully.

The volume consists of three parts. In the first, the mechanical properties of materials are explained—that is, the phenomena of elasticity and plasticity, and the relations between stress and deformation, so far as they have been scientifically ascertained. In the second, the apparatus used in the engineering laboratory is described. Lastly, the third part contains a collection of the most complete and trustworthy results of testing of all the ordinary materials in use. Chapters IV. to VII. contain admirable descriptions of various kinds of testing machines, measuring instruments, and other useful appliances used in an engineering laboratory. These chapters are freely illustrated with excellent woodcuts of the different instruments, the larger ones of the testing machines being remarkably good.

The chapters dealing with iron and steel show a large knowledge of their practical working and characteristics. We agree with the author that a standard form for tensile test pieces ought to be accepted by engineers in this country, so that results may be more easily compared.

In all the large steel-works in this country there is now to be found a testing machine with the necessary apparatus for testing the material. This is intended for the makers' own information, and it is also used by the engineers or their assistants under whose specification the steel is being made. Take, for instance, steel plates for the bridge work under the Indian Government specification. Every plate rolled is tested in the following manner in order to guard against the acceptance of brittle or dangerous steel. As each plate is delivered from the rolls, and before it is sheared to dimensions, four samples in duplicate are marked off in the spare material for testing purposes. Tensile test pieces are taken both lengthwise and across the plate, and similar ones for the quenched bend tests. These test pieces are all stamped with suitable numbers before they are sheared off the plate to be tested. The steel must be of such a strength and quality as to be equal to a tensile strength of between twenty-seven and thirty-one tons per square inch, and to indicate a contraction of the tested area at the point of fracture of not less than 35 per cent.; the percentage of elongation in a length of 10 inches must be not less than 20.

The bend test pieces, heated to a low cherry heat, and cooled in water at a temperature of 82° F., must stand bending double round a curve of which the diameter is not more than three times the thickness of the piece tested. If these requirements are not in every case satisfactory, the plate represented by a defective test is rejected; unless it can be shown that the tested specimen has surface or other defects, in which case the duplicate test-piece is duly tested to take its place.

It must be evident, therefore, that a tremendous amount of testing is now being carried out in this and other countries, and the value of the material, the acceptance or rejection of which depends on the results of the tests, will reach a large amount. On this account we are always glad to increase our knowledge of the behaviour of the material under test, and of the machinery used in the process.

Prof. Unwin is to be congratulated on having successfully fulfilled his task. The work is worthy of his reputation, and should find a place in every engineer's library.

OUR BOOK SHELF.

Entomology for Beginners. For the Use of Young Folks, Fruit-growers, Farmers, and Gardeners. By A. S. Packard, M.D., Ph.D. Small 8vo, pp. xvi. and 367. (New York: Henry Holt and Co., 1888.)

DR. PACKARD'S works on entomological subjects are so well and favourably known, that the book he has now written under the above modest title will certainly obtain a large circulation.

As its name implies, it is a work especially addressed to beginners in the study of entomology, and gives them a brief outline of the extent of the subject, and descriptions of the various methods pursued in its research.

In some respects this work is based upon the same author's "Guide to the Study of Insects," many of the paragraphs being the same in both books and also many of the woodcuts which illustrate them. But the portion relating to classification is much abridged in the smaller book, whilst that which treats of the mounting of microscopic preparations, &c., is given at much greater length. It is this latter part that will be found of more permanent value to the student, who will soon master the brief classification put before him.

The demand for works like the present, we are glad to think, is likely to increase. It is in the examination of the embryology and in the dissection of insects in their various stages of development that the labours of entomologists will be largely engaged when the discovery and description of new species of insects begin to grow slack.

It is when we consider the vastness of the subject of entomology, and the practically inexhaustible field of investigation lying before us, that we feel grateful for such works as Dr. Packard's, which bring before us in a concise form recent methods employed in the treatment of subjects always difficult to manipulate and to render fit for microscopic examination. O. S.

The First Ascent of the Kasai: being some Records of Service under the Lone Star. By C. S. L. Bateman. (London: George Philip and Son, 1889.)

WHEN Lieutenant Wissmann made his memorable descent of the River Kasai, he was accompanied by a number of natives whose good-will he had secured by wise and kindly treatment. At Léopoldville, whence Lieutenant Wissmann returned to Europe, it was decided that Dr. Wolf should take the various members of this escort back to their homes, and that the Expedition under his command should be sent on to establish a station at the confluence of the Lulua with the Luebo. This Expedition was joined by Mr. Bateman as second in command; and in the present volume he records his experiences in the ascent of the Kasai, and during the time when he was engaged in directing the making of the Luebo station. Mr. Bateman is sometimes tempted to indulge rather too much in fine writing, but, notwithstanding this drawback, his book is one of considerable interest. He gives a good general idea of the manner in which he was impressed by the scenery through which he passed or in the midst of which he lived; and he is at great pains to describe, as accurately as possible, the various native tribes with whom he came in contact. Above all, the book is valuable for the light it throws on the influence that is being exerted by the Congo State. Mr. Bateman worked as one of its officials at the Luebo station, and no one who reads the record of what he accomplished can doubt that acting through such agents the new State is preparing the way for the growth of legitimate commerce, and for the development of wholesome relations generally between Europeans and the natives. The volume contains many illustrations, produced from the author's original sketches in pencil, water-colour, pen and ink, or sepia.

Tabular List of all the Australian Birds. By E. P. Ramsay. Pp. 1–38, with Map. (Sydney: Published by the Author, 1888.)

THE present list by Dr. E. P. Ramsay shows that a considerable increase in our knowledge of Australian ornithology has taken place since the year 1878, when the author last issued a list of the birds of Australia. The method adopted in this most useful publication is of the simplest and most effective nature, fourteen natural districts being recognized by Dr. Ramsay, and the distribution of each species being shown by an indication of a number corresponding to the number of the district in the table, so that the known range of every Australian species is seen at a glance. References are given to all the newly-described species, and on the opposite page to each table Dr. Ramsay gives critical notes of great importance to the student. A map is also given which indicates the natural areas of the different provinces of Australia, and enumerates most of the places where well-known collections have been made. A list of the 39 species known from Lord Howe Island and Norfolk Island is also appended, and a new species of owl from the former is described as *Ninox albaria*.

R. B. S.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Meteoric Theory of Nebulæ, &c.

IN his interesting letter (p. 436), Mr. Preston draws attention to a point which was not referred to in my paper.

It has generally been supposed that the incandescence of the trail of a falling star was an evidence of the volatilization of the solid materials of a meteorite, and in the lower regions of the atmosphere this must certainly be true to a greater or less degree. But, in a paper read before the French Academy on February 18 (see abstract in NATURE, February 28, p. 432), M. E. Minary argues that the incandescence of meteorites cannot be explained by the transformation of motion into heat; and, after the reading of the paper, M. Cornu remarked that the luminosity of the trail of a meteorite might be an electrical phenomenon, without any considerable rise of temperature. I quote this in order to show that we should be cautious in accepting Mr. Preston's conclusion as to the rapid retardation and volatilization of meteorites when moving through a highly rarefied gas.

It may be admitted that, when a swarm of meteorites is closely packed, it will soon assume the form of gas throughout, but I cannot believe that gaseous friction affords a valid objection to the meteoric theory, when the meteorites are in loose order. The solidification of volatilized metals would, I suppose, take place in a few minutes or seconds, and the mean interval between collisions was shown, in my paper, to be conveniently measured in hours or days. It would not be unreasonable to suppose that the so-called permanent gases also solidify, when cooled to the low temperature which must obtain. But, apart from this possible solidification, Mr. Lockyer suggests that the permanent gases would be quickly occluded in the solids volatilized at the same instant.

The fusion of meteorites, so as to compensate fractures, forms, as I have myself said, perhaps the most serious difficulty in the theory. It cannot be supposed that fusions take place except under favourable circumstances; but, if a swarm of meteorites does not degrade into dust, these favourable circumstances must occur often enough to counterbalance fractures.

Mr. Preston does not seem to be correct in respect to Clausius's theory of the constant ratio of internal to translatory energy in a gas. Clausius assumes, and does not prove that, in the average of a number of collisions, the molecule will absorb an amount of energy proportional to the mean violence of the blows with which it has been struck. Clausius's law must be at least approximately true within considerable variations of temperature, but it is certainly not a rigorous law of mechanical systems.

The case of meteorites in collision is totally different; they are incapable of taking up more than an infinitesimal amount of vibratory energy. The energy which, in the molecules of a gas, is absorbed, in the case of meteorites goes in volatilizing solids. There seems to be no reason why the particular ratio which Clausius determines from the numerical value of the ratio of the two specific heats (γ) in a gas, should hold good in a swarm of meteorites. We have no idea of the ratio of the two quasi-specific heats in a swarm of meteorites, and therefore we are not entitled to use the equation $\beta = \frac{2}{3}\frac{1}{\gamma - 1}$, and to put $\gamma = 1{\cdot}4$, deducing $\beta = \frac{5}{3}$.[1] In other words, I do not see room for making any deduction as to the ratio between the kinetic energy of a swarm of meteorites and the thermal energy existing in the volatilized gases.

I fail to see any parallelism between Clausius's law and the conclusion drawn as to the lost potential energy in the formation of a swarm of meteorites. The loss of energy is an outcome of the law of gravitation, as applicable to the formation of a gaseous star, and is deducible without any reference to the kinetic theory of gases, or to its analogue for meteorites.

G. H. DARWIN.

[1] Maxwell, "Theory of Heat" (1875), p. 317. Mr. Preston's statement of the law is inaccurate.

The Formation of Ledges on Hill-sides.

IN NATURE for February 28, Mr. Ernst draws attention to the formation of these ledges as observed by himself in Caracas. They are probably to be found in many places, if carefully looked for. The following diagram, taken from a photograph, illustrates one of the most striking instances I know, to be found near Ballantrae, on the Ballantrae–Girvan road, Ayrshire. The ledges, which are very numerous and fairly regular, occur on the western face of a series of low hills, very near to the sea-shore. The subsoil is thin and open. The angle of slope ranges from perhaps 30° to 60°, seldom higher. Where the angle is much higher, the soil slips away bodily, and the grass with it, leaving a bare space; indeed, at one point of the road the precipitated soil forms readily visible mounds at the base of the cliff.

The whole locality is very unfavourable to earthworms, and I agree with Mr. Ernst that the earthworm theory must, as far as any practical effect is concerned, be surrendered.

On the other hand, had the ridges been due to anything like glacier action, as Mr. Ernst suggests, I should have expected

detrital mounds below the ledges at the foot of each slope. These, however, do not occur, and the soil meets the narrow strip of plain with surprising angular sharpness.

It seems clear that the ledges owe their origin to the action of rain-water, which would naturally penetrate below the surface covering of grass, and dissolve with comparative rapidity portions of the porous soil below. The grass layer would eventually have nothing to support it in places, and would collapse to a lower level. The effect of collapse, supposing the layer to hold together, would necessarily be a wrinkle or ledge at right angles to the ground slope. EDMUND J. MILLS.

Glasgow, March 4.

Weight, Mass, and Force.

IF Mr. Gray, as in his teaching he no doubt unconsciously often does, will always say "force of a pound" instead of "weight of a pound" when he wishes to express the force of attraction of the Earth on a pound weight, there will be no divergence between his theoretical instruction and the language of practical men and of every-day life.

But to the majority the expression "weight of a pound" will always call up the mental picture of a "pound weight," so that the idea of the mass of a pound and of the force with which it is attracted by the earth cannot be dissociated in the use of the word "weight."

Supposing, however, we accept the definition of the "weight of a body" as never meaning anything else than the "force with which the earth attracts the body," how are we to interpret "the weight of the Sun, of the Moon, of Jupiter, &c.," and what is the "weight of the Earth?"

As Mr. Gray declines my previous challenge, will he condescend to point out the fallacy in the following argument? "The weight of the Moon being the force with which the Moon is attracted by the Earth, *ergo*, by the law that Action and Reaction are equal and opposite, the weight of the Earth is equal to the weight of the Moon."

With our present system of instruction in elementary theoretical dynamics we run the risk of wasting our time on a mechanics which is as unreal as is the mediæval Greek grammar taught in our schools, a grammar that was never vernacular even in the palmiest days of Attic literature.

The warning note in the introduction to "Numerical Examples in Practical Mechanics," by R. G. Blaine, is well timed and deserves careful attention. A. G. GREENHILL.

The Inheritance of Acquired Characters.

A VERY strong *a priori* objection to the line on which most experiments on the inheritance of acquired characters are carried on is the following. These experiments involve mutilation; and a tendency to transmit characters so produced would, considering that every accident or fight produces some slight mutilation, involve the animals in a process of degeneration. Hence the tendency to transmit the characters acquired *by mutilation* would be constantly bred out by natural selection. But a tendency to transmit characters acquired *by habit* in youth rests on quite another basis, and would tend to the conservation of the race.

I do not know if observations have been made on the physique of the offspring of persons engaged in trades where apprenticeship begins before puberty: they would be most valuable.

But the following case seems to me to be thoroughly to the point. A. B. is moderately myopic and very astigmatic in the left eye; extremely myopic in the right. As the left eye gave such bad images for near objects, he was compelled in childhood to mask it, and acquired the habit of leaning his head on his left arm for writing, so as to blind that eye; or of resting the left temple and eye on the hand, with the elbow on the table. At the age of fifteen the eyes were equalized by the use of suitable spectacles, and he soon lost the habit completely and permanently. He is now the father of two children—a boy and a girl—whose vision (tested repeatedly and fully) is emmetropic in both eyes, so that they have not inherited the *congenital* optical defect of their father. All the same, they both have inherited his early acquired habit, and need constant watchfulness to prevent their hiding the left eye, when writing, by resting the head on the left fore-arm or hand. Imitation is here quite out of the question.

Considering that every habit involves changes in the proportional development of the muscular and osseous systems, and hence, probably, of the nervous system also, the importance of inherited habits, natural or acquired, cannot be

overlooked in the general theory of inheritance. I am fully aware that I shall be accused of flat Lamarckism; but a nickname is not an argument. MARCUS M. HARTOG.

Cork, March 6.

A Fine Meteor.

A FINE meteor was visible here to-night at 6.36 p.m. It fell perpendicularly almost due north-north-east, disappearing about 20° above the horizon, and was then as nearly as possible of the brilliancy and colour of Venus, which was shining in the south-west at the time. Length of path, I think, about 20°, but I am not positive that I saw the beginning of it.

B. WOODD SMITH.

Hampstead Heath, N.W., March 11.

Bishop's Ring.

I AM informed by Miss E. Brown, of Cirencester, that she saw Bishop's ring in full day-time as recently as last month, not far from 12 o'clock one day, the sun being hidden behind a cloud at the time. It appeared very similar in extent, as well as colour, though not intensity, to what it did after the Krakatão eruption. Also another day she noticed something like it when looking at the sun simply through a dark glass.

I have very occasionally myself in the last few years seen a somewhat similar phenomenon, but duller and dirtier in colour than Bishop's ring usually was, and which gave me the impression of being lower down in the atmosphere than that. I attributed this to smoke or some other local impurity. It would appear that the phenomenon seen by Miss Brown, out in the country, cannot have been thus caused; but I suppose that she must really have seen the corona produced by volcanic dust, through an exceptionally pure lower atmosphere.

Sunderland, March. T. W. BACKHOUSE.

The Philosophical Transactions.

MOST people who interest themselves in science would be glad to possess a complete set of the Philosophical Transactions. But in the first place complete sets are scarce, and in the second it would be much too expensive for ordinary people; furthermore, it is not in everyone's library that room could be found for its reception. Now, Drs. Hutton, Shaw, and Pearson brought out a first-rate abridgment of this valuable publication from its commencement to the end of the year 1800. May I venture to suggest that the Royal Society would be doing a good work by publishing on the same lines a continuation up to the year 1900? If they commenced now, and brought out a volume at intervals, the whole thing might be completed in the early years of the twentieth century. The cost ought not to be very great, and probably nearly every free library would subscribe. S.

ON THE COMPOSITION OF WATER.[1]

DURING the past year I have continued the work described in a former communication on the relative densities of hydrogen and oxygen (Roy. Soc. Proc., February 1888, vol. xliii. p. 356; see also NATURE, vol. xxxvii. p. 418), in the hope of being able to prepare lighter hydrogen than was then found possible. To this end, various modifications have been made in the generating apparatus. Hydrogen has been prepared from potash, in place of acid. In one set of experiments the gas was liberated by aluminium. In this case the generator consisted of a large closed tube sealed to the remainder of the apparatus; and the aluminium was attached to an iron armature so arranged that, by means of an external electro-magnet, it was possible to lower it into the potash, or to remove it therefrom. The liberated gas passed through tubes containing liquid potash,[2] corrosive sublimate, finely powdered solid potash, and, lastly, a long length of phosphoric anhydride. But the result was disappointing, for the hydrogen proved to be no lighter than that formerly obtained from sulphuric acid.

I have also tried to purify hydrogen yet further by absorption in palladium. In his recent important memoir (*Amer. Chem. Journ.*, vol. x. No. 4), "On the Combustion of Weighed Quantities of Hydrogen and the Atomic Weight of Oxygen," Mr. Keiser describes experiments from which it appears that palladium will not occlude nitrogen—a very probable impurity in even the most carefully prepared gas. My palladium was placed in a tube sealed, as a lateral attachment, to the middle of that containing the phosphoric anhydride; so that the hydrogen was submitted in a thorough manner to this reagent, both before and after absorption by the palladium. Any impurity that might be rejected by the palladium was washed out of the tube by a current of hydrogen before the gas was collected for weighing. But as the result of even this treatment I have no improvement to report, the density of the gas being almost exactly as before.

Hitherto the observations have related merely to the densities of hydrogen and oxygen, giving the ratio 15·884, as formerly explained. To infer the composition of water by weight, this number had to be combined with that found by Mr. Scott as representing the ratio of volumes. The result was—

$$\frac{2 \times 15\cdot884}{1\cdot9965} = 15\cdot914.$$

The experiments now to be described are an attempt at an entirely independent determination of the relative weights by actual combustion of weighed quantities of the two gases. It will be remembered that in Dumas's investigation the composition of water is inferred from the weights of the oxygen and of the water, the hydrogen being unweighed. In order to avoid the very unfavourable conditions of this method, recent workers have made it a point to weigh the hydrogen, whether in the gaseous state, as in the experiments of Prof. Cooke and my own, or occluded in palladium, as in Mr. Keiser's practice. So long as the hydrogen is weighed, it is not very material whether the second weighing relate to the water or to the oxygen. The former is the case in the work of Cooke and Keiser, the latter in the preliminary experiments now to be reported.

Nothing could be simpler in principle than the method adopted. Globes of the same size as those employed for the density determinations are filled to atmospheric pressure with the two gases, and are then carefully weighed. By means of Sprengel pumps the gases are exhausted into a mixing chamber, sealed below with mercury, and thence by means of a third Sprengel are conducted into a eudiometer, also sealed below with mercury, where they are fired by electric sparks in the usual way. After sufficient quantities of the gases have been withdrawn, the taps of the globes are turned, the leading tubes and mixing chamber are cleared of all remaining gas, and, after a final explosion in the eudiometer, the nature and amount of the residual gas are determined. The quantities taken from the globes can be found from the weights before and after operations. From the quantity of that gas which proved to be in excess, the calculated weight of the residue is subtracted. This gives the weight of the two gases which actually took part in the combustion.

In practice, the operation is more difficult than might be supposed from the above description. The efficient capacity of the eudiometer being necessarily somewhat limited, the gases must be fed in throughout in very nearly the equivalent proportions; otherwise there would soon be such an accumulation of residue that no further progress could be made. For this reason nothing could be done until the intermediate mixing chamber was provided. In starting a combustion, this vessel, originally full of mercury, was charged with

[1] A Paper read at the Royal Society, by Lord Rayleigh, Sec. R.S., on March 7.

[2] Of course, this tube was superfluous in the present case, but it was more convenient to retain it.

equivalent quantities of the two gases. The oxygen was first admitted until the level of the mercury had dropped to a certain mark, and subsequently the hydrogen down to a second mark, whose position relatively to the first was determined by preliminary measurements of volume. The mixed gases might then be drawn off into the eudiometer until exhausted, after which the chamber might be re-charged as before. But a good deal of time may be saved by replenishing the chamber from the globes simultaneously with the exhaustion into the eudiometer. In order to do this without losing the proper proportion, simple mercury manometers were provided for indicating the pressures of the gases at any time remaining in the globes. But even with this assistance close attention was necessary to obviate an accumulation of residual gas in the eudiometer, such as would endanger the success of the experiment, or, at least, entail tedious delay. To obtain a reasonable control, two sparking places were provided, of which the upper was situate nearly at the top of the eudiometer. This was employed at the close, and whenever in the course of the combustion the residual gas chanced to be much reduced in quantity; but, as a rule, the explosions were made from the lower sparking point. The most convenient state of things was attained when the tube contained excess of oxygen down to a point somewhat below the lower sparking wires. Under these circumstances, each bubble of explosive gas readily found its way to the sparks, and there was no tendency to a dangerous accumulation of mixed gas before an explosion took place. When the gas in excess was hydrogen, the manipulation was more difficult, on account of the greater density of the explosive gas retarding its travel to the necessary height.

In spite of all precautions several attempted determinations have failed from various causes, such as fracture of the eudiometer and others which it is not necessary here to particularize, leading to the loss of much labour. Five results only can at present be reported, and are as follows:—

December 24, 1888	... ...	15·93
January 3, 1889	... ...	15·98
,, 21, ,,	... ...	15·98
February 2, ,,	... ...	15·93
,, 13, ,,	... ...	15·92
Mean	... ...	15·95

This number represents the atomic ratio of oxygen and hydrogen as deduced immediately from the weighings with allowance for the unburnt residue. It is subject to the correction for buoyancy rendered necessary by the shrinkage of the external volume of the globes when internally exhausted, as explained in my former communication.[1] In these experiments, the globe which contained the hydrogen was the same (14) as that employed for the density determinations. The necessary correction is thus four parts in a thousand, reducing the final number for the atomic weight of oxygen to—

15·89,

somewhat lower than that which I formerly obtained (15·91) by the use of Mr. Scott's value of the volume ratio. It may be convenient to recall that the corresponding number obtained by Cooke and Richards (corrected for shrinkage) is 15·87, while that of Keiser is 15·95.

In the present incomplete state of the investigation, I do not wish to lay much stress upon the above number, more especially as the agreement of the several results is not so good as it should be. The principal source of error, of a non-chemical character, is in the estimation of the weight of the hydrogen. Although this part of the work cannot be conducted under quite such favourable conditions as in the case of a density determination, the error in the difference of the two weighings should not exceed 0·0002 gramme. The whole weight of the hydrogen used is about 0·1 gramme;[1] so that the error should not exceed 3 in the last figure of the final number. It is thus scarcely possible to explain the variations among the five numbers as due merely to errors of the weighings.

The following are the details of the determination of February 2, chosen at random:—

Before combustion ... $G_{14} + H + 0·2906 = G_{11}$ ... pointer 20·05
After ,, ... $G_{14} + H + 0·4006 = G_{11}$ ... pointer 20·31

Hydrogen taken = 0·1100 − 0·0000$_5$ = 0·10995 gramme.

Before combustion ... $G_{13} + O = G_{11} + 2·237$ ... pointer 20·00
After ,, ... $G_{13} + O = G_{11} + 1·357$ ... pointer 19·3

Oxygen taken = 0·8800 + 0·0001 = 0·8801 gramme.

At the close of operations the residue in the eudiometer was oxygen, occupying 7·8 cubic centimetres. This was at a total pressure of 29·6 − 16·2 = 13·4 inches of mercury. Subtracting 0·4 inch for the pressure of the water vapour, we get 13·0 as representing the oxygen pressure. The temperature was about 12° C. Thus, taking the weight of a cubic centimetre of oxygen at 0° C., and under a pressure of 76·0 centimetres of mercury to be 0·00143 gramme, we get as the weight of the residual oxygen—

$$0·00143 \frac{7·8}{1 + 12 \times 0·00367} \frac{13·0 \times 2·54}{76·0} = 0·0046 \text{ gramme.}$$

The weight of oxygen burnt was, therefore, 0·8801 − 0·0046 = 0·8755 gramme.

Finally, for the rate of atomic weight—

$$\frac{\text{Oxygen}}{\frac{1}{2}\text{ Hydrogen}} = 15·926.$$

In several cases the residual gas was subjected to analysis. Thus, after the determination of February 2, the volume was reduced by additions of hydrogen to 1·2 cubic centimetre. On introduction of potash there was shrinkage to about 0·9, and, on addition of pyrogallic acid, to 0·1 or 0·2. These volumes of gas are here measured at a pressure of $\frac{1}{3}$ atmosphere, and are, therefore, to be divided by 3 if we wish to estimate the quantities of gas under standard conditions. The final residue of (say) 0·05 cubic centimetre should be nitrogen, and, even if originally mixed with the hydrogen—the most unfavourable case—would involve an error of only $\frac{1}{2000}$ in the final result. The 0·1 cubic centimetre of carbonic anhydride, if originally contained in the hydrogen, would be more important, but this is very improbable. If originally mixed with the oxygen, or due to leakage through india-rubber into the combustion apparatus, it would lead to no appreciable error.

The aggregate impurity of 0·15, here indicated, is tolerably satisfactory in comparison with the total quantity of gas dealt with—2000 cubic centimetres. It is possible, however, that nitrogen might be oxidized, and thus not manifest itself under the above tests. In another experiment the water of combustion was examined for acidity, but without definite indications of nitric acid. The slight reddening observed appeared to be rather that due to carbonic acid, some of which, it must be remembered, would be dissolved in the water. These and other matters demand further attention.

The somewhat complicated glass-blowing required for the combustion apparatus has all been done at home by my assistant, Mr. Gordon, on whom has also fallen most of the rather tedious work connected with the evacuation of globes and other apparatus, and with the preparation of the gases.

[1] The necessity of this correction was recognized at an early stage, and, if I remember rightly, was one of the reasons which led me to think that a re-determination of the density of hydrogen was desirable. In the meantime, however, the question was discussed by Agamennone (*Atti* (*Rendiconti*) *d. R. Accad. dei Lincei*, 1885), and some notice of his work reached me. When writing my paper last year I could not recall the circumstances; but since the matter has attracted attention I have made inquiry, and take this opportunity of pointing out that the credit of first publication is due to Agamennone.

[1] It was usual to take for combustion from two-thirds to three-fourths of the contents of the globe.

EXAMINATIONS IN ELEMENTARY GEOMETRY.

IN the spring of 1870 a letter appeared in the columns of NATURE suggesting the formation of an Association which should have for its object the improvement of geometrical teaching. As to the great desirability of such improvement there could be no doubt. It was felt that one of the greatest obstacles in the way of effecting it was the nature of the examinations, and that no change in that nature was likely to be brought about except by the combined action of those who desired it. The idea took root, and a circular was drawn up in which the objects of the proposed Association were stated to be—

(1) To collect and distribute information as to the prevailing methods of instruction in geometry practised in this and other countries, and to ascertain whether the desire for change was general.

(2) To use its influence to induce examining bodies to frame their questions in geometry without reference to any particular text-book.

(3) To stamp with its approval some text-book already published, or to bring out a new one under its own auspices.

The movement led to a conference at University College in 1871, which resulted in the formation of the Association for the Improvement of Geometrical Teaching. Many difficulties beset the way of the desired improvement. In addition to all the arguments which may be used against radical change in any department of affairs, the uncomprising supporters of Euclid (or rather of Simson's edition of his works) had the very cogent one that a Commission appointed by the Italian Government to inquire into the state of geometrical teaching in Italy found it "so unsatisfactory, and the number of bad text-books so great and so much on the increase, as to compel them to recommend the adoption of Euclid pure and simple,"[1] and were able to quote the authority of distinguished French mathematicians as to the great merits of Euclid as compared with modern French writers. We need not wonder, then, and need perhaps scarcely regret, that the outward and direct progress of the Association towards the realization of the aims set forth in its programme was slow.

One of its first steps was to attempt to draw up a Syllabus of Geometry in place of Euclid's scheme of propositions, which might form the basis of future text-books. Members were requested to send in, and the Committee of Management to report upon, programmes of the subjects which a text-book ought to include. Eleven programmes were sent in, which agreed upon many fundamental points; and after much work by a sub-committee, and much discussion from time to time at the general meetings of the Association, a Syllabus of Geometry was at last published as the work of the Association. It is worthy of remark that a Committee of the British Association, containing such eminent mathematicians as Profs. Cayley, Clifford, H. J. S. Smith, and Sylvester, appointed for the purpose of considering the possibility of improving the methods of instruction in elementary geometry, and reappointed to consider this Syllabus, reported that "it appears to have been drawn up with such care and with such regard to the essential conditions of the problem as to render it highly desirable that it should be considered in detail by authorized representatives of the Universities and other great examining bodies of the United Kingdom, with a view to its adoption, subject to any modification which such detailed consideration may show to be necessary, as the standard for examinations."

The secretaries then applied to many of the principal examining bodies, submitting the Syllabus for their consideration, with a view to its adoption as the basis of examination.

[1] See First Annual Report of the A.I.G.T.

The direct result of this application was small. It was felt that the non-existence of a text-book based on the Syllabus was a bar to its adoption, and a sub-committee was appointed to prepare proofs of the propositions. A text-book embodying these, and entitled "The Elements of Plane Geometry," was published by Messrs. Sonnenschein and Co. The appearance of Part II. of this work in 1886 seemed to afford a fitting opportunity for memorializing the Universities as to the advisability of relaxing their regulations. The following petition was drawn up, and received about 180 signatures, among which may be noticed those of Sir R. S. Ball, Prof. Chrystal, Prof. Henrici, Dr. Hirst, and Prof. Tait :—

"We, the undersigned members of the Association for the Improvement of Geometrical Teaching, and others interested in the teaching of elementary geometry, believing that greater freedom in the teaching of that subject than is consistent with a rigid adherence to the letter of Euclid's 'Elements' is highly desirable, would welcome such a change in the examinations in elementary geometry conducted by the Universities and other examining bodies as would admit of the subject being studied from text-books other than editions of Euclid, without the student being thereby placed at a disadvantage in those examinations."

The Council forwarded the petition to the Universities, praying them to take the subject of the petition into their favourable consideration, with a view to adapting the regulations for the examinations in elementary geometry conducted by the Universities so as to meet the desire for greater freedom felt by a large number of teachers, and supported by the judgment of many eminent mathematicians.

The Hebdomadal Council of the University of Oxford having referred the application of the Association to the Board of the Faculty of Natural Science, the Board reported as follows :—

(1) "That a rigid adherence to the ordinary text-books of Euclid should no longer be insisted on, but that a greater freedom of demonstration should be allowed, both in geometrical teaching and in examination.

(2) "That, nevertheless, Euclid's *method* should be required in all pass examinations in geometry in so far as that no axioms other than those of Euclid shall be admitted, and that no proof of a proposition be allowed which assumes the truth of any proposition which does not precede it according to Euclid's order.

(3) "That the University should not prescribe the use of any particular text-book or text-books."

This Report was adopted by the Hebdomadal Council on June 20, 1887.

The Senate of the University of Cambridge having referred to the Special Board for Mathematics, the Board reported as follows :—

"The majority of the Board are of opinion that the rigid adherence to Euclid's text is prejudicial to the interests of education, and that greater freedom in the method of teaching geometry is desirable. As it appears that this greater freedom cannot be attained while a knowledge of Euclid's text is insisted upon in the examinations of the University, they consider that such alterations should be made in the regulations of the examinations as to admit other proofs besides those of Euclid, while following, however, his general sequence of propositions, so that no proof of any proposition occurring in Euclid should be accepted in which a subsequent proposition in Euclid's order is assumed.

"To give effect to this view, the Board recommend that for Regulation 10 (2) of the previous examination (*Ordinationes*, p. 8) the following be substituted :—

"Elementary geometry, viz. the substance of the first three books, the definitions 1–10 of Book V., and the substance of the first nineteen propositions of the Sixth Book of Euclid's 'Elements.' Euclid's definitions will be

required, and no axioms or postulates except Euclid's may be assumed. The actual proofs of propositions as given in Euclid will not be required, but no proof of any proposition occurring in Euclid will be admitted in which use is made of any proposition which in Euclid's order occurs subsequently."

The subject came on for discussion in the School on January 26, 1888, and a grace finally passed the Senate (March 8, 1888), making the alteration in the regulations for the previous examination recommended in the Report.

The Council of the Association then forwarded an Address to Her Majesty's Civil Service Commissioners, drawing their attention to the recent Reports of the University Boards, and asking for corresponding alterations in the regulations for the Woolwich and Sandhurst examinations, as follows:

"In view of the above facts, the Council feel themselves justified in inviting the attention of Her Majesty's Civil Service Commissioners to the desirability of a change in one of the existing regulations for the examinations conducted under their authority, especially those for admission to Sandhurst and Woolwich. By this a candidate is required to 'satisfy the Commissioners in Euclid, Books I.-IV. and VI.' The Council, having reason to believe that this regulation is very generally understood to imply that Euclid's text is required without any but mere verbal variations, beg to suggest that the subject should be defined as 'Elementary Geometry to the extent of Books I.-IV. and VI. of Euclid's "Elements,"' with a note to the effect that 'any proofs will be admitted which are themselves sound, do not assume other axioms than those of Euclid, and are not inconsistent with the logical sequence of Euclid's theorems.'

"The Council would prefer to have no particular text-book prescribed, and to have the candidate's general knowledge of geometry tested, rather than his power of reproducing Euclid's text.

"While making this request, however, the Council wishes to express its acknowledgment of the high character of the papers in Euclid set by the Civil Service Examiners, and particularly of the frequent introduction of stimulative exercises on the book work set.

"The Council would esteem it a favour if they were allowed to express their views on this important question more at large by a deputation from their body at an interview with the Commissioners."

The following reply was received from the Commissioners:—

"*Civil Service Commission, June* 30, 1888.

"Sir,—I am directed by the Civil Service Commissioners to acknowledge the receipt of a circular signed by you in behalf of the Council of the Association for the Improvement of Geometrical Teaching, requesting that certain changes may be introduced into the regulations for the examinations held under their authority, and in particular those for admission to Sandhurst and Woolwich;

"And, in reply, I am to acquaint you, for the information of the Council, that the regulations for the military examinations are framed and issued by the military authorities, and that the Commissioners have no power to alter them, but that the interpretation which the Commissioners have given them has been substantially in accordance with the views expressed by the Cambridge Special Board for Mathematics and the Oxford Board of the Faculty of Natural Science. They have, that is to say, instructed their occasional examiners to make no deduction from the marks allotted to a question because a candidate substitutes another proof for Euclid's, if this proof is a sound one and keeps to Euclid's sequence of propositions. To remove any misconception which may exist, the Commissioners propose to introduce a note to this effect in the examination papers. It will be understood, however, that they do not intend thereby to fetter their discretion of changing their practice whenever they may consider it expedient.

"In conclusion, while thanking the Council for their offer to express their views on this question more at large by the medium of a deputation from their body, the Commissioners think that the agreement of their opinion on this matter renders it unnecessary that the members of the Council should give themselves the trouble of an interview.

"I have the honour to be, Sir,
"Your obedient servant,
"E. POSTE."

Doubtless these concessions are in form the sorriest minimum of change that could well be granted; but the spirit of the Reports is excellent, particularly if examiners allow themselves, as it is quite open for them to do, to interpret "Euclid's order" as his *logical* and not merely his *numerical* order. Probably, if there is a strong desire for further relaxation generally felt and expressed by teachers, there will be no great difficulty in getting it granted; but it is by no means clear that such a desire exists at present to a sufficient degree to bring pressure to bear on the authorities. One does not easily see why the sequence of the Syllabus and the "Elements" should not have been granted as an alternative to that of Euclid, as is done by the University of Edinburgh. These works are the outcome of considerable discussion and deliberation by practised teachers, and it seems scarcely right to ignore them so entirely. As far as Cambridge is concerned, there is one detail of reform which might be effected without going beyond the terms of the grace of the Senate; *i.e.* the authorization of a list of additional propositions not contained in Euclid's text, to be freely used in the demonstration of all propositions subsequent to the position they may be regarded as occupying in Euclid's sequence. Such a list need not be long, and its adoption would be a boon to those teachers who wish to use the Syllabus.

ELECTROSTATIC MEASUREMENT.[1]

THE number of electrostatic units of potential or electromotive force in the electro-magnetic unit of potential is essentially a velocity, and experiments have proved it to be so nearly equal to the velocity of light that from all the direct observations hitherto made we cannot tell whether it is a little greater than, or a little less than, or absolutely equal to, the velocity of light.

Sir W. Thomson is engaged now in making a set of electrometers which will measure by electrostatic force potentials of from 40 volts to 50,000 volts. The standardization of these instruments up to 200 or 300 volts is made exceedingly easy, by aid of his centiampere balance and continuous rheostat, with a voltaic battery of any kind, primary or secondary, capable of giving a fairly steady current of $\frac{1}{10}$ of an ampere through it and the platinoid resistance in series with it. The accuracy of the electro-magnetic standardization, within the range of the direct application of this method, is quite within $\frac{1}{20}$ per cent. A method of multiplication by aid of condensers, which was explained, gives an accuracy quite within $^1/_5$ per cent. for the measurement in volts up to 2000 or 3000 volts; and with not much less accuracy, by aid of an intermediate electrometer, up to 10,000 volts. Such a potential as 10,000 volts is convenient for measurement by an absolute electrostatic balance, which was fully explained in the lecture, and illustrated by a drawing. But hitherto he has not been able to make sure of the absolute accuracy of the electrostatic balance to closer than $^1/_2$ per cent. The results of a great number of measurements made in the Physical Laboratory of the University of Glasgow during the last

[1] Abstract of part of Sir William Thomson's Royal Institution Lecture of Friday evening, February 8, relating to the velocity of light.

two months, give the required number, commonly called "v," within $1/2$ per cent. of 300,000 kilometres per second; the velocity of light is known to be within $1/4$ per cent. of 300,000 kilometres per second. Results of previous observers for determining "v" had almost absolutely proved at least as close an agreement with the 300,000,000 metres as this. He expressed his obligations to his assistants and students in the Physical Laboratory of Glasgow University, Messrs. Meikle, Shields, Sutherland, and Carver, who worked with the greatest perseverance and accuracy, in the laborious and often irksome observations by which he had attempted to determine "v" by the direct electrometer method, as exactly as, or more exactly than, it has been determined by other observers and other methods.

The measurement of "v" by Sir William Thomson and Profs. Ayrton and Perry, communicated to the British Association at Bath, was too small (292) on account of the accidental omission of a correction regarding the effective area of the attracted disk in the absolute electrometer. When this correction is applied their result is brought up to 298, which exactly agrees with Profs. Ayrton and Perry's previous determination by another method, in Japan. Prof. J. J. Thomson's result is 296·3. It is understood that Rowland has found 299. The result of Sir William Thomson's latest observations, founded wholly on the comparison of electrometric and electro-magnetic determinations of potential in absolute measure, is 30·1 legal ohms, or 30·04 Rayleigh ohms. Assuming, as is now highly probable, that the Rayleigh ohm is considerably nearer than the legal ohm to the true ohm, the result for "v" is 300,400,000 metres per second. Sir William does not consider that this result can be trusted as demonstrating the truth within $1/3$ per cent.

NOTES.

THE Royal Society's Bakerian Lecture for the present year is to be "On a Magnetic Survey of the British Isles for the epoch January 1, 1886," by Prof. A. W. Rücker, F.R.S., and Prof. T. E. Thorpe, F.R.S.

CAPTAIN W. J. L. WHARTON, R.N., F.R.S., Hydrographer to the Admiralty, has been elected a member of the Athenæum Club, in accordance with the rule which empowers the Committee to elect annually a certain number of persons of distinguished eminence in literature, science, or art, or for public services.

THE American Association for the Advancement of Science will meet this year at Toronto from August 27 to September 3. It is expected that the attendance will rival that at the Boston meeting of 1880. The President of the Association is Prof. Mendenhall. Major Powell, as retiring President, gives the address.

THE French Association for the Advancement of Science will meet at Paris from August 8 to 15.

THE French Ministry of Public Instruction has decided to create a laboratory of pathological physiology at the École des Hautes Études, Paris. The Director will be M. François-Franck, assistant of M. Marey at the Collège de France.

DR. SELMAR SCHÖNLAND, of the Botanic Garden, Oxford, has been appointed to the Curatorship of the Albany Museum, Grahamstown, Cape Colony.

IN the spring, M. Hasselberg will go from Pulkowa to Stockholm, having been elected Fellow of the Royal Academy of Sciences, and Director of the Physical Institution of the Academy, in succession to Prof. Edlund.

THE Bombay Anthropological Society has resolved to communicate with the executors of Mr. E. T. Leith, the founder o the Society, with a view to secure the publication of his literary remains under the supervision and at the expense of the Society.

FURTHER experiments are to be carried out shortly at Chatham by the Balloon Department with the Bruce system of electric balloon signalling. The apparatus used will be that which the Government purchased of the inventor, Mr. Eric Stuart Bruce.

WE regret to hear of the death of Captain John Ericsson, the famous Swedish engineer. He died the other day at New York, at the age of eighty-six. Captain Ericsson was a man of extraordinary intellectual resource, and his name stands high among the great inventors of the present century. An effective screw-propeller was invented independently both by him and by Francis Pettit Smith. Smith's patent was taken out in May 1836, a little before Ericsson's; but the date of Ericsson's invention was considerably earlier than that of his English rival. The screw-propeller of Ericsson was at once adopted by the United States Navy, but in England he had the mortification of being officially informed that it was useless, because, "the power being applied at the stern, it would be absolutely impossible to make the vessel steer." Afterwards he had occasion to learn that an inventor's difficulties may be not less formidable in the New World than in the Old, for he was badly treated in connection with the *Princeton* screw steamer, designed by him for the United States Government in 1844, and in connection with the *Monitor*, which he built during the Civil War. During his last years he was much occupied with what he called "the sun motor," an article on which, by himself, recently appeared in NATURE (vol. xxxviii. p. 319).

DR. JOHN CALL DALTON, the physiologist, died at New York on February 12, at the age of sixty-four.

AFTER the ordinary meeting of the Royal Meteorological Society, at 25 Great George Street, Westminster, on Wednesday, the 20th inst., the Fellows and their friends will inspect the Society's tenth annual Exhibition of Instruments. The Exhibition will remain open until Friday, the 22nd inst. It promises to be very interesting and instructive. It will be specially devoted to actinometers and solar radiation apparatus, but will also include several new instruments, and a number of photographs of flashes of lightning, clouds, &c. Persons, not Fellows, wishing to visit the Exhibition, can obtain tickets on application to Mr. W. Marriott, Royal Meteorological Society, 30 Great George Street, S.W.

WE have received, from the Meteorological Reporter to the Government of India, the "Registers of Original Observations" made at seven selected observatories during the months of January to July 1888. These observations, although not exactly in the form prescribed, are published in pursuance of a decision of the Meteorological Congress held at Vienna in 1873, that each country should publish daily observations for a certain number of stations. The Indian observations have been published in this monthly form since January 1879, while for the years 1875-78 they formed an appendix to the Annual Reports. They contain complete information of all the principal elements for four hours daily, together with daily means, and monthly means for each of the four hours. The registers also contain means of the chief elements for each hour of the day at Alipore (Calcutta), and the hourly movement of the wind at Lucknow and Nagpur. The more rapid dissemination of the information by the publication of monthly parts, instead of annual volumes, renders it more valuable to persons interested in meteorological investigations.

M. KLOSSOVSKI, who published some time ago an important work on the thunderstorms of Russia, has now made another valuable contribution to the meteorology of South

Russia, dealing with the amounts of rain and snow in the Governments of Kherson and Bessarabia (Memoirs of the Odessa Society of Naturalists, vol. xiii. Part 1). Pluviometric observations were made in 1887 at 158 stations, and 1160 descriptions of thunderstorms have been sent in. It appears that the average yearly amount of rain and snow in the Government of Kherson is about 411 millimetres (434 millimetres at Odessa), a line drawn from the mouth of the Dniester to Orenburg separating the regions which receive more than 400 millimetres of rain every year from those in the south which have less than that. The Caucasus shore of the Black Sea, with its heavy rains (1500 to 2000 millimetres every year), makes, of course, an exception to the rule. The rains and the snow are usually brought to Bessarabia by cyclones, those which come from the south bringing with them the heaviest rains. No fewer than 96 per cent. of the cyclones which come from Hungary and the Balkan Peninsula are sure to bring with them more or less heavy rains to Kherson, and the like is true of 51 per cent. of those which follow a course to the south of Odessa altogether. The approach of cyclones can be easily foreseen, but the changes of weather are so sudden in South Russia that storm-warnings ought to be given from Odessa instead of St. Petersburg.

AN earthquake was noticed at Kasina, in Croatia, on February 23. At Aquila, in the Abruzzi, on February 28, five shocks occurred between 4 and 5 a.m. No damage was done.

A SLIGHT shock of earthquake occurred in Eastern Pennsylvania on March 8. It was felt at Lancaster, Harrisburg, Gettysburg, and their environs, and also at Wilmington (Delaware), and several places in Maryland and New Jersey.

ANOTHER beautiful instance of the formation of geometrical isomers, compounds precisely similar in constitution, and differing only in the relative position of their atoms in space, has been discovered by Prof. Wislicenus and Herr Hölz. The compound in question is dibromide of crotonylene, $CH_3—CBr=CBr—CH_3$. Crotonylene itself, $CH_3—C\equiv C—CH_3$, is the third member of the acetylene series of hydrocarbons, and combines directly with bromine to form a dibromide of the above constitution, which is now shown to have its atoms arranged in space in the manner

$$\begin{array}{l} CH_3—C—Br \\ \quad\;\; \| \\ CH_3—C—Br \end{array}$$

, the two similar groups being symmetrical to an imaginary plane lying between them. On attempting, however, to prepare crotonylene dibromide from the tetrabromide by abstraction of two atoms of bromine, or from one of the tribrombutanes, $CH_3—CHBr—CBr_2—CH_3$, by splitting off a molecule of hydrobromic acid, quite a different substance was obtained. Analyses indicated exactly the same empirical formula as before, but the boiling-point was found to be about 3° higher than that of the first isomer, and its behaviour with zinc dust was markedly different. These facts can only be explained on the supposition that its arrangement in space differs in being centro-symmetrical,

$$\begin{array}{l} CH_3—C—Br \\ \quad\;\; \| \\ \;\;Br—C—CH_3 \end{array}$$

, and this second isomer is therefore termed iso-crotonylene dibromide. Both compounds combine directly with a further quantity of bromine to form the same crystalline tetrabromide, $CH_3—CBr_2—CBr_2—CH_3$. In order to prepare the ordinary compound, the equivalent of a molecule of liquid bromine is allowed to drop from the fine jet of a dropping-funnel into a quantity of crotonylene contained in a flask surrounded by a freezing mixture, as crotonylene boils at 18° C. The reaction is very violent, each drop producing loud hissing; the product is washed with soda, dried over calcium chloride, and distilled. After two fractionations, the dibromide is obtained boiling constantly between 146° and 147°. On the addition of another molecule of bromine, the whole solidifies in large octahedral crystals of the tetrabromide. The iso-compound is best obtained by dropping in a similar manner one molecular equivalent of bromine into cooled monobrom-butylene; distillation of the resulting product yields tribrom-butane. This is diluted with alcohol, and a molecular equivalent of sodium ethylate added: a most violent reaction again occurs with elimination of a molecule of hydrobromic acid. On addition of water, the iso-compound is precipitated as an oil, which, on drying and subsequent distillation, is found to boil constantly between 149° and 150°. Addition of another molecule of bromine resulted in the production of octahedral crystals of the tetrabromide identical with those given by the ordinary compound. On reduction of each of the compounds in alcoholic solution with zinc dust, the plane-symmetrical ordinary compound is much more rapidly attacked than the centro-symmetrical iso-dibromide; when the reaction in each case was allowed to proceed under the same conditions for one hour, 99·6 per cent. of the ordinary compound was converted back to crotonylene, while only 60·6 per cent. of the iso- had been attacked.

THE Bureau des Longitudes has just issued an extract from the *Connaissance des Temps* (1890), for the use of schools of hydrography and aspirants to the grade of captain, either for coasting or ocean service, a recent resolution of the Minister of Marine having rendered such an extract very desirable. It contains the tables of the rising and setting of the sun and moon, and the place of the sun and sidereal time at mean noon for each day of the year. The moon's position is given only for every twelve hours throughout the year, instead of for every hour as in the complete edition, and the tables of lunar distances have been reduced to one star for each day. The positions of only thirty of the principal stars are given, as against 300 in the complete edition. The tables of refraction, and corrections for parallax, are reproduced in full, as are also the tables for conversion of mean time to sidereal time. The positions of the planets, Jupiter's satellites, &c., have been entirely omitted. Useful extracts from the data relating to tides already published in the *Annuaire des Marées* are given, with instructions for their use. The "establishment of the port" for every important port in the world is given, as well as the unit from which the height of the tide can be calculated. Our own authorities would do well to follow the example of the Bureau des Longitudes, and publish a similar extract from the *Nautical Almanac*.

THE Director of the Marine Biological Association reports that the breeding season of a large number of marine animals has begun, and that embryologists may find ample opportunities for study at the sea-side. Of Teleostean fishes, the spawning season of the herring has passed some time since, that of the plaice and flounder is just over, whilst the whiting, whiting pout, and merry sole (*Arnoglossus megastoma*) are now ripe. Nudibranch Mollusks are now visiting the shores to deposit their eggs, and large specimens of *Doris tuberculata* and *Eolis papillosa* may commonly be taken on the rocks at Plymouth. The uncommon *Capulus hungaricus* has been found with ova attached. Among the Crustacea the common shrimp, the prawn, and *Pandalus annulicornis* are hatching out, and the surface net begins to show numerous zoeæ of *Porcellana longicornis*. The Nauplii of Balanus and of Copepods are also abundant at this season of the year. The trochosphere larvæ of Chætopods (*Terebella* and *Phyllodoce*) are beginning to make their appearance, and ova and larvæ of Echinoderms are very abundant, particularly the ova of Ophiura, and Bipinnaria and Plutcus larvæ of Asterids and Echinids. The gonophores of the Hydrozoa are for the most part well developed, and filled with ripe ova during the coming month.

AT a meeting of the Royal Botanic Society on Saturday, a branch of coffee thickly set with ripe fruit from a plant growing in the Society's conservatory was shown. Dr. Prior mentioned

as a curious fact that in parts of Abyssinia the fleshy outside husk only was eaten, the part we use being thrown away as worthless.

In the February number of *Himmel und Erde*, Herr O. Jesse, of the Berlin Astronomical Observatory, calls attention to the importance of an accurate study of the luminous night-clouds which, since 1885, have been visible in Europe in the months of June and July. Herr Jesse is of opinion that these phenomena are interesting from an astronomical as well as from a meteorological point of view, because their periodic movement, taken in conjunction with their extraordinary height, suggests that they manifest the activity of cosmical forces. He holds, therefore, that they may throw some light upon the question whether cosmical space is filled with a resisting medium, and that their action may be a repetition of occurrences which played a great part in the earlier period of the development of the earth and of the planets generally. He urges that the phenomena should be carefully observed, and appeals to all who may take this task in hand to send him the results of their observations.

Mr. A. T. Drummond, in recent Canadian publications, takes the view that Lake Superior is the most ancient of the St. Lawrence Great Lakes, dating back to Cambrian and, it may be, earlier times, and that whilst its waters at perhaps more than one period found an outlet to the ocean through the Mississippi valley, the lake formed in other ages one of the sources of a great river system which terminated on the Atlantic seaboard. He traces the course of this river from the Michigan basin, and from Lake Superior across Lake Huron to the headlands at the entrance to the Georgian Bay. Here its waters were hurled over the cliffs in a great fall more than rivalling Niagara. At the lower level another considerable stream joined it from the north, and the united rivers then skirted the face of the continuous, shaly, precipitous cliffs which cross Ontario to the Lake Ontario valley. Following the course of the escarpments which they created in this valley, the waters eventually reached the sea through the Mohawk and Hudson Rivers. At a recent period, the elevation of the land between the Georgian Bay and Lake Ontario blocked the course of the river, and, filling the Georgian Bay with water, created a new outlet, not by the present St. Clair River, which was of later birth, but to the south-eastward of Lake Huron, where, through a channel now buried by clays, the Erie basin was reached. The course from this basin to the Ontario valley was through the great fracture in the limestones at Hamilton, and not over the Niagara Falls. The change in the flow of these waters to the Niagara River was one of the opening episodes in the later history of Lake Erie. The Ottawa River was at this time a large stream flowing much in the same course as now, whilst the St. Lawrence was a less important river, taking its rise in the Adirondack Mountains, which, at their then greater elevation, blocked the present outlet of Lake Ontario.

Mr. Thomas Wilson, of the Smithsonian Institution, Washington, writing in the current number of the *American Naturalist*, gives a rather gloomy account of the treatment accorded by public authorities in the United States to science in general, and to archæology in particular. "The States of Ohio," he says, "or Wisconsin, or West Virginia, or Mississippi, not to mention New York or New England, have either of them within their borders as much unstudied, unsearched, and unclassified archæologic riches as has any one of the great countries of Europe: England, France, Germany, Spain, or Italy. Yet these countries, each of them, do more for archæology than equals the combined efforts of the United States and all the State Governments. I confess to a feeling of depression when, on visiting the Prehistoric Museum at Salisbury, England, I found there stored and displayed, in a beautiful building, erected in the midst of a lovely park, for its sole occupancy, the prehistoric collection of Squier and Davis, gathered by them from the mounds of the United States in the Ohio and Mississippi valleys. It went begging through the United States, knocked at the door of Congress, and besought a purchaser at the ludicrous price of $1000, but without finding a response. And in disgust with their countrymen, and in despair of ever being able to interest their Government or fellow-citizens, they sold their collection to England and retired from the field of archæologic investigation."

We learn from an article in *Science* that the pottery industry in the United States gives employment directly to about ten thousand people, to whom wages amounting annually to four million dollars are paid, this amount being nearly 50 per cent. of the total value of the output of the potteries. In addition to these, there are many thousand more employed in the preparation of the materials for the potters' use, such as mining the clays, quartz, and felspar, and grinding and washing the materials. To these people nearly as much more in wages is paid. According to *Science*, the American potter does not claim to be the peer of his foreign competitor in art productions, but he does claim to equal any foreign manufacturer in the class of china which he produces for the American people—both fine and common "crockery" for domestic uses. "To-day," says *Science*, "the English potter is copying American shapes, designs, and styles of decorations. How different is this state of affairs from that which existed a few years ago, when the American potter depended upon foreign ideas for his shapes and designs! With the development of the manufacturing process, talent for designing shapes and patterns or styles of decoration has likewise progressed, until we have made our own American shapes and designs, which foreigners have been compelled to copy and adopt in order to find a market for their wares in the United States."

In his "Butterflies of the Eastern United States," a part of which we lately reviewed, Mr. Scudder has an interesting paper —Excursus xxiii.—on mimicry and protective resemblance among butterflies. He points out that cases of mimicry are far more common in the tropics than in temperate regions, even relatively. The accounts of travellers in the tropics constantly mention the attacks of birds upon butterflies, while instances of butterflies being seen pursued by birds are vastly more rare in the temperate regions. Mr. Scudder himself has never seen one. In the tropics, moreover, there are many other insectivorous animals, such as lizards. "In our own country, therefore," says Mr. Scudder, "we should not look for many instances of mimicry of any decided type. The most striking is unquestionably that of *Basilarchia archippus*, which mimics *Anosia plexippus*, and the closely related case of *Basilarchia eros* and *Tasitia berenice*, the last two butterflies largely supplanting the first two on the peninsula of Florida. In both these instances the mimicry is enjoyed by both sexes. A third case is found in the less close but still striking mimicry of *Basilarchia astyanax* by the female of *Semnopsyche diana*, an instance the more remarkable as the mimicked species belongs to the same genus as our two other mimicking forms."

At a recent meeting of the Ceylon Branch of the Royal Asiatic Society, two papers were read by Dr. Trimen and Mr. A. P. Green, describing a visit paid by them to Ritigala. Dr. Trimen's paper was devoted to an account of the flora to be found on the mountain, and the difference between it and that of the surrounding country. Ritigala is the highest ground between the central mountains of Ceylon and the mountains of Southern India. It is only 2506 feet high; next to it in height in the central plain of Ceylon being Friar's Hood (2147 feet), Westminster Abbey (1829 feet), and Gunner's Quoin

(1736 feet). Ritigala is completely isolated, and its summit is frequently surrounded by mist, especially during the south-west monsoon—that is, when the plains are suffering severely from drought. The branches of the stunted trees on the mountain are covered with masses of *Meteorium* moss and lichens, like those on the high mountains. Unfortunately the long-continued drought had withered up much of the vegetation, and therefore the expedition was not as productive as it otherwise would have been.

CAPTAIN MOORE, of H.M.S. *Rambler*, has lately described—in a paper read before the China Branch of the Royal Asiatic Society—the appearance and effects of the remarkable "bore" which often occurs in Hangchow Bay. This dangerous visitor is the result of the struggle between the advancing tide in the great estuary and the current of the river. Captain Moore and his officers on several occasions observed the progress of the wave, and their investigations may be summarized as follows:—The rate at which the bore travels varies from ten to about thirteen miles per hour. The height of the bore rarely exceeds 12 or 14 feet, and broken water, in which no small boat could live, follows it for some distance. With the passing of the wave the tide rises many feet in a few seconds; in one instance, observed by Captain Moore, it rose from 9 feet 4 inches below to 4 feet 7 inches above mean level. The rush of the bore was so strong that the force of the waves breaking against the broadside of the *Rambler* sent the water into the mizzen chains and the spray on to the poop. The junks in that region are protected by platforms, with narrow steps cut in the sides. To the north of the estuary is a great sea-wall, built to protect the surrounding country from being flooded by these great tidal-waves. It is thirty-five miles long, and it is strengthened, where the bore strikes most strongly, by an elliptical stone buttress, 253 feet long by 63 feet wide. Behind this the junks are drawn up for shelter.

IN one of the recent American Consular Reports the preparation of Japanese *koji* (yeast) is described by Prof. Georgeson, of the Imperial Agricultural College at Tokio. *Koji* is made not only in special factories, but also in *saké* (rice-beer) breweries. The materials used are water, rice of the common starchy kind only, and *tane* (seed or leaven), the spores of a fungus (*Eurotium oryzeæ*, Ahlb.). It is this latter substance which, when germinating on the rice, changes a portion of the starch into dextrose and dextrin, and produces the fermentation. The rice is carefully washed, and the thin covering of the seed is always removed. It is then allowed to remain for some hours in water, and having been steamed, it is spread on mats to cool, and when the temperature has fallen to about 100° F., the *tane* is scattered uniformly on the mass and then thoroughly mixed with it. The whole is then allowed to remain for eighteen or twenty hours covered with mats, after which the rice is spread in a thin layer in a number of shallow, wooden trays, which are taken to the warmest room in the factory, and there left standing for four or five hours, when the contents are stirred by the hand, and again after an interval of four hours the operation is repeated. On the third day the fungus grows very rapidly, and great heat is generated. Care, however, is taken that the heat does not become too great. This is the usual mode, but there are many other methods.

THE additions to the Zoological Society's Gardens during the past week include a Guinea Baboon (*Cynocephalus sphinx*) from West Africa, presented by Mr. W. J. Vinton; a Valentyn's Phalanger (*Cuscus orientalis* ♂) from the Solomon Islands, presented by Mr. Chas. M. Woodford, C.M.Z.S; an Owen's Apteryx (*Apteryx oweni*), two Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, presented by Capt. C. A. Findlay, R.M.S. *Ruapehu*; two Nuthatches (*Sitta cæsia*), British, presented by Mr. J. Young, F.Z.S.; a Chimpanzee (*Anthropopithecus troglodytes* ♂) from West Africa, two Brown Cranes (*Grus canadensis*) from North America, three Black Swans (*Cygnus nigricollis*) from Australia, a Larger Hill-Mynah (*Gracula intermedia*) from India, deposited; a Hoffmann's Sloth (*Cholopus hoffmanni* ♀) from Panama, six Brent Geese (*Bernicla brenta*), European, purchased.

OUR ASTRONOMICAL COLUMN.

DISTRIBUTION OF SUN-SPOTS IN LATITUDE.—As remarked in the last issue of NATURE (p. 449), spots have been decidedly more numerous in the southern hemisphere of the sun during the last six years than in the northern. Since, however, the two hemispheres were about equally occupied in 1882, and the northern had the decided advantage in 1881 and 1880—whilst for nearly a quarter of a century previous there had been no long-continued difference between the two—it might be supposed that their present predominance in the southern hemisphere was one which would disappear in a series of observations spread over any great number of years. Prof. Spoerer, in a couple of valuable papers which he has just published—"Ueber die Periodicität der Sonnenflecken seit dem Jahre 1618," and "Sur les différences que présentent l'hémisphère nord et l'hémisphère sud du Soleil," the latter appearing in the *Bulletin Astronomique* for February—has given reason for believing that this is not the case, and that there have been at least two considerable periods, previous to the present one, in which the southern hemisphere was by far the more prolific in spots. The first of these was from 1621 to 1625, during which Scheiner's observations give us no spots in the northern hemisphere for 1621 and 1622, very few up to February 1625, and decidedly fewer than in the southern hemisphere until 1626, when a practical equality was established. The second period was in every way a more remarkable one, lasting for more than forty years. We have no record of any northern spots from 1672 to 1704; a few were seen in 1705, but their appearance there was looked upon as a most remarkable circumstance: Cassini and Maraldi recorded that they had never seen spots in the northern hemisphere before; and later, in 1714, on the occasion of the appearance of three northern spots, it was stated in a paper in the *Mémoires* of the Paris Academy that that hemisphere had been free from spots for forty years. This period, 1672–1713, seems to have been an exceptional one from several points of view. For much of the time there seem to have been scarcely any spots visible at all. Cassini, noting the third spot seen in 1676, remarks that in that year they had been more frequent than in the twenty preceding years. Flamsteed, in 1684, says that a spot he saw then was the first he had seen since 1676. Cassini, later on, states that the only spot seen since 1686 was that of May 1688, whilst in 1705 it is recorded ("Histoire de l'Académie," 1795, p. 128) that since Scheiner's observations, made sixty years before, two groups of spots had hardly ever been seen at the same time. Ten years later a similar remark is made; but, in 1716, spots became much more numerous, and as many as eight groups were seen at one time, from August 30 to September 3. The "law of zones," which Prof. Spoerer demonstrated for recent periods, the law that, from minimum to minimum, the spots show a tendency to seek lower and lower latitudes, breaking out afresh in high latitudes directly the next minimum is passed, seems to have been in abeyance during this remarkable period. The mean latitude seems to have been about 8° or 9°, but there was no regular drift downward made evident. The law, as Prof. Spoerer shows in the first of the above-named papers, appears to be unmistakably illustrated by Scheiner's observations at the time of the minimum of 1619, whilst the observations of 1643 and 1644 gave also a low mean latitude previous to the minimum of 1645. Observations at minima since the exceptional period of 1672–1713 supply many illustrations of the law of zones, as Prof. Spoerer takes occasion to demonstrate for the minima of 1755, 1775, 1784, 1833, and 1844.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *MARCH* 17–23.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 17

Sun rises, 6h. 11m.; souths, 12h. 8m. 23·5s.; sets, 18h. 7m.: right asc. on meridian, 23h. 49·4m.; decl. 1° 9′ S. Sidereal Time at Sunset, 5h. 48m.

Moon (Full on March 17, 12h.) rises, 18h. 10m.; souths, 0h. 42m.*; sets, 6h. 58m.*: right asc. on meridian, 12h. 24·6m.; decl. 2° 39′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	5 32 ...	10 29 ...	15 26 ...	22 9·6 ...	12 51 S.
Venus.....	6 55 ...	14 44 ...	22 33 ...	2 25·1 ...	19 21 N.
Mars	6 53 ...	13 38 ...	20 33 ...	1 19·3 ...	8 8 N.
Jupiter....	2 50 ...	6 46 ...	10 42 ...	18 26·2 ...	23 0 S.
Saturn....	13 47 ...	21 25 ...	5 3*...	9 7·7 ...	17 44 N.
Uranus ...	20 14*...	1 39 ...	7 4 ...	13 18·3 ...	7 34 S.
Neptune..	8 27 ...	16 11 ..	23 55 ...	3 52·3 ...	18 32 N.

* Indicates that the rising is that of the preceding evening and the southing and setting those of the following morning.

Mar.	h.	
18 ...	21 ...	Mercury at greatest distance from the Sun.
20 ...	10 ...	Sun in equator: day and night of equal length.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	Mar. 19,	21 10	*m*
R Canis Majoris ...	7 14·5 ..	16 11 S. ...	,, 19,	18 49	*m*
		and at intervals of		27 16	
U Monocerotis ...	7 25·5 ..	9 33 S. ...	Mar. 19,		*m*
V Cancri	8 15·4 ...	17 38 N. ...	,, 23,		*M*
S Cancri	8 37·6 ...	19 26 N. ...	,, 21,	6 16	*m*
R Leonis	9 41·6 ...	11 57 N. ...	,, 23,		*M*
W Virginis	13 20·3 ...	2 48 S. ...	,, 23,	23 0	*M*
U Coronæ	15 13·7 ...	32 3 N. ...	,, 17,	22 15	*m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,, 17,	20 0	*m*
			,, 21,	1 30	m_2
Y Cygni	20 47·6 ...	34 14 N. ...	,, 17,	18 0	†*m*
		and at intervals of		36 0	
δ Cephei	22 25·0 ...	57 51 N. ...	Mar. 17,	20 0	*M*
			,, 23,	5 0	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

† Y Cygni should be watched with especial care, as the time of minimum is subject to considerable uncertainty.

Meteor-Showers.

	R.A.	Decl.	
Near θ Ursæ Majoris ...	144° ...	50° N. ...	March 20.
,, β Ursæ Majoris ...	162 ...	57 N. ...	Slow.
,, ι Herculis	263 ...	47 N.	

GEOGRAPHICAL NOTES.

THE paper on Monday night at the Royal Geographical Society was by the Hon. G. Curzon, M.P., and dealt with the Trans-Caspian Railway, over which Mr. Curzon recently travelled from the Caspian to Samarcand, a distance of 900 miles. Mr. Curzon described the structure of the railway, the engineering and other difficulties met with, the geographical features of the country traversed, and referred in some detail to the political, civilizing, and commercial results of the undertaking. The line is on a 5-foot gauge, which is uniform with the railway system of European Russia, but not with that of British India. The rails are of steel, from 19 to 22 feet long, and are laid upon wooden sleepers at the rate of 2000 sleepers to every mile, being simply spiked down without chairs or bolts. Every piece of timber, iron, and steel employed was brought from the forests or workshops of Russia, for the most part down the Volga and across the Caspian. The line is a single one from start to finish, except at the stations, where there are invariably sidings, and sometimes triangles, for an engine to reverse; it is laid upon a low earthwork or embankment thrown up with the soil scooped out of a shallow trench on either side. The permanent way is not metalled. It has been claimed that this railway is an astonishing engineering phenomenon, inasmuch as it traverses a country previously believed to be inaccessible to such a method of locomotion. But Mr. Curzon maintains, apart from the local lack of material due to the appalling dearth of the country, it is probably the easiest and simplest railway ever built. The region which it traverses is as flat as a billiard-table for almost the entire distance, the steepest gradient being 1 in 150. There are no tunnels, only a few insignificant cuttings, and but three bridges—across the Tejend, across the Murghab at Merv, and across the Amu-daria. The two main difficulties arose from scarcity of water and superabundance of sand. The former was not difficult to overcome, and the various means employed to check the destructive effects of sand will no doubt prove efficacious, though constant watchfulness along the whole line will be required. The really formidable sands are limited to three districts: (1) the first thirty miles from the Caspian; (2) the stretch between the Merv Oasis and the Oxus; and (3) the stretch between the Oxus and Bokhara. Here no vegetation is either visible or, with rare exceptions, possible; the sand, of the most brillant yellow hue, is piled in loose hillocks and mobile dunes, and is swept hither and thither by powerful winds. It has all the appearance of a sea of troubled waves, billow succeeding billow in melancholy succession, with the sand driving like spray from their summits, and great smooth-swept troughs lying between, on which the winds leave the imprint of their fingers in wavy indentations, just like an ebb tide on the sea-shore. Near the Caspian the permanent way was soaked with sea-water so as to give it consistency; in other parts it was covered with a sort of armour-plating of clay. Elsewhere, and in the more desolate regions, other plans were adopted. Light wooden palisades, 3 or 4 feet high, made of pine laths, were driven into the tops of the dunes and formed a barrier against which the winds might pile the sands with impunity. Nurseries for suitable desert plants were started in the Persian Mountains, and the product of these, tamarisk, wild oats, &c., were planted on the sand-hillocks contiguous to the line. Here, too, was planted that strange and interesting denizen of the wilderness the *Saxaoul* (*Haloxylon ammodendron*), which with a scanty and often ragged upper-growth, strikes its sturdy roots deep down into the sand, and somehow or other derives sustenance from that to which it gives stability and permanence. Fascines of the branches of this plant were also cut, laid at right angles to the rails along the edge of the earthwork or embankment, and covered over with a layer of sand. In spite of all these precautions, the sand must always constitute a serious danger to the line. In referring to Merv, and the miles and miles of ruins of the various old Mervs, Mr. Curzon gives the area of the oasis as 1600 square miles, with a population of not more than 100,000. The desert by which the oasis is surrounded is appalling. East and west, and north and south, stretches a troubled ocean of sand, each wave arrested, as it were, in mid career, when just curving to fall. Mr. Curzon never saw anything more melancholy than this wilderness with its sickle-shaped dome-like ridges of sand, succeeding each other with the regularity of infantry files. Each has the appearance of being cloven through the crown, the side facing towards the north-east, whence the prevailing winds blow, being uniform, convex, and smooth, while the southern face is vertical and abrupt. With regard to the famous bridge over the Oxus, Mr. Curzon states that its total length is 2000 yards, and that it rests on more than 3000 piles. The level of the rails is about 30 feet above low, but only 5 feet above high, water.

TO the March number of the *Scottish Geographical Magazine*, an instructive paper on the Islands of Melanesia is contributed by Dr. R. H. Codrington. Mr. Ravenstein, in a paper accompanied by a map, on Lake Bangweolo, corrects the configuration of the lake based on a wrong interpretation of Livingstone's observations. Comparing them with subsequent results, Mr. Ravenstein shows that the length of the lake is north and south, and that the Luapula issues from its south-west corner. Mr. Ralph Richardson brings together a useful collection of data on the Edinburgh earthquake of January last.

THE Foreign Office has just published some extracts from a journal kept by Mr. W. J. Archer, British Vice-Consul at Chiengmai, or Zimmé, of a visit to Chiengtung, in May and June 1888. Mr. Archer traversed the little-known and mountainous region lying between the Rivers Salween and Cambodia, taking altogether thirty-six days for his journey both ways. From Chiengmai to Chienghai there are only a few towns, some of them inhabited by Luwas, the aborigines of the country. The hills, which abound in tigers, are uncultivated, but the valleys and low-lying grounds appeared well tilled, and bearing good crops of rice and paddy. North of Chienghai he saw the effects of attacks by the dacoits on the villagers. The former were Ngios from the Chiengtung territory, led by a Lao. Mr. Archer says that he was surprised to see what little traffic there was on the road, but this, perhaps, was due to some extent to

the very heavy rains. The highest mountain-range of the whole region is a little south of Chiengtung, and is about 4400 feet high. After leaving Müang Hai, no signs of habitation were seen until Chiengtung was reached. As an instance of the insecurity of the country, he mentions that with one party he saw a young woman armed like the men. The plateau of Chiengtung is about 2700 feet above the sea-level, and is very bare and badly cultivated. The town itself is walled, and is situated in the southern corner of the plateau, which is inclosed by high mountains on the west, south, and east. The chief products are rice, cotton, and opium; tea is also cultivated on the hills. Cotton goods are imported from Moulmein; salt, silk, and other articles from Yünnan; cocoa-nuts and betel-nuts from Chienghai. The inhabitants of the district belong to several races; the majority are of the same people as the ruling family, but there are also numbers of Western Shans, of the hill tribes (the Kins, Kans, and Musös), and Lëms and Lüs. The houses of the people are as miserable as could be, and their great poverty is shown by the fact that, instead of their temples being made of brick, as is the case in Siam, they are wretched sheds, almost as miserable as the houses. Mr. Archer returned by a different road, and found whole districts uninhabited between Chiengtung and Muang Lën, which is close to the Cambodia River.

THE *Manchester Guardian* publishes some interesting information as to the movements of Dr. Lansdell. In October last he arrived at Khoten, whence he set out for Yarkand. He left Yarkand for Western Tibet and crossed the Kilian Pass at a height of 17,000 feet on December 2. On the 10th the Karakoram was passed without very much difficulty; but a few days later, when crossing the Saser, the party suffered very much from the intense cold. On November 14, Dr. Lansdell sighted at Changlung the first inhabited huts of the Tibetans. Crossing over Khardung Pass with great difficulty, he finally arrived at Leh, where he remained for a few days, setting out on December 5 to descend to Kashmir. To aid in this attempt, forty men were sent by the local authorities, and Zogiha Pass was crossed in safety, and Kashmir reached. In a few days Srinagur was reached, but Soultem Passes were blocked, and an attempt to penetrate them was abandoned. At Rawul Pindee, Dr. Lansdell's journey finished, he having travelled from Kuldja 2000 miles, crossing seven of the highest passes in the world.

THE Government Geologist who set out from Adelaide some months ago to explore Central Australia has just returned. He travelled as far north as Alice Springs—that is, to the Tropic of Capricorn—and spent nearly a fortnight there examining the ruby and gold fields. Between Anna Creek and Alice Springs the country is well watered, but at the diggings water is very scarce. In all, some 2400 miles were travelled, and, around the ruby fields, camels were employed. The heat was very great, on one occasion reaching as much as 124° F. in the shade.

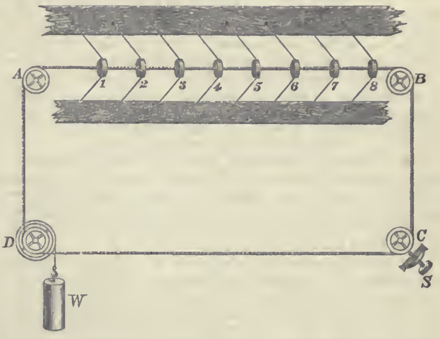
THE DISCHARGE OF A LEYDEN JAR.[1]

IT is one of the great generalizations established by Faraday, that all electrical charge and discharge is essentially the charge and discharge of a Leyden jar. It is impossible to charge one body alone. Whenever a body is charged positively, some other body is *ipso facto* charged negatively, and the two equal opposite charges are connected by lines of induction. The charges are, in fact, simply the ends of these lines, and it is as impossible to have one charge without its correlative as it is to have one end of a piece of string without there being somewhere, hidden it may be, split up into strands it may be, but somewhere existent, the other end of that string.

This I suppose familiar fact that all charge is virtually that of a Leyden jar being premised, our subject for this evening is at once seen to be a very wide one, ranging in fact over the whole domain of electricity. For the charge of a Leyden jar includes virtually the domain of electrostatics; while the discharge of a jar, since it constitutes a current, covers the ground of current electricity all except that portion which deals with phenomena peculiar to steady currents. And since a current of electricity necessarily magnetizes the space around it, whether it flow in a straight or in a curved path, whether it flow through wire or burst through air, the territory of magnetism is likewise invaded; and inasmuch as a Leyden jar discharge is oscillatory, and we now know the vibratory motion called light to be really an oscillating electric current, the domain of optics is seriously encroached upon.

But though the subject I have chosen would permit this wide range, and though it is highly desirable to keep before our minds the wide-reaching import of the most simple-seeming fact in connection with such a subject, yet to-night I do not intend to avail myself of any such latitude, but to keep as closely and distinctly as possible to the Leyden jar in its homely and well-known form, as constructed out of a glass bottle, two sheets of tinfoil, and some stickphast.

The act of charging such a jar I have permitted myself now for some time to illustrate by the mechanical analogy of an inextensible endless cord able to circulate over pulleys, and threading in some portion of its length a row of tightly-gripping beads which are connected to fixed beams by elastic threads.



Mechanical analogy of a circuit partly *dielectric*; for instance, of a charged condenser. A is its positive coat, B its negative.

The cord is to represent electricity; the beads represent successive strata in the thickness of the glass of the jar, or, if you like, atoms of dielectric or insulating matter. Extra tension in the cord represents negative potential, while a less tension (the nearest analogue to pressure adapted to the circumstances) represents positive potential. Forces applied to move the cord, such as winches or weights, are electromotive forces; a clamp or fixed obstruction represents a rheostat or contact-breaker; and an excess or defect of cord between two strata of matter represents a positive or a negative charge.

The act of charging a jar is now quite easily depicted as shown in the diagram.

To discharge the jar one must remove the charging E.M.F. and unclamp the screw, *i.e.* close the circuit. The stress in the elastic threads will then rapidly drive the cord back, the inertia of the beads will cause it to overshoot the mark, and for an instant the jar will possess an inverse charge. Back again the cord swings, however, and a charge of same sign as at first, but of rather less magnitude, would be found in the jar if the operation were now suspended. If it be allowed to go on, the oscillations gradually subside, and in a short time everything is quiescent, and the jar is completely discharged.

All this occurs in the Leyden jar, and the whole series of oscillations, accompanied by periodic reversal and re-reversal of the charges of the jar, is all accomplished in the incredibly short space of time occupied by a spark.

Consider now what the rate of oscillation depends on. Manifestly on the elasticity of the threads and on the inertia of the matter which is moved. Take the simplest mechanical analogy, that of the vibration of a loaded spring, like the reeds in a musical box. The stiffer the spring, and the less the load, the faster it vibrates. Give a mathematician these data, and he will calculate for you the time the spring takes to execute one complete vibration, the "period" of its swing. [Loaded lath in vice.]

The electrical problem and the electrical solution are precisely the same. That which corresponds to the flexibility of the spring is in electrical language called static capacity, or, by Mr. Heaviside, permittance. That which corresponds to the inertia of ordinary matter is called electro-magnetic inertia, or self-induction, or, by Mr. Heaviside, inductance.

[1] Friday evening discourse at the Royal Institution of Great Britain, on March 8, by Prof. Oliver J. Lodge, F.R.S.

Increase either of these, and the rate of oscillation is diminished. Increasing the static capacity corresponds to lengthening the spring; increasing the self-induction corresponds to loading it.

Now the static capacity is increased simply by using a larger jar, or by combining a number of jars into a battery in the very old-established way. Increase in the self-induction is attained by giving the discharge more space to magnetize, or by making it magnetize a given space more strongly. For electro-magnetic inertia is wholly due to the magnetization of the space surrounding a current, and this space may be increased or its magnetization intensified as much as we please.

To increase the space we have only to make the discharge take a long circuit instead of a short one. Thus we may send it by a wire all round the room, or by a telegraph wire all round a town, and all the space inside it and some of that outside will be more or less magnetized. More or less, I say, and it is a case of less rather than more. Practically very little effect is felt except close to the conductor, and accordingly the self-induction increases very nearly proportionally to the length of the wire, and not in proportion to the area inclosed: provided also the going and return wires are kept a reasonable distance apart, so as not to encroach upon each other's appreciably magnetized regions.

But it is just as effective, and more compact, to intensify the magnetization of a given space by sending the current hundreds of times round it instead of only once; and this is done by inserting a coil of wire into the discharge circuit.

Yet a third way there is of increasing the magnetization of a given space, and that is to fill it with some very magnetizable substance such as iron. This, indeed, is a most powerful method under many circumstances, it being possible to increase the magnetization and therefore the self-induction or inertia of the current some 5000 times by the use of iron.

But in the case of the discharge of a Leyden jar iron is of no advantage. The current oscillates so quickly that any iron introduced into its circuit, however subdivided into thin wires it may be, is protected from magnetism by inverse currents induced in its outer skin, as your Professor of Natural Philosophy[1] has shown, and accordingly it does not get magnetized; and so far from increasing the inductance of the discharge circuit it positively diminishes it by the reaction effect of these induced currents: it acts, in fact, much as a mass of copper might be expected to do.

The conditions determining rate of oscillation being understood, we have next to consider what regulates the damping out of the vibrations, *i.e.* the total duration of the discharge.

Resistance is one thing. To check the oscillations of a vibrating spring you apply to it friction, or make it move in a viscous medium, and its vibrations are speedily damped out. The friction may be made so great that oscillations are entirely prevented, the motion being a mere dead-beat return to the position of equilibrium; or, again, it may be greater still, and the motion may correspond to a mere leak or slow sliding back, taking hours or days for its accomplishment. With very large condensers, such as are used in telegraphy, this kind of discharge is frequent, but in the case of a Leyden jar discharge it is entirely exceptional. It can be caused by including in the circuit a wet string, or a capillary tube full of distilled water, or a slab of wood, or other atrociously bad conductor of that sort; but the conditions ordinarily associated with the discharge of a Leyden jar, whether it discharge through a long or a short wire, or simply through its tongs, or whether it overflow its edge or puncture its glass, are such as correspond to oscillations, and not to leak. [Discharge jar first through wire and next through wood.]

When the jar is made to leak through wood or water the discharge is found to be still not steady: it is not oscillatory indeed, but it is intermittent. It occurs in a series of little jerks, as when a thing is made to slide over a resined surface. The reason of this is that the terminals discharge faster than the circuit can supply the electricity, and so the flow is continually stopped and begun again.

Such a discharge as this, consisting really of a succession of small sparks, may readily appeal to the eye as a single flash, but it lacks the noise and violence of the ordinary discharge; and any kind of moving mirror will easily analyze it into its constituents and show it to be intermittent. [Shake a mirror, or waggle head or opera-glass.]

It is pretty safe to say, then, that whenever a jar discharge is not oscillatory it is intermittent, and when not intermittent is oscillatory. There is an intermediate case when it is really dead-beat, but it could only be hit upon with special care, while its occurrence by accident must be rare.

[1] Lord Rayleigh.

So far I have only mentioned resistance or friction as the cause of the dying out of the vibrations; but there is another cause, and that a most exciting one.

The vibrations of a reed are damped partly indeed by friction and imperfect elasticity, but partly also by the energy transferred to the surrounding medium and consumed in the production of sound. It is the formation and propagation of sound-waves which largely damp out the vibrations of any musical instrument. So it is also in electricity. The oscillatory discharge of a Leyden jar disturbs the medium surrounding it, carves it into waves which travel away from it into space: travel with a velocity of 185,000 miles a second: travel precisely with the velocity of light. [Tuning-fork.]

The second cause, then, which damps out the oscillations in a discharge circuit is *radiation: electrical* radiation if you like so to distinguish it, but it differs in no respect from ordinary radiation (or "radiant heat" as it has so often been called in this place); it differs in no respect from Light except in the physiological fact that the retinal mechanism, whatever it may be, responds only to waves of a particular, and that a very small, size, while radiation in general may have waves which range from 10,000 miles to a millionth of an inch in length.

The seeds of this great discovery of the nature of light were sown in this place: it is all the outcome of Faraday's magneto-electric and electrostatic induction: the development of them into a rich and full-blown theory was the greatest part of the life-work of Clerk-Maxwell: the harvest of experimental verification is now being reaped by a German. But by no ordinary German. Dr. Hertz, now Professor in the Polytechnicum of Karlsruhe, is a young investigator of the highest type. Trained in the school of Helmholtz, and endowed with both mathematical knowledge and great experimental skill, he has immortalized himself by a brilliant series of investigations which have cut right into the ripe corn of scientific opinion in these islands, and by the same strokes as have harvested the grain have opened up wide and many branching avenues to other investigators.

At one time I had thought of addressing you this evening on the subject of these researches of Hertz, but the experiments are not yet reproducible on a scale suited to a large audience, and I have been so closely occupied with some not wholly dissimilar, but independently conducted, researches of my own—researches led up to through the unlikely avenue of lightning-conductors—that I have had as yet no time to do more than verify some of them for my own edification.

In this work of repetition and verification Prof. Fitzgerald has, as related in a recent number of NATURE (February 21, p. 391), probably gone further; and if I may venture a suggestion to your Honorary Secretary, I feel sure that a discourse on Hertz's researches from Prof. Fitzgerald next year would be not only acceptable to you, but would be highly conducive to the progress of science.

I have wandered a little from my Leyden jar, and I must return to it and its oscillations. Let me very briefly run over the history of our knowledge of the oscillatory character of a Leyden jar discharge. It was first clearly realized and distinctly stated by that excellent experimentalist, Joseph Henry, of Washington, a man not wholly unlike Faraday in his mode of work, though doubtless possessing to a less degress that astonishing insight into intricate and obscure phenomena; wanting also in Faraday's circumstantial advantages.

This great man arrived at a conviction that the Leyden jar discharge was oscillatory by studying the singular phenomena attending the magnetization of steel needles by a Leyden jar discharge, first observed in 1824 by Savary. Fine needles, when taken out of the magnetizing helices, were found to be not always magnetized in the right direction, and the subject is referred to in German books as anomalous magnetization. It is not the magnetization which is anomalous, but the currents which have no simple direction; and we find in a memoir published by Henry in 1842, the following words:—

"This anomaly, which has remained so long unexplained, and which, at first sight, appears at variance with all our theoretical ideas of the connection of electricity and magnetism, was, after considerable study, satisfactorily referred by the author to an action of the discharge of the Leyden jar which had never before been recognized. The discharge, whatever may be its nature, is not correctly represented (employing for simplicity the theory of Franklin) by the single transfer of an imponderable fluid

from one side of the jar to the other; the phenomenon requires us to admit *the existence of a principal discharge in one direction and then several reflex actions backward and forward, each more feeble than the preceding, until the equilibrium is obtained.* All the facts are shown to be in accordance with this hypothesis, and a ready explanation is afforded by it of a number of phenomena, which are to be found in the older works on electricity, but which have until this time remained unexplained."[1]

The italics are Henry's. Now if this were an isolated passage it might be nothing more than a lucky guess. But it is not. The conclusion is one at which he arrives after a laborious repetition and serious study of the facts, and he keeps the idea constantly before him when once grasped, and uses it in all the rest of his researches on the subject. The facts studied by Henry do in my opinion support his conclusion, and if I am right in this it follows that he is the original discoverer of the oscillatory character of a spark, although he does not attempt to state its theory. That was first done, and completely done, in 1853, by Sir William Thomson; and the progress of experiment by Feddersen, Helmholtz, Schiller, and others has done nothing but substantiate it.

The writings of Henry have been only quite recently collected and published by the Smithsonian Institution of Washington in accessible form, and accordingly they have been far too much ignored. The two volumes contain a wealth of beautiful experiments clearly recorded, and well repay perusal.

The discovery of the oscillatory character of a Leyden jar discharge may seem a small matter, but it is not. One has only to recall the fact that the oscillators of Hertz are essentially Leyden jars—one has only to use the phrase "electro-magnetic theory of light"—to have some of the momentous issues of this discovery flash before one.

One more extract I must make from that same memoir by Henry,[2] and it is a most interesting one; it shows how near he was, or might have been, to obtaining some of the results of Hertz; though, if he had obtained them, neither he nor any other experimentalist could possibly have divined their real significance.

It is, after all, the genius of Maxwell and of a few other great theoretical physicists whose names are on everyone's lips[3] which endows the simple induction experiments of Hertz and others with such stupendous importance.

Here is the quotation:—

"In extending the researches relative to this part of the investigations, a remarkable result was obtained in regard to the distance at which induction effects are produced by a very small quantity of electricity; a single spark from the prime conductor of a machine, of about an inch long, thrown on to the end of a circuit of wire in an upper room, produced an induction sufficiently powerful to magnetize needles in a parallel circuit of iron placed in the cellar beneath, at a perpendicular distance of 30 feet, with two floors and ceilings, each 14 inches thick, intervening. The author is disposed to adopt the hypothesis of an electrical *plenum* [in other words, of an ether], and from the foregoing experiment it would appear that a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400,000 feet of capacity; and when it is considered that the magnetism of the needle is the result of the difference of two actions, it may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light."

Comparable it is, indeed, for we now know it to be the self-same process.

One immediate consequence and easy proof of the oscillatory character of a Leyden jar discharge is the occurrence of phenomena of sympathetic resonance.

Everyone knows that one tuning-fork can excite another at a reasonable distance if both are tuned to the same note. Everyone knows, also, that a fork can throw a stretched string attached to it into sympathetic vibration if the two are tuned to unison or to some simple harmonic. Both these facts have their electrical analogue. I have not time to go fully into the matter to-night, but I may just mention the two cases which I have myself specially noticed.

A Leyden jar discharge can so excite a similarly-timed neighbouring Leyden jar circuit as to cause the latter to burst its dielectric if thin and weak enough. The well-timed impulses accumulate in the neighbouring circuit till they break through a quite perceptible thickness of air.

Put the circuits out of unison by varying the capacity or by including a longer wire in one of them; then, although the added wire be a coil of several turns, well adapted to assist mutual induction as ordinarily understood, the effect will no longer occur.

That is one case, and it is the electrical analogue of one tuning-fork exciting another. It is too small at present to show here satisfactorily, for I only recently observed it, but it is exhibited in the library at the back.

The other case, analogous to the excitation of a stretched string of proper length by a tuning-fork, I published last year under the name of the experiment of the recoil kick, where a Leyden jar circuit sends waves along a wire connected by one end with it, which waves splash off at the far end with an electric brush or long spark.

I will show merely one phase of it to-night, and that is the reaction of the impulse accumulated in the wire upon the jar itself, causing it to either overflow or burst. [Sparks of gallon or pint jar made to overflow by wire round room.[1]]

The early observations by Franklin on the bursting of Leyden jars, and the extraordinary complexity or multiplicity of the fracture that often results, are most interesting.

His electric experiments as well as Henry's well repay perusal, though of course they belong to the infancy of the subject.

He notes the striking fact that the bursting of a jar is an extra occurrence, it does not replace the ordinary discharge in the proper place, it accompanies it; and we now know that it is precipitated by it, that the spark occurring properly between the knobs sets up such violent surgings that the jar is far more violently strained than by the static charge or mere difference of potentials between its coatings; and if the surgings are at all even roughly properly timed, the jar is bound to either overflow or burst.

Hence a jar should always be made without a lid, and with a lip protruding a carefully considered distance above its coatings: not so far as to fail to act as a safety valve, but far enough to prevent overflow under ordinary and easy circumstances.

And now we come to what is after all the main subject of my discourse this evening, viz. the optical and audible demonstration of the oscillations occurring in the Leyden jar spark. Such a demonstration has, so far as I know, never before been attempted, but if nothing goes wrong we shall easily accomplish it.

And first I will do it audibly. To this end the oscillations must be brought down from their extraordinary frequency of a million or hundred thousand a second to a rate within the limits of human audition. One does it exactly as in the case of the spring—one first increases the flexibility and then one loads it. [Spark from battery of jars and varying sound of same.]

Using the largest battery of jars at our disposal, I take the spark between these two knobs—not a long spark, ¼ inch will be quite sufficient. Notwithstanding the great capacity, the rate of vibration is still far above the limit of audibility, and nothing but the customary crack is heard. I next add inertia to the circuit by including a great coil of wire, and at once the spark changes character, becoming a very shrill but an unmistakable whistle, of a quality approximating to the cry of a bat. Add another coil, and down comes the pace once more, to something like 5000 per second, or about the highest note of a piano. Again and again I load the circuit with magnetizability, and at last the spark has only 500 vibrations a second, giving the octave, or perhaps the double octave, above the middle C.

[1] "Scientific Writings of Joseph Henry," vol. i. p. 201. Published by the Smithsonian Institution, Washington, 1886.

[2] *Loc. cit.*, p. 204.

[3] And of one whose name is not yet on everybody's lips, but whose profound researches into electro-magnetic waves have penetrated further than anybody yet understands into the depths of the subject, and whose papers have very likely contributed largely to the theoretical inspiration of Hertz—I mean that powerful mathematical physicist, Mr. Oliver Heaviside.

[1] During the course of this experiment, the gilt paper on the wall was observed by the audience to be sparkling, every gilt patch over a certain area discharging into the next, after the manner of a spangled jar. It was probably due to some kind of sympathetic resonance. Electricity splashes about in conductors in a surprising way everywhere in the neighbourhood of a discharge. For instance, a telescope in the hand of one of the audience was reported afterwards to be giving off little sparks at every discharge of the jar. Everything which happens to have a period of electric oscillation corresponding to some harmonic of the main oscillation of a discharge is liable to behave in this way. When light falls on an opaque surface it is quenched. What the audience saw was probably the result of waves of electrical radiation being quenched by the walls of the room, and generating electrical currents in the act. It is these electric surgings which render such severe caution necessary in the erection of lightning-conductors. This explanation is merely tentative. I have had no time to investigate the matter locally.

One sees clearly why one gets a musical note: the noise of the spark is due to a sudden heating of the air; now if the heat is oscillatory, the sound will be oscillatory too, but both will be an octave above the electric oscillation, if I may so express it, because two heat-pulses will accompany every complete electric vibration, the heat production being independent of direction of current.

Having thus got the frequency of oscillation down to so manageable a value, the optical analysis of it presents no difficulty: a simple looking-glass waggled in the hand will suffice to spread out the spark into a serrated band, just as can be done with a singing or a sensitive flame, a band too of very much the same appearance.

Using an ordinary four-square rotating mirror driven electromagnetically at the rate of some two or three revolutions per second, the band is at the lowest pitch seen to be quite coarsely serrated; and fine serrations can be seen with four revolutions per second in even the shrill whistling sparks.

The only difficulty in seeing these effects is to catch them at the right moment. They are only visible for a minute fraction of a revolution, though the band may appear drawn out to some length. The further away a spark is from the mirror, the more drawn out it is, but also the less chance there is of catching it.

With a single observer it is easy to arrange a contact maker on the axle of the mirror which shall bring on the discharge at the right place in the revolution, and the observer may then conveniently watch for the image in a telescope or opera-glass, though at the lower pitches nothing of the kind is necessary.

But to show it to a large audience various plans can be adopted. One is to arrange for several sparks instead of one; another is to multiply images of a single spark by suitably adjusted reflectors, which if they are concave will give magnified images; another is to use several rotating mirrors; and indeed I do use two, one adjusted so as to suit the spectators in the gallery.

But the best plan that has struck me is to combine an intermittent and an oscillatory discharge. Have the circuit in two branches, one of high resistance so as to give intermittences, the other of ordinary resistance so as to be oscillatory, and let the mirror analyze every constituent of the intermittent discharge into a serrated band. There will thus be not one spark, but a multitude of successive sparks, close enough together to sound almost like one, separate enough in the rotating mirror to be visible on all sides at once.

But to achieve it one must have great exciting power. In spite of the power of this magnificent Wimshurst machine, it takes some time to charge up our great Leyden battery, and it is tedious waiting for each spark. A Wimshurst does admirably for a single observer, but for a multitude one wants an instrument which shall charge the battery not once only but many times over, with overflows between, and all in the twinkling of an eye.

To get this I must abandon my friend Mr. Wimshurst, and return to Michael Faraday. In front of the table is a great induction coil; its secondary has the resistance needed to give an intermittent discharge. The quantity it supplies at a single spark will fill our jars to overflowing several times over. The discharge circuit and all its circumstances shall remain unchanged. [Excite jars by coil.]

Running over the gamut with this coil now used as our exciter instead of the Wimshurst machine—everything else remaining exactly as it was—you hear the sparks give the same notes as before, but with a slight rattle in addition, indicating intermittence as well as alternation. Rotate the mirror, and everyone should see one or other of the serrated bands of light at nearly every break of the primary current of the coil. [Rotating mirror to analyze sparks.]

The musical sparks which I have now shown you were obtained by me during a special digression[1] which I made while examining the effect of discharging a Leyden jar round heavy glass or bisulphide of carbon. The rotation of the plane of polarization of light by a steady current, or by a magnetic field of any kind properly disposed with respect to the rays of light, is a very familiar one in this place. Perhaps it is known also that it can be done by a Leyden jar current. But I do not think it is; and the fact seems to me very interesting. It is not exactly new—in fact, as things go now it may be almost called old, for it was investigated six or seven years ago by two most highly skilled French experimenters, Messrs. Bichat and Blondlot.

[1] Most likely it was a conversation which I had with Sir Wm. Thomson, at Christmas, which caused me to see the interest of getting slow oscillations. My attention has mainly been directed to getting them quick.

But it is exceedingly interesting as showing how short a time, how absolutely no time, is needed by heavy glass to throw itself into the suitable rotatory condition. Some observers have thought they had proved that heavy glass requires time to develop the effect, by spinning it between the poles of a magnet and seeing the effect decrease; but their conclusions cannot be right, for the polarized light follows every oscillation in a discharge, the plane of polarization being waved to and fro as often as 70,000 times a second in my own observation.

Very few persons in the world have seen the effect. In fact, I doubt if anyone had seen it a month ago except Messrs. Bichat and Blondlot. But I hope to make it visible to most persons here, though I hardly hope to make it visible to all.

Returning to the Wimshurst machine as exciter, I pass a discharge round the spiral of wire inclosing this long tube of CS_2, and the analyzing Nicol being turned to darkness, there may be seen a faint—by those close to not so faint, but a very momentary—restoration of light on the screen at every spark. [CS_2 tube experiment on screen.]

Now I say that this light restoration is also oscillatory. One way of proving this fact is to insert a biquartz betweeen the Nicols. With a steady current it constitutes a sensitive detector of rotation, its sensitive tint turning green on one side and red on the other. But with this oscillatory current a biquartz does absolutely nothing. [Biquartz.]

That is one proof. Another is that rotating the analyzer either way weakens the extra brightening of the field, and weakens it equally either way.

But the most convincing proof is to reflect the light coming through the tube upon our rotating mirror, and to look now not at the spark, or not only at the spark, but at the faint band into which the last residue of light coming through polarizer and tube and analyzer is drawn out. [Analyze the light in rotating mirror.]

At every discharge this faint streak brightens in places into a beaded band: these are the oscillations of the polarized light: and when examined side by side they are as absolutely synchronous with the oscillations of the spark itself as can be perceived.

Out of a multitude of phenomena connected with the Leyden jar discharge I have selected a few only to present to you here this evening. Many more might have been shown, and great numbers more are not at present adapted for presentation to an audience, being only visible with difficulty and close to.

An old and trite subject is seen to have in the light of theory an unexpected charm and brilliancy. So it is with a great number of other old familiar facts at the present time.

The present is an epoch of astounding activity in physical science. Progress is a thing of months and weeks, almost of days. The long line of isolated ripples of past discovery seem blending into a mighty wave, on the crest of which one begins to discern some oncoming magnificent generalization. The suspense is becoming feverish, at times almost painful. One feels like a boy who has been long strumming on the silent keyboard of a deserted organ, into the chest of which an unseen power begins to blow a vivifying breath. Astonished, he now finds that the touch of a finger elicits a responsive note, and he hesitates, half delighted, half affrighted, lest he be deafened by the chords which it would seem he can now summon forth almost at will.

ON THE LIMIT OF THE SOLAR SPECTRUM, THE BLUE OF THE SKY, AND THE FLUORESCENCE OF OZONE.

THERE are two facts of particular interest which have been observed in connection with the light which we receive from the sun and the sky. First, though the ultra-violet spectrum of the sun is very well represented by the iron spectrum taken from the electric arc, yet its length is nothing like so great, and there is no fading away of feeble lines and a weakening of strong ones, which would be the case if the rays were affected by a turbid medium through which they were transmitted, but there is a sudden and sharp extinction which points to a very definite absorption. Secondly, light from the clearest sky, unaffected by aqueous vapour, is of a deep and beautiful blue colour, more of

an indigo-violet tint than ordinary so-called sky-blue. There is nothing more beautiful in Nature than the blueness of the heavens.

The limitation of the solar spectrum has been the subject of elaborate investigation by M. Cornu.[1] He proved by direct experiment that the ultra violet rays are absorbed with energy by the atmosphere, and showed that there is a variation in the amount of absorption corresponding with different altitudes, so that the absorbent matter is at each elevation proportional to the barometric pressure, and consequently in constant relation to the mass of the atmosphere. This fact alone is sufficient to exclude water-vapour from consideration as being the medium of absorption. Moreover, water-vapour, while it absorbs the red and infra-red rays, transmits the ultra-violet very completely.

Photographs taken in 1879 on the Riffelberg, at an altitude therefore of 8414 feet, reached to wave-length 2932; but at Viège, an altitude of 2165 feet, to only 2954.

In short, it was shown that within the limits of altitude at which Cornu's observations were made there was a difference of 10 tenth-metrets of wave-length for every 984 feet of dry atmosphere, the shortening of the spectrum being due to the gaseous constituents. Notwithstanding this, it was stated by Prof. Liveing, in a lecture delivered at the Royal Institution on March 9, 1883, that we must suppose the absorbent substance, whatever it may be, is not in our atmosphere, because, when observations are made upon the summit of an elevation like the Riffelberg, the lengthening of the spectrum reaches only a very trifle beyond U. "The same reason will lead us to reject the notion that the absorption can be due to matter in interplanetary space, for it is not easy to suppose that the gases which pervade that space in extreme tenuity can differ much from those in our atmosphere, because the earth in its annual course must pick up whatever they are, and they must then diffuse into our atmosphere. The absorption is, therefore, probably neither in our atmosphere nor in interplanetary space, and we must look for it in the solar atmosphere."

It is then suggested that the blotting out of the sun's light beyond U is caused by something in the solar atmosphere higher up than the metallic vapours which give rise to Fraunhofer's lines, but at a lower temperature. It has been shown by Young that the Fraunhofer lines are bright lines, but appear black against the brilliant light of the photosphere. It never appeared to me that Prof. Liveing's objection to Cornu's explanation of the limitation of the solar spectrum by our atmosphere was valid, because it was proved beyond question that the atmosphere absorbs the ultra-violet rays, and also that on the same day and hour, at different elevations, the extent of rays absorbed was proportional to the barometric pressure—that is, to the quantity of air through which the rays passed. A considerable acquaintance with absorption-spectra in the ultra-violet region has proved to me that when an absorption-band has been blotted out by increasing the proportion of substance, or by increase of the thickness of the absorbent layer, a stage is soon reached at which any further increase only causes a trifling difference in absorbent action, and in fact that many substances attain a maximum of absorption beyond which there is no change unless we increase the density of the substance, and so probably alter its molecular structure. Under the same conditions of pressure, increased thickness of the absorbent layer only very slightly increases the absorbent action, and that in a degree which is by no means proportional to the layer of material.

There is a difficulty in accepting Prof. Liveing's views, because we know nothing, as he remarks, in the solar atmosphere capable of causing such absorption, and at the same time of transmitting the Fraunhofer lines of the less refrangible portions of the spectrum in the condition in which we observe them.

The matter was very fully considered by me two years previously—that is to say, in the year 1881. The absorption spectra of various gases were examined by photographing the ultra-violet rays which were transmitted by carefully measured quantities of gas at the atmospheric pressure, and one of these gases was ozone. It proved to be a substance with most extraordinary absorptive powers, so that even when very much diluted it exhibited an absorption-band of great intensity which was carefully investigated. By examining the spectrum transmitted by increasing quantities of ozone the band disappeared, and there was a complete and total absorption of rays extending to about wave-length 3160. Any further increase did not cause a

[1] "Sur l'Absorption Atmosphérique des Radiations ultra-violettes," *Journ. de Physique*, t. x. 1881.

corresponding shortening of the spectrum. The band was observed between wave-lengths about 2850 and 2320; but with increased proportions of ozone the rays transmitted were restricted to about 2920, and became more restricted in presence of greater quantities of gas.[1] Thus:—

Extreme ray transmitted by ozonized oxygen.		Length of column of gas traversed by the rays.		Actual volume of ozone per square centimetre of sectional area of column.
λ.		Centimetres.		Cubic cent.metres.
3035	...	92	...	1·012
3150	...	196·5	...	2·162
3160	...	288·5	...	3·175

It was found that a quantity of ozone proportional to the average quantity present in a vertical column of the atmosphere caused an absorption similar to that observed in the solar spectrum—that is to say, terminating about 2950. Largely increased quantities did not largely, but only in a trifling degree, increase the absorption. Furthermore, it was shown that the atmosphere contained ozone as a normal constituent, and that it was present in greater proportion in the upper regions than near the earth's surface. It was proved that all the other minute constituents of the atmosphere were either non absorbent or exerted absorption in a manner different from that of ozone, and that the quantity of ozone commonly present in the atmosphere is quite sufficient to account for the limitation of the solar spectrum, without taking into account the possible absorption caused by the great thickness of oxygen and of nitrogen. The possibility of oxygen being the absorptive substance seemed very great, considering the small difference in constitution between the molecules of ozone and oxygen. It must be understood that the conclusions were arrived at by reasoning from strictly quantitative experiments, and seemed almost incontrovertible, and it may be stated that none of the facts alleged have ever been questioned. It was impossible to deal with the matter further without costly appliances for the compression of oxygen and nitrogen into tubes capable of holding a quantity comparable with the amount of oxygen and of nitrogen in a vertical column of the atmosphere, and for this reason the investigation fell into abeyance.

It was also considered that the problem might be attacked in another manner.

Messrs. Liveing and Dewar have recently made a very interesting and important communication to the *Chemical News* (vol. lviii. p. 163), on the absorption-spectrum of oxygen. In a tube 1·6 metre in length, filled with the gas at a pressure of 160 atmospheres, all rays were absorbed beyond wave-length 2665, but they began to diminish at 2705. With a tube 6 metres or 20 feet long, and with a pressure of 90 atmospheres, it seems that an absorption-band is to be traced at wave-length 3640 to 3600, and there is a complete absorption beyond 3360.

The gas seen in quantity corresponding to that in a vertical column of the atmosphere appears to have a faint blue tint. There can be no doubt whatever that the oxygen of the air exerts a powerful absorption on the rays of the sun, but it does not appear from these experiments that this absorption is exactly the cause of the limitation of the spectrum, as described by Cornu, since when observed in tubes it is carried into a region of longer wave-length than is observed at the level of the sea; thus at Dublin the limit in summer is usually about 3130.[2] It is no doubt the high density of the gas which causes the absorption to be stronger than that of the atmosphere. It should be noted that the oxygen in the 20-foot tube was the quantity in a vertical column of the atmosphere. It is probable that there are several substances in interplanetary space, or in the solar atmosphere, which, besides oxygen and ozone in the air, cause an absorption of the sun's rays and a limitation of the length of the spectrum, but as Messrs. Liveing and Dewar point out, our atmosphere places a limit to the observations we can make on the rays of other heavenly bodies.

Touching the colour of the sky, Prof. Tyndall has told us that four centuries ago it was believed that the floating particles in the atmosphere render it a turbid medium through which we

[1] "On the Absorption-Spectrum of Ozone, and on the Absorption of Solar Rays by Atmospheric Ozone," Journ. Chem. Soc. 1881, xxxix., pp. 57, 111, 119.

[2] In the *Phil. Mag.*, September 1888, p. 288, Messrs. Liveing and Dewar refer only to my first paper on the absorption-spectrum of ozone, Journ. Chem Soc., xxxix, p. 57, but not to the more complete paper on this spectrum at pp. 111–119, *loc. cit.*, which indicates the possible limits of the ozone in the atmosphere.

look at the darkness of space. The blue colour, according to his view, is supposed to be caused by reflection from minute particles, which can reflect chiefly the blue rays by reason of their small size. Experiments on highly attenuated vapours during condensation to cloudy matter were the basis of this reasoning. It always seemed to me that if a view be seen through a turbid medium which reflects chiefly the blue rays, it would not appear blue, but the complementary colour, yellow; and therefore this theory could not account for the blue of distance. In fact, when a light mist hangs over the surface of the earth, and the rays of the sun are transmitted in a direction approaching the horizontal, the result is that the sun and all objects lying in the direction looking towards it appear yellow, while the mist in the opposite direction appears blue, and only translucent, not transparent. The blue of the sky, if caused by such a similar action of floating particles, would not be seen when the sun was overhead, nor could it be seen by looking in the direction of the sun.

The blue would not be transparent and in character similar to the blue of a clear distance, in which the outlines of mountains and rocks are perfectly distinct and sharp, the shadows being of an intensely deep blue, and the most distant objects the deepest in colour. In 1880, Messrs. Hautefeuille and Chappuis liquefied ozone, and found that its colour was indigo blue (*Comptes rendus*, xcv. p. 522). On December 12, 1880, M. Chappuis presented the Academy of Sciences of Paris with a paper on the visible spectrum of ozone. He recognized the most easily visible of the absorption-bands of ozone in the solar spectrum, and in consequence he stated that a theory of the blue colour of the sky could not be established without taking into account the presence of ozone in the atmosphere, for the luminous rays which reach us will of necessity be coloured blue by their transmission through the ozone contained in the atmosphere. And since ozone is an important constituent of the upper atmosphere, its blue colour certainly plays an important part in the colour of the sky. In March 1881, quantitative experiments made by me were published to show how much of blueness could be communicated to layers of gas of different thicknesses when given volumes of ozone are present. I showed that ozone is a normal constituent in the upper atmosphere, that it is commonly present in fresh air, and I accounted for its abundance during the prevalence of westerly and south-westerly winds. It was likewise shown that it was impossible to pass rays of light through as much as 5 miles of air without the rays being coloured sky-blue by the ozone commonly present, and that the blue of objects viewed on a clear day at greater distances up to 35 or 50 miles must be almost entirely the blueness of ozone in the air. The quantity of ozone giving a full sky-blue tint in a tube only 2 feet in length is 2½ milligrammes in each square centimetre of sectional area of the tube. It is necessary to mention that a theory of the blue of the sky was propounded by M. Lallemand ("Sur la Polarisation et la Fluorescence de l'Atmosphère," *Comptes rendus*, lxxv. p. 707, 1872) after his observations had been found inconsistent with all previous explanations. If the coloration be due to reflection from minute particles of floating matter, or if it be due to white light being transmitted through a blue gas, the blue portion of the sky should be polarized quite as much as white light coming from the same direction in the heavens. But the experiments of M. Lallemand prove that this is not so. Upon these experiments he bases his theory that the blue colour of the atmosphere is due to a blue fluorescence like that seen in acid solutions of sulphate of quinine—that is to say, caused by a change of refrangibility in the ultra-violet rays.

Ångström first threw out the idea of fluorescence being a property of certain gases in the atmosphere. To possess this property the gas must be capable of absorbing either in part or entirely the ultra-violet and violet rays, and of emitting them with a lowered refrangibility and without being polarized. Ozone possesses the property of absorption in the highest degree in the ultra-violet region, and I have now to announce that strongly ozonized oxygen is highly fluorescent when seen in a glass bottle two inches in diameter illuminated by an electric spark passing between cadmium electrodes. The colour of the fluorescence is a beautiful steel blue. This fluorescence has not been observed in other gases, but it is in the highest degree probable that oxygen is fluorescent, though this has yet to be proved. There can be, however, little doubt that the colour of the sky is caused in part by the fluorescence of ozone, and also to some extent by the transmission of rays through the blue gas. The blue of distance is doubtless to be attributed more to transmission than the blue of the sky, though it is quite conceivable that fluorescence also here comes into play. Whatever other cause concurs in the production of the blue of the heavens, it has certainly been established by M. Chappuis that the properties of ozone participate in its production.

In August 1884, a very short note was sent by me to NATURE concerning the red solar halo seen at Zermatt and on the Riffelberg with great distinctness. I recorded the occurrence of a dark band in the spectrum, slightly more refrangible than D, which was seen to vary in intensity; a second band a little less refrangible than D was also observed. On account of the altitude at which the observations were made, viz. 9000 feet, and the state of the weather at the time, these bands were considered to be due to some constituent of dry air.

The subject of the telluric rays has become of increased interest since M. Cornu has studied the dark lines in the neighbourhood of D, but unfortunately the rays absorbed to which I refer are both a little more and a little less refrangible than those figured on his map of this region. If we accept the number 5890 tenth-metres as approximately representing the mean value of the lines D^1 D^2, the narrow bands observed by me have wave-lengths about (1) 5950 and (2) 5770 at their darkest parts, as far as one can ascertain with a hand spectroscope giving excellent definition but small dispersion. They are very variable, being dependent on the state of the weather, and are more distinct and broader when viewed with the sun on the horizon. In London during the dry calm weather of June and July 1884, they were very strong, but variable in different parts of the sky.

The less refrangible band, or broad line as it usually appears, below D, is generally over-lapped by a band belonging to water vapour, the chief "rain-band." On this account observations at an elevation of 10,000 feet or so during perfectly dry weather were considered of interest. The bands were observed against the blue sky on several occasions, but they were also at other times entirely absent or barely visible. There is some liability to a group of iron, barium, and other solar lines being mistaken for the more refrangible band when it is not decidedly strong. Chappuis observed bands in the blue sky coincident with ozone bands, and I have on that account always expected to find some indication of the spectrum of ozone in the upper atmosphere, but the reason why there must always be a difficulty in obtaining evidence of any absorption due to this substance arises from the strongest visible band of ozone, with wave-length 6095 to 5935, being masked by the band of water vapour; and secondly, because the total amount of white light so preponderates as to overpower the effect of absorption—that is to say, the rays absorbed are only a small fraction of those transmitted, so that the bands are faint and the colour due to absorption is either not seen or seen only with difficulty. Owing to this fact we cannot distinguish the blue colour of the clouds when the sunlight is bright; but when the sky is completely over-clouded with cumuli a faintly bluish tint is given to the cloud-shadows, even at the zenith. Near the horizon not only are the bright parts of the clouds blue, but their shadows have a rich blue tint.[1] The blueness varies somewhat: at times it may be seen to shift about in the sky; it has been observed, for instance, to pass over from south-west to north-east. The second but less conspicuous band of ozone absorbs rays with wave-length 5770 to 5600. Both bands have been observed in a dry atmosphere at elevations varying from 6000 to 10,000 feet, both in the blue of the sky and against white clouds. The measurements, very imperfectly made under difficulties, showed them to have wave-lengths about (1) 5950, (2) 5770, in the centre of the dark portion, while, according to Chappuis, the bands of ozone are—

(1) 6095 to 5935	...	mean, 6010
(2) 5770 to 5600	...	mean, 5680.

On Ångström's chart, a dark band, diminishing in depth towards the east, extends from 5785 to 5680, which is classed among the *raies atmosphériques*; this is similar to the band observed by me when viewing the sun or bright clouds near the horizon, and is similar to the second ozone band.

The work of Prof. Piazzi-Smyth, "Madeira Spectroscopic," does not give that portion of the spectrum which would serve for comparison. The "low-sun band," δ, comes very near to band (2), wave-length 5770, while the "rain-band" comes very near (1), 6095. On several subsequent occasions the two bands were

[1] Prof. Pickering has proved that sunlight as it reaches us is blue, which must be the case if it has passed through a blue medium (Proc. American Academy of Sciences, 1880, p. 236).

observed at lower levels than Zermatt and the Riffel, but with even less intensity. On the last occasion (November 10, 1884) that such an observation was recorded, there was only a trace of these faintly seen. No doubt a clear atmosphere, free from the turbidity so easily created by condensing moisture, is essential to their visibility. Schœne has observed bands in a bright sky before sunrise and after sunset, during an intense frost in Central Russia. The measurements taken identified them with ozone bands, and leave scarcely any doubt whatever of the presence of ozone in the atmosphere, and if it can be so recognized, it must communicate its characteristic blue colour to the air (Journ. Chem. Soc. Abstracts, vol. xlviii. part 2, p. 713). The remarkable crepuscular phenomena seen at the close of 1883 proved highly favourable to such investigations.

In order to continue a series of observations on the solar spectrum near D it would be best to employ a fairly good dispersion and large lenses with long focus admitting a large amount of light to the eye, or, better still, to specially prepared photographic plates highly sensitive to the yellow rays.

The very extensive absorption of the ultra-violet rays by oxygen leads us to expect it to be fluorescent. All such absorbents are fluorescent more or less, and generally strongly, but when the absorbed rays are of very short wave-length the fluorescence is not always visible. Thus there are many substances which do not appear fluorescent by lime-light nor by dull daylight, but are strongly so when seen by electric light, especially if it has passed through no glass or other medium than a quartz lens and a short column of air. Some substances are not fluorescent when seen in glass vessels, because the glass has absorbed those rays of which the refrangibility would have been lowered by the fluorescent substance. In air, and by the light of an electric spark rich in ultra-violet rays, such as that from cadmium electrodes, almost everything is fluorescent. The whole range of the cadmium spectrum has been viewed by me, owing to the fluorescence of the purest white blotting-paper. The light, of course, is feeble, and the eye has to be trained to make observations in total darkness.

Pure water, however, never appears fluorescent. Some solutions in water, which transmit all the ultra-violet rays as far as 2304, are fluorescent, though whether this is caused by impurities or not has not been decided.

It cannot any longer be doubted (1) that the extreme limit of the solar spectrum observed by Cornu is caused by the gases in the atmosphere, probably both by oxygen and ozone; (2) that the blue of the sky is a phenomenon caused by the fluorescence of the gaseous constituents of the atmosphere, and probably ozone and oxygen are the chief fluorescent substances; (3) that ozone is generally present in the air in sufficient quantity to render its characteristic absorption-spectrum visible, and that therefore it gives a blue colour to the atmosphere by absorption, through which blue medium we observe distant views; (4) that water vapour does not participate in the coloration of the atmosphere under like conditions and in the same manner as ozone.

W. N. HARTLEY.

Royal College of Science, Dublin.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Vice-Chancellor of Cambridge University publishes the following:—

"*Ferndene, Gateshead, March* 4, 1889.

"SIR,—You may be aware that some years ago I erected here a refracting telescope, 25-inch aperture and 30 feet focal length.

"Owing to unfavourable atmospheric conditions and other reasons, the instrument has done no systematic work in its present position. I should much like to place it where it would work under capable direction.

"I contemplate offering my telescope and the dome and instruments connected with it to the University of Cambridge. The part that the University has taken of late years in the advancement of science induces me to hope that the possession of an instrument especially adapted to the study of stellar physics may give impetus to the development of the subject.

"I beg you therefore to give the matter consideration, and let me know what proposals can be made to insure proper use and maintenance of the instruments, and publication of yearly reports, in case my offer is acceptable to the University.

"I have the honour to be, Sir,

"Your obedient Servant,

"R. S. NEWALL.

"The Vice-Chancellor of the University of Cambridge."

At the Congregation to-day, at 2 p.m., the following Grace, having received the sanction of the Council, is to be offered to the Senate:—

"That the Vice-Chancellor, Dr. Routh, Dr. Glaisher, Prof. Adams, Prof. Liveing, Prof. Darwin, and Prof. Thomson, be appointed a Syndicate to consider Mr. R. S. Newall's munificent proposal to present his telescope to the University, and the arrangements and expenditure which would be necessary to maintain and utilize it for astronomical and physical research, and to report to the Senate."

SCIENTIFIC SERIALS.

IN the *Journal of Botany* for February and March, Mr. A. Fryer continues his notes on pond-weeds, the present instalment being characterized by two plates and the description of a new species, *Potamogeton falcatus*, from Huntingdonshire. Another addition to the British flora is recorded by the Rev. E. S. Marshall, in *Festuca heterophylla*, from Witley, in Surrey; and Mr. F. J. Hanbury describes no less than four species or sub-species of the difficult genus, *Hieracium*, new to science—all from the extreme north of Scotland. Messrs. G. Murray and L. A. Boodle commence a monograph of *Avrainvillea*, a genus of siphonocladaceous Algæ, which they consider most nearly allied to *Penicillus* and *Udotea*.

THE *Botanical Gazette* for January contains a short description and history, illustrated by five plates, of the new Botanical Laboratory at the University of Philadelphia. The occurrence of a new phosphorescent Fungus is noted, *Agaricus* (*Clitocybe*) *illudens*, in which the phosphorescence appears to reside in the hymenium. In the number for February, Dr. Henrietta E. Hooker gives a highly interesting description of the structure and mode of life of the common dodder of Massachusetts, *Cuscuta Gronovii*. It is stated to have a habit of entirely withdrawing its roots from the soil by the contraction of the coils of its twining stem as soon as it commences to lead a parasitic life on its host. Miss Emily L. Gregory continues her paper on the "Development of cork-wings on certain trees."

Rivista Scientifico-Industriale, January 31.—On the oxalate of lime in plants, by Prof. Aser Poli. In these remarks, which are made in connection with C. Acqua's recent contribution to the study of the crystals of the oxalate of lime in plants, it is argued that even on Acqua's own showing, the presence of these crystals cannot be regarded as necessary to the life of the plant. In some they are not found at all, and where they do exist they seem to be rather an inevitable consequence of the production of oxalic acid in the presence of the salts of lime.—Prof. Ercole Fossati continues, without concluding, his elaborate monograph on the thermic and electric properties of iron subjected to magnetic influences.

Bulletin de la Société des Naturalistes de Moscou, 1888, No. 3. —On the peculiarities of the skulls of the horned cattle of the Kalmucks, by P. Kuleschoff (in German), with photographs of skulls. The great likeness between the skulls of the Kalmuck race of horned cattle and those of *Bos sondaicus* and the zebra brings the author to the conclusion that the ancestors of the European cattle must be searched for in India.—On the Orthopteres of Crimea, by O. Retowski (with three plates).—On the structure of the pseudo-scorpions, or *Chernetidæ*, by A. Croneberg (both in German). The differences in the structure of this group and that of the scorpions proper are pointed out, and Thorell's views on the affinities of the *Chernetidæ* are confirmed.—Review of the generative organs of the *Pompilidæ*, by General Radoszkowski (in French, with four plates).—Supplementary note on the great comet 1887 I., by Th. Bredichin (in French). It belongs to the author's third class of comets—that is, it consists of heavy elements; and more accurate calculations convince the author that it could consist only of elements having a great molecular weight, such as gold, mercury, and lead.—Some additions to the flora of Moscow, by S. Milutin.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 21.—"The Innervation of the Pulmonary Vessels." By J. Rose Bradford, M.B., D.Sc., George Henry Lewes Student, and H. Percy Dean, M.B., B.S., B.Sc. Communicated by E. A. Schäfer, F.R.S. (From the Physiological Laboratory of University College, London.)

Hitherto no direct experimental proof of the existence of vasomotor fibres for the vessels of the mammalian lung has been obtained. The method used by us consisted in exciting the roots of the upper dorsal nerves, and recording simultaneously the effects produced on the aortic and pulmonary blood-pressure. The aortic pressure was measured in the usual way, *i.e.* a mercurial manometer was connected with the carotid artery. A second manometer was then connected with the branch of the pulmonary artery distributed to the lower lobe of the left lung. All the observations were made on dogs.

Before considering the results obtained by excitation of the upper dorsal nerves, it is necessary to know what effect a given rise of aortic pressure will have on the pulmonary pressure. This was determined by the three following methods:—

I. Excitation of the peripheral end of a divided splanchnic.

II. Excitation of the lower end of the spinal cord divided in the middle of the dorsal region.

III. Compression of the thoracic aorta.

By all these methods an enormous rise in the systemic blood-pressure is obtained, but the simultaneous pulmonary rise is very small. Thus the aortic pressure may be doubled or even quadrupled. The rise of the pulmonary pressure is always small and sometimes absent. The rise, as a rule, is about one-fifth of the total pulmonary pressure, *i.e.* the rise is then very small compared with the doubling or quadrupling of the systemic pressure. The relative ratio of the pulmonary rise to the carotid rise is that the former is about one-twentieth of the latter. Hence an enormous increase of systemic pressure is required in order to cause even a small rise of pressure in the pulmonary vessels.

The same occurs when the aorta is compressed, no rise of pulmonary pressure occurs unless the aortic rise is not only very marked but also of some duration, *e.g.* 30 seconds.

Excitation of the central end of the sciatic causes but a very slight pulmonary rise. The central end of the vagus gives larger effects, due to reflex contraction of the pulmonary arterioles, and the central end of a posterior root of one of the upper dorsal nerves gives still greater effects.

If the vasomotor centre in the medulla be excited, a great rise of both pulmonary and systemic pressure is produced. The same excitation of the medulla after division of the cord in the mid-dorsal region produces as great a pulmonary rise, but the systemic effect is now quite small.

Excitation of the upper dorsal roots, *i.e.* from the second to the seventh, produces rises of pulmonary pressure as marked as any of the preceding, but accompanied either by a small aortic rise or by no aortic effect. With the third nerve a marked rise of pulmonary pressure accompanied by a fall of aortic pressure is seen. Hence these nerves contain the vaso-constrictor fibres for the pulmonary vessels, since the effects produced on the pulmonary pressure must be due to constriction of the pulmonary arterioles, inasmuch as the simultaneous carotid effects are quite incompetent to cause them.

Finally, although it is undoubted from the results of the research that the mammalian pulmonary vessels receive vasomotor fibres, yet it is probable that this vasomotor mechanism is but poorly developed when compared to that regulating the systemic vessels.

Geological Society, February 20.—Dr. W. T. Blanford, F.R.S., President, in the chair.—The President announced that a special general meeting, for the consideration of the by-laws, would be called for Friday, March 15, at 4.30 p.m.—The following communications were read:—On the Cotteswold, Midford, and Yeovil Sands, and the division between Lias and Oolite, by S. S. Buckman. After giving a short sketch of the work and opinions of other writers, the author proceeded with the evidence on which his own views are based. He described a series of sections of the typical exposures of "sands" and contiguous strata, commencing near Stroud and terminating on the Dorset coast. Dividing the series into *seven* horizons, characterized by their distinctive Ammonites, viz. *Amm. communis*, *variabilis*, *striatulus*, *dispansus*, the genus *Dumortieria*, *Amm. Moorei*, and *opalinus*, and taking the *striatulus* beds as a fixed starting-point, the author demonstrated how the strata varied in regard to that horizon. The Cotteswold Sands, containing the *variabilis* and part of the *communis* horizons, were below the *striatulus* beds; the Midford Sands, containing the *dispansus* horizon, were above, *Gramm. striatulum* occupying a thin bed at the base; the Yeovil Sands, containing the *Moorei* and *Dumortieria* horizons, overlay a bed containing Ammonites of the *dispansus* horizon, and were consequently still later deposits. Since the different sands were deposited not on a horizontal plane, in point of time, but, as it were, obliquely, the deposit of Cotteswold Sands having ceased before that of Yeovil Sands commenced, it was incorrect to lump all the "sands" from the Cotteswolds to the Dorset coast under the single local name "Midford Sands," thereby implying a contemporaneity which did not exist, while the use of the present restricted local names was defended. The Ammonites were apparently uninfluenced by changes in the character of the deposit, since the same species are found in Limestone in the Cotteswolds, in Sands at Midford, and in argillaceous Marl at Ilminster. The change from argillaceous to arenaceous or calcareous deposits has been looked upon as so distinct a feature, that it has been utilized as a great argument in favour of drawing the line between Lias and Oolite at that point; but if this be done, the line is always drawn at different horizons in different districts. If lithology furnishes no reason for a dividing-line at this point, it was shown that neither did palæontology. It was also shown that the Ammonite family Hildoceratidæ dominated the period from the *falcifer* to the *concavus* zones, and that with the close of the latter zone they died out with singular abruptness, and that, furthermore, there exists, both in England and upon the Continent, a marked hiatus at the same point due to the absence of a zone or a number of zones. On account of these facts the proposal was put forward that d'Orbigny's term "Toarcien" should be employed to designate the strata fron the *falcifer* zone to the *concavus* zone inclusive, that this term should not be used in the sense of merely an extended "Upper Lias," but to mark an entirely distinct transition-formation—a definite part of the Jurassic period—separating the typical Lias from the mass of thoroughly Oolitic strata. The reading of the paper was followed by a discussion, in which the President, Mr. H. B. Woodward, Prof. Blake, Rev. H. H. Winwood, and Mr. Hudleston, took part.—On some nodular felstones of the Lleyn Peninsula, by Miss Catherine A. Raisin. Communicated by Prof. T. G. Bonney. Some remarks on the paper were made by Mr. Cole, Dr. Hicks, and Prof. Bonney.—On the action of pure water, and of water saturated with carbonic acid gas, on the minerals of the mica family, by Alexander Johnstone.

PARIS.

Academy of Sciences, March 4.—M. Des Cloizeaux, President, in the chair.—Remarks accompanying the presentation of a work entitled "Introduction à l'étude de la Chimie des anciens et du moyen age," by M. Berthelot. This work forms a sequel to the author's "Origines de l'Alchimie" and "Collection des anciens Alchimistes grecs," thus completing a series of historical researches which fully establish the true character of the old philosophic doctrines, methods, and practices, which were hitherto supposed to be mainly absurd and fanciful, but which must henceforth enter into the scheme of historical evolution of the positive sciences. Here M. Berthelot gives a full description and translation of the Leyden papyrus of Egyptian origin, the oldest extant treatise on chemistry. The signs, notations, and appliances of the ancient alchemists are also described and reproduced by the photogravure process.—On the artificial reproduction of halos and parhelic circles, by M. A. Cornu. The author obtains the halos more effectively than by Brewster's method, by depositing on a sheet of glass a crystalline powder of potash alum, which is obtained from a heated saturated solution shaken while cooling. The phenomenon is sufficiently bright to be projected and rendered visible to an audience. This remark applies also to the parhelic circles, which are obtained by an extremely simple process.—On the chondroid plaques in the tendons of birds, by M. L. Ranvier. By employing a solution of osmic acid, the author shows that the chondroid plaques in the tendons of the feet of finches and other small birds contain cellules filled with a fatty substance. By means of other reagents the presence is also revealed of glycogen and cartilage.—On the great storm of March 11, 12, and 13, 1888, in the United States, by M. H. Faye. Lieut. George Dyer's monograph on this terrific hurricane with its accompanying blizzard describes it as of an exceptional character,

to which is inapplicable the law based on the circular theory—that is, the eight-point rule. Instead of the usual barometric pressure in form of a basin, it is stated to have presented the aspect of an immense linear depression, a trough of low barometer moving eastwards. This view is incompatible with the well known hypothesis advocated by M. Faye, who accordingly endeavours here to account for the recorded phenomena without having recourse to the "highly improbable theory of a vast trough of low temperature."—On the complete rectification of the sextant, by M. Gruey. Two methods are proposed, dealing with the rectification of the axis of rotation, R, of the large mirror M, with the rectification of the large and small mirrors M and M', and with that of the axes U and S.—On the separation of zinc and cobalt, by M. H. Baubigny. In a previous paper (*Comptes rendus*, cviii. p. 236) the author described the process by which he has succeeded in completely separating zinc and nickel. The further experiments here carried out tend to show that, even in the state of sulphates, zinc and cobalt cannot be totally separated by sulphuretted hydrogen in presence of a small excess of free sulphuric acid unless the quantity of cobalt be relatively slight. The separation may also be effected with sufficient completeness if the quantity of zinc be slight.—Observations on saccharification by diastase, by M. L. Lindet. During saccharification the decomposition of the starch into maltose and dextrines is always accompanied by a secondary reaction in which the diastase attacks the dextrines and transforms them to maltose. This reaction is arrested by the presence of a certain quantity of maltose, which, however, may be removed by alcoholic fermentation, as maintained by Payen, although denied by O'Sullivan and others. Here M. Lindet confirms Payen's theory by another process, employing phenylhydrazine as a reagent for precipitating the maltose in the state of insoluble phenylmaltosazone.—On some new neutral and acid ethers of the camphols, by M. A. Haller. These ethers are formed under the same conditions as the succinates already described in the *Comptes rendus* of February 25.—Influence of mineral substances on the structure of plants, by M. Henri Jumelle. The experiments here carried out with lupins cultivated under like conditions, but some with distilled water, some with Knop's mineral solution, show that the presence of mineral substances is followed by a greater development of sap, and diminished formation of supporting elements; further, that the absence of salts considerably modifies the structure of plants, the modifications, however, being largely due less to the absence of the salts themselves than to the consequent diminution of water attracted and retained by the salts.—Papers were contributed by M. E. Goursat, on isogonal transformations in mechanics; by M. S. Arloing, on the general effects of the substances produced by *Bacillus heminecrobiophilus* under natural and artificial culture; by M. Stanislas Meunier, on the Carboniferous rocks containing *Bacillarites*, Stur; and by M. de Rouville, on the genus *Amphion* (*Pander*) in the Cabrières district, Hérault.

Berlin.

Physiological Society, February 1.—Prof. du Bois Reymond, President, in the chair.—Prof. Moebius spoke on the movements of the flying-fish through the air. He first described, from personal observation, the way in which the fish shoot out of the water from both bows of the ship, and then propel themselves horizontally for a distance of several ship's-lengths with their pectoral and abdominal fins stretched out flat, skimming along without moving their fins, always in the direction of the wind, but either with or against the same. When they meet the crest of a wave they raise themselves slightly in the air, falling again to the same extent in the succeeding trough of the sea. Occasionally a slight buzzing of the fins may be observed, similar to that of the movements of the wings in many insects. At night they frequently fall on the deck of the ship. As the result of a detailed investigation, the speaker had proved that these fish do not fly, since the anatomical arrangements of their fins and muscles are not adapted to this purpose. What really occurs is that when frightened by the approach of a ship or any enemy they shoot up out of the water, as do so many other fish, and are then carried along by the wind, which strikes on the under surface of their outstretched and evenly-balanced fins. Notwithstanding the general acceptance which was accorded to the above investigation, it was urged by many that the buzzing of the fins, the rising over the crest of a wave, and the falling overboard after having landed on the deck of a ship, were evidences that this fish really executes movements which result in flight. In reply to this, the speaker pointed out that the buzzing of the fins takes place when a strong current of air is directed against the outspread fins of a dead flying-fish by means of a bellows, and further, that the rising over the crest of a wave or the bulwarks of a ship may be explained by the ascending currents of air which are always produced whenever a strong horizontal wind strikes against any elevated object such as a wave or part of a ship. Thus, finally, with the exception of the movements involved in its oblique sudden exit from the sea, all the motions of a flying-fish when in the air are really passive.—Dr. Posner spoke on the conversion of mucous membrane into cuticular tissue. It has long been known that ectodermal tissue can become converted into that which is characteristic of the alimentary tract; thus, for instance, when pieces of skin are transplanted into the cavity of the mouth, they become completely converted into mucous membrane, and the epidermis becomes an epithelium. On the other hand, no observations existed as to whether the reverse conversion of mucous into epidermal tissue is possible. No conclusive evidence could be drawn from the cuticular conversions which occur in mucous membranes which are derived from ingrowths of the ectoderm, and equally inconclusive were the cases of cuticular conversion which are observed in strictures of the urethra, and in the bladder, and in cases of papilloma and pachydermia of the mucous membrane of the mouth, larynx, and œsophagus. The speaker had found in Leidig a hint that in those animals which do not chew their food a conversion of the gastric mucous membrane occurs, which often amounts to a real cuticular formation. It is true that in birds the gizzard is possessed of tubular glands which pour out a secretion which at once sets into a mass as hard as bone, and provides a means of comminuting the food. But, on the other hand, cuticular growths are observed in the stomachs of Edentata, which consist of a true conversion of epithelium into epidermis. The speaker had been able to study this conversion in the stomach of Manis, and found by chemical and microscopical investigation that the whole stomach, even down to the region of the pylorus, is lined with a true epidermis, and that typical papillæ are developed underneath the same. The conversion of endodermal into ectodermal structures is hereby clearly proved, a fact which is not devoid of significance in pathology.

February 15.—Prof. du Bois Reymond, President, in the chair.—Prof. F. E. Schulze spoke on the organization and mode of living of Sponges. The simplest form of Sponge consists of a sac, which, being composed of three layers, is equivalent to the embryonic form of the more complicated types. This sac is attached by its base, and has at its apex an opening—the osculum; its wall is composed of an outer layer of epithelial cells (ectoderm), a middle layer of connective-tissue with migratory cells (mesoderm), and an inner layer of collared flagellated cells (endoderm), and is perforated with round and regularly distributed apertures. The more complex forms arise by a thickening and folding of the wall, the thickening being still more marked in the most complicated forms. In these, a system of branched canals takes the place of the simple apertures in the wall, communicating with the exterior by round openings, and leading internally into cavities lined with collared flagellated cells (choanocytes); from these cavities a further system of branched canals leads into the now limited internal cavity with its osculum. The skeleton of the Sponge is composed of chalk, siliceous earth, spongin, or foreign substances. In the simplest Sponges, consisting of a simple sac, the skeleton is made up of the simplest star-shaped spicules, with three rays, so as to give support to a membrane which is perforated with regularly alternating apertures; in the more complicated forms, these spicules possess four or six rays, as supplying the most convenient supporting structure, while a collar of simple rays is developed round the osculum. According to the material of which the skeleton is chiefly composed, Sponges may be classified as chalky, siliceous, horny, or sandy. The Hexactinellidæ, with six-rayed spicules, inhabit deep seas. In all Sponges, a continuous stream of water is observed entering the openings on the surface of the body, and emerging at the osculum, so that the Sponges filter the water in which they live. The movement of the water is brought about by the flagella; the contraction and retraction of the osculum is produced by elongated protoplasmic cells, but these cannot be regarded as muscle-cells until it has been proved to a certainty that they are connected with nerves, and receive their impulse from these nerves. Only one observer has as yet described a nervous system

in Sponges, the majority not yet having seen it. Nothing has as yet been definitely ascertained as to the mode of nutrition of Sponges: from among the several hypotheses, some suppose that the solid organic particles which are suspended in the water are taken up by the ectoderm-cells and digested in the body; or else that these particles are taken up by the flagellated cells—that is to say, are passed from within outwards; or that the digestion is cellular, inasmuch as the amœboid migratory cells take up the food-particles, digest them, and pass on the digestive products to the rest of the body; or, finally, that Sponges, like plants, only absorb food-stuffs in solution. The reproduction of Sponges is both asexual and sexual, the first resulting from natural or artificial fission: natural fission, consisting of a simple separation, by constriction, of a portion of the body-substance, occurs in fresh-water Sponges. A further asexual mode of reproduction is by means of buds and gemmules. In the sexual mode of reproduction, the females develop eggs all over their surface, and the males spermatozoa, the latter consisting of a head and tail. The egg, after impregnation, goes through the various stages of segmentation met with in the higher animals, and then develops into a sac-like embryo.—Dr. Uhthoff gave an account of his researches on the dependence of visual acuteness in spectral colours upon the intensity and wave-length of the light; these have been recently reported to the Physical Society by Dr. Kœnig (NATURE, February 21, p. 408).

Physical Society, February 8.—Prof. Kundt, President, in the chair.—Dr. Michelson spoke on the normal rate of combustion of explosive mixtures of gases. When such a mixture is ignited at one point, and the temperature of combustion is propagated only by conduction from this point, then the surface at which combustion is taking place separates the burnt from the still unburnt portion of the mixture; the temperature of this unburnt portion is then raised by conduction to that at which it ignites, and it burns. Mallard and Le Chatelier have determined the rate at which this ignition is propagated, by observing the onward movement of the flame in a cylindrical vessel filled with the mixture of gases; Bunsen, on the other hand, allowed the explosive mixture to stream out of a burner with a known velocity, and took this as being equal to the rate of propagation of the combustion in the case where the flame was just on the point of striking back into the tube to which the burner was attached. An objection which may be urged against the first method is that the velocity with which the flame is propagated increases very rapidly the further it travels, so that it is uniform only at the beginning of the explosion. The objection to the second method is, that the flame is continually wavering in and out of the edge of the burner. Dr. Michelson made use, in his experiments, of the dark cone in the centre of the flame, in which the gases are still unburnt, and whose luminous envelope forms the limit of the commencing combustion. When the rate of supply of the combustible gases is uniform, this cone is very steady, and the rate at which the gases stream out from its surface is exactly equal to the rate of combustion of these gases. The volumes of the gases consumed were measured in accurate meters, and the size of the luminous envelope to the centre cone of the flame was determined from photographs of the flame. The mixtures examined were those of coal-gas and air, hydrogen and air, carbonic oxide and oxygen, hydrogen and oxygen, carbonic oxide and air, and of methane and air. With coal-gas and air the rate of combustion increased as the mixture contained more of the coal-gas, reaching a maximum with 18 per cent. of this gas, and then gradually became less; the maximum rate of propagation of the combustion was 70 centimetres per second. With hydrogen and air, the maximum rate was observed with 40 per cent. of hydrogen in the mixture, being then 270 centimetres per second, and then becoming less. The curve representing the rate of combustion of carbonic oxide and oxygen presented a very different appearance. The maximum rate was only obtained with 75 per cent. of the carbonic oxide, and was about equal to the maximum for coal-gas. For the other three mixtures, no curves could be drawn. With a mixture of hydrogen and oxygen, the speaker estimated the maximum rate of combustion as being about 10 metres per second, but no actual measurement was possible, since the mixture could not be expelled at this rate from the burner, which consisted of a glass tube 1 metre long.—Prof. Preyer spoke on combination-tones. He endeavoured to prove that difference- and summation-tones have no objective existence. The first of these are the outcome of a co-vibration in the inner ear. When two different tuning-forks are made to vibrate for a long time, and are then damped, and a third tuning-fork, whose vibration-frequency is only slightly greater than that of the difference in frequency of the other two, is applied to the head, the experimenter hears the corresponding beats. Persons with a defective tympanic membrane in one ear, and a normal membrane in the other, were unable to appreciate difference-tones with the former which they could perfectly well do with the latter. According to the speaker, the summation-tones are really difference-tones, due to the fundamental tone and over-tones of the vibrating forks.—Prof. H. W. Vogel exhibited a complete spectrum of cyanogen, which he had obtained by photographing the spectrum of an arc-light produced by a concave grating; the spectrum extended in the red beyond the line A. Most remarkable was the abundance of lines which were fixed on the photograph, and made visible. The speaker discussed briefly the respective advantages of a concave grating for laboratory experiments on spectra, and of the ordinary prism for practical purposes, especially for observations during solar eclipses.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Scottish Moors and Indian Jungles: Captain J. T. Newall (Hurst and Blackett).—The Scientific Works of C. William Siemens, 3 vols.: edited by E. F. Bamber (Murray).—The Collected Mathematical Papers of Arthur Cayley, vol. i. (Cambridge Press).—A Catalogue of the Newton Manuscripts, Portsmouth Collection (Cambridge Press).—The Gamekeeper's Manual: A. Porter (Douglas).—Catalogue of the Fossil Cephalopoda in the British Museum (Natural History), part 1: A. H. Foord (London).—Catalogue of the Chelonians, Rhynchocephalians, and Crocodiles, in the British Museum (Natural History), new edition: G. A. Boulenger (London).—A Hand-book of Cryptogamic Botany: A. W. Bennett and G. Murray (Longmans).—Blackie's Modern Cyclopedia, vol. i.: edited by C. Annandale (Blackie).—A Treatise on Chemistry, vol. ii., part 2, new edition: Sir H. E. Roscoe and C. Schorlemmer (Macmillan).—Henri Sainte Claire Deville; sa Vie et des Travaux: J. Gay (Paris, Gauthier-Villars).—The House that Jack Built in Diversified Consideration: F. J. Wilson (Reeves).—Encyklopædie der Naturwissenschaften Erste Abthg., 59 Liefg.; Zweite Abthg., 51 and 52 Liefg. (Breslau, Trewendt).—Our Earth and its Story: edited by P. Brown (Cassell).—Revision of North American Umbelliferæ: J. M. Coulter and J. N. Rose.—Notes from the Leyden Museum, vol. x. Nos. 1–4, vol. xi. No. 1 (Leyden).—Folk-Lore Journal, vol. vii. part 1 (E. Stock).

CONTENTS.

THURSDAY, MARCH 21, 1889.

BAKU PETROLEUM.

The Region of the Eternal Fire. By Charles Marvin. (London: Allen and Co., 1888.)

THIS book is not, as its title might imply, an eschatological treatise, nor is it a work of fiction after the manner of Mr. Rider Haggard. It is simply a plain, straightforward narrative of a journey to the petroleum region of the Caspian, undertaken with a view of investigating what Mr. Marvin terms "the kerosene factor of the Central Asian problem." It has, however, this connection with eschatology, that the region of which it treats is, or was, holy ground. The peninsula of Apsheron, on which Baku stands, has been famous from time immemorial, and even before the time of Cyrus thousands of the followers of Zoroaster had worshipped on its sacred soil. With the conquest of Persia, first by Heraclius, and twelve years later by the Arabs, the power of the Magi of the Zoroastrian sect was shattered; and the worship of the Eternal Fire in the Surakhani temple for ever passed away, and in its place are now the symbols of a new cult in the shape of greasy derricks and dingy kerosene distilleries.

The story of Baku and its Oil King, Ludwig Nobel, reads like a tale of the "Arabian Nights." Ten years ago the place was a sleepy Persian town: it is now a thriving city, owning more shipping than Cronstadt or Odessa, and the centre of a vast and rapidly increasing trade. But even in the thirteenth century the "sacred element" was so far robbed of its sanctity that the crude petroleum was extensively exported into various parts of Asia. In "The Book of Ser Marco Polo, the Venetian," edited by Colonel Yule, we read that—

"On the confines towards Georgine there is a fountain from which oil springs in great abundance, inasmuch as a hundred shiploads might be taken from it at one time. This oil is not good to use with food, but 'tis good to burn, and is also used to anoint camels that have the mange. People come from vast distances to fetch it, for in all countries round there is no other oil."

Jonas Hanway, to whom Englishmen must be for ever grateful for the introduction of the umbrella to this country, visited Baku about the middle of the eighteenth century in the interest of one of the powerful trading companies of the time; and in 1754 he published a very complete account of the district and of the uses to which its naphtha or petroleum was put. The oil was then, as now, mainly employed for light and fuel, but we are also told that—

"The Russians drink it both as a cordial and medicine; but it does not intoxicate. If taken internally, it is said to be good for the stone as also for disorders of the breast. . . . Externally applied it is of great use in scorbutic pains, gouts, cramps, &c., but it must be put to the part affected only; it penetrates instantaneously into the blood, and is apt for a short time to create pain. It has also the property of spirits of wine to take out greasy spots in silks or woollens, but the remedy is worse than the disease, for it leaves an abominable odour. They say it is carried into India as a great rarity, and being prepared as a japan is the most beautiful and lasting of any that has yet been found."

Since that time Baku and its wonders have been frequently described, and the importance of the place with respect to the Central Asian question has been repeatedly pointed out by such travellers as Marsh, Valentine Baker, O'Donovan, and Arnold. Up to 1872 the extraction of the oil was a monopoly, but in the following year it was thrown open to the world, and hundreds of wells have since been sunk, mainly by the energy of Swedes and Russians. Geologically speaking, practically nothing is known about this extraordinary district, and even the engineers who bore for the oil and work the wells are ignorant of the conditions which affect the supply of petroleum. At the present time there must be at least five hundred wells and fountains situated close together on less than a thousand acres of ground, but the sources seem to be absolutely independent of each other. The supply is simply (to use Dominie Sampson's word) "prodigious"; and every year, as the borings get deeper, the fountains become more prolific. These borings are nothing like so deep as in America: not a single Baku well has yet approached a depth of 1000 feet. In 1883 two flowing wells each sent out nearly 30,000,000 gallons in less than a month from a depth of 700 feet. In America there are said to be 25,000 drilled petroleum wells, but a single Baku well has thrown up as much oil in a day as nearly the whole of the 25,000 in America put together. Mr. Marvin thus describes one of these "spouting" wells:—

"'In Pennsylvania that fountain would have made its owner's fortune; there's £5000 worth of oil flowing out of the well every day. [The actual value was at least £11,200 a day.] Here it has made the owner a bankrupt.' These words were addressed to me by an American petroleum engineer, as I stood alongside a well that had burst the previous morning, and out of which the oil was flying twice the height of the Great Geyser in Iceland, with a roar that could be heard several miles round. The fountain was a splendid spectacle—it was the largest ever known at Baku. . . . The derrick itself was 70 feet high, and the oil and sand, after bursting through the roof and sides, flowed fully three times higher, forming a greyish-black fountain, the column clearly defined on the southern side, but merging into a cloud of spray 30 yards broad on the other. . . . The diameter of the tube up which the oil was rushing was 10 inches. On issuing from this the fountain formed a clearly-defined stem, about 18 inches thick, and shot up to the top of the derrick, where, in striking against the beam, which was already worn half through by the friction, it got broadened out a little. Thence, continuing its course more than 200 feet high, it curled over and fell in a dense cloud to the ground on the north side, forming a sand-bank [from the amount of admixed sand], over which the olive-coloured oil ran in innumerable channels towards the lakes of petroleum that had been formed on the surrounding estates. . . . Standing on the top of the sand-shoal, we could see where the oil, after flowing through a score of channels from the ooze, formed in the distance, on lower ground, a whole series of oil lakes, some broad enough and deep enough to row a boat in. Beyond this, the oil could be seen flowing away in a broad channel towards the sea."

Flowing wells yielding from 40,000 to 160,000 gallons of oil daily are common in Baku, and the ordinary yield obtained by pumping is from 10,000 to 25,000 gallons daily; and many of these pumping wells have been worked for years without any diminution in the supply. A well belonging to Gospodin Kokereff had up to the

date of Mr. Marvin's book produced 60,000,000 gallons of oil, and the supply showed no sign of decreasing. The waste occasioned by "spouting" is at times enormous; millions of gallons of oil being lost from the want of any storage accommodation. Occasionally the neighbouring proprietors who happen to have reservoirs empty may thus obtain the oil at a nominal price. On one occasion 2,000,000 gallons were sold at about 7½*d.* per ton. When the Droojba fountain "spouted," the crude article, we are told, altogether lost its value for the moment.

"Fedoroff filled his reservoirs with 2,800,000 gallons of oil for 300 roubles, or £30. . . . Thousands of tons were burnt outside the district to get rid of it; thousands were led towards the Caspian; huge lakes of oil were formed near the well, and on one occasion the liquid suddenly flowed into a distant engine-house, and but for the promptness of the engineer in extinguishing his petroleum furnace the whole locality would have been ablaze. Houses were completely buried by the sand cast up by the oil; all efforts to stop the fountain on the part of Baku experts were fruitless."

After great exertions on the part of the well owners of the district, the fountain was eventually gagged, but not before 500,000 tons of oil had "spouted," equal to a loss at the current value of American petroleum of upwards of £1,000,000 sterling. But the record of the Droojba fountain was beaten in 1886, when a single well "spouted" as much as 11,000 tons of petroleum per diem; an amount equal to the aggregate daily yield of the 25,000 wells of America, the thousands of wells in Galicia, Roumania, and Burmah, and the shale oil distilleries of Scotland and New South Wales. As a result the market is now glutted, and the crude oil has been selling at times at the rate of fifty gallons for a penny!

We have not space to indicate all the many points of Mr. Marvin's interesting narrative, or to do justice to his account of the economic results which he thinks must inevitably follow from the prodigious source of wealth which Russia possesses in this wonderful district. It must be remembered that petroleum ton for ton is more potent than coal as a source of power. Hundreds of immense floating cisterns driven by petroleum furnaces are carrying this fuel across the Caspian and up the Volga, to be spread throughout Russia and Germany, and along the Baltic coasts. We learn from a recent Consular Report that pipe lines are being laid from Baku to Batoum: the Caspian and Black Sea Naphtha Conduit Company has now been formed, and the line is to be laid within the next four years. The conduit is to have a forked line on the Black Sea, reaching Batoum and Poti, and the capacity of the line is such as to admit of the daily passage of 1,200,000 gallons of naphtha. In a few years, therefore, this petroleum fuel will be scattered along the Mediterranean coasts and through Southern Europe. Possibly we may have it burning in our own Underground Railway before long. Indeed, as Mr. Marvin tells us, we shall surely see the Parsee back again at Baku, not to worship the Everlasting Fire, but for the purpose of buying lamp oil for the bazaars of India. What the effect of this intercourse will be on the future of India time will show. Meanwhile Russia is steadily making her way towards the gates of India, and Tchernayeff's road to Central Asia will be an accomplished fact before many years are past; and since the discovery of the new springs near the Mervi Kultuk Bay, the railway to Khiva will possess its own supply of fuel. A few days ago Mr. G. Curzon read an interesting paper to the Royal Geographical Society on the Transcaspian Railway, which must have opened many people's eyes to the development of Russia's power in Central Asia. In the meantime what are we doing with the sources of wealth in petroleum which we possess in Upper Burmah? Along the valley of the Irrawadi, and within 60 miles of the Rangoon-Prome railway, are enormous deposits of petroleum, probably as copious as those of America, if not so rich as those of Baku, and certainly capable of supplying the whole of India with light and fuel. Perhaps those capitalists who are so eager to rush into the ruby mines of Burmah might more profitably devote their wealth to exploiting the petroleum springs of that country, for it needs not the gift of prophecy to assert that Burmese petroleum in the long run will be certainly more precious than Burmese rubies.

We can heartily commend Mr. Marvin's book to all who are interested in the Central Asian question, for, as he says in the outset, petroleum is bound to become an important factor in that problem. Hannibal was said to have dissolved the Alps by vinegar. It is far more likely that petroleum will dissolve the sort of Chinese wall that our Governments are feebly setting up to keep the Russian trader and the *tchinovnik* out of India.

T. E. THORPE.

A TEXT-BOOK OF ELEMENTARY BIOLOGY.

A Text-book of Elementary Biology. By R. J. Harvey Gibson, M.A., F.R.S.E., Lecturer on Botany, University College, Liverpool. (London: Longmans, Green, and Co., 1889.)

THE above-named work is one of those which, as has been remarked in these pages (vol. xxxviii. p. 52), "the system of examining the whole world on a limited schedule . . . is bound to produce," and the essence of it is devoted to a consideration of those type-organisms which the examining body have set down for study. It contains 345 pages small octavo, and is divided into eight chapters, with an introduction. The first three chapters are devoted to generalities, and the last one to a "history of biology."

The author decries the "evils of the cram system," and proceeds at once to assert that "this must be my apology . . . for the introduction of so many speculations and explanations of casual relationship," while he claims for his treatise the special distinction that it deals "with the relationship of botany to zoology, and of both to the fundamental sciences of physics and chemistry." In fulfilling this determination the author gives, at the outset, a physico-chemical *résumé*. We regard the whole of this as out of place and superfluous, inasmuch as University students (for whom the book is written) will, if properly trained, have received the same information in a more tangible and authoritative form, at the hands of Professors of the special subjects. We strongly deprecate this growing tendency towards usurpation of the functions of others, especially when it

is seen that the sole object in view has here been that of incorporating tall-talk about that modern bogey "anabolism,"[1] with its antithesis, and about other heresies, which neither the pure physicist nor chemist would tolerate. Much that has been written of late under these and similar heads is now, by common consent, tabooed, as a mere garbling with ill-defined terms. As originally presented, it is, to say the least, over-reaching and often childish in its ambiguity: as diluted in the work before us, it bodes mischief whereby it becomes unendurable. It cannot be denied that for many a raw student such phantasies have an especial charm. In this work they are so interwoven with the more solid portions of the text as to bias and distort the intellect.

The "conditions of the environment necessary for the maintenance of life" and the "balance of Nature" are discussed and dismissed before the student is made familiar (in any but misleadingly general terms) with the constitution of the living organism. This we regard as a fatal error, revolting alike to common-sense and to established precedent, and we can only surmise that the adoption of so extraordinary a course has resulted from the influence of a wrong-headedness, at work upon the author's elementary training.

It will be seen that the author has, in our opinion, failed (and that, most probably, from faults not entirely his own) in the mode of treatment of his leading novelty. When first we realized the extent to which he had wandered into subjects not professionally his own, our suspicions were aroused as to whether he might not have erred proportionally within the limits of his recognized domain. The volume abounds in inaccuracies and misstatements. The methods of expression are frequently loose and contradictory: for example, on p. 293 we read that, in the frog, "the air is sucked into the interior of the body to the blood," and on p. 294 that the frog "forces the air into the lungs." On p. 145 a fair description is given of the bulb of the lily, whilst on p. 174 the same plant is "termed an annual." Things are too frequently declared to be "obviously," "naturally," or "clearly" so and so, and the author has yet to realize that with elementary students *nothing* must be taken for granted; while he has, on most points, grossly violated the inductive method (cf. the statements concerning the differentiation and structure of the nervous system, as successively presented on pp. 231, 232, 245).

The author's selection of types is unprecedentedly capricious. On the animal side, the Arthropod and Mollusk are omitted; while on the vegetable side, the description (p. 78) of an imaginary apical cell in *Spirogyra* implies complete ignorance of the type chosen for study. Nor must we disguise the fact that while the author tolerates those types now in vogue, he loses no opportunity of depreciating their educational value (pp. 219, 233, 264). We would remind him that these have served exceedingly well in the past, and that it is the manner of their manipulation by a certain class of teachers, rather than their constitution, which the unsuccessful student has cause to lament.

We deem detailed criticism superfluous, as there are no six pages in this book free from error, and, for a long succession, no two without inaccuracy. The following extracts will suffice. On pp. 265-66 we read that, in the frog, the alimentary system has become differentiated into "a buccal cavity, where the food is torn in pieces, or masticated; an œsophagus, or tube for the carriage of the triturated food (*sic*)," &c.; on p. 301 we are told that the occipital region of the skull, in the same animal, "consists of a floor and two side walls of bone (the basi- and two ex-occipitals)"; on p. 327, the oviduct of the frog is said to contain, when ready for oviposition, fertilized ova. Now as to the botanical side. *Penicillium* is selected as the type of Fungi, but the descriptions and figures apply throughout to *Eurotium*. In describing *Polytrichum*, his type of the Mosses, the author informs us (p. 103) that the leaves are "composed of almost undifferentiated parenchyma." A ridiculous attempt is made, three pages further on, to show homology between the archegonium of a moss and the conceptacle of *Fucus;* and the diagrammatic figure illustrative of the same can only bewilder the student, and mislead him as to the real structure and mode of development of the organs in question. Under the head of "cell-fusions," the statement is made (p. 149) that "the adjacent walls may have become completely broken down, as in tracheides": no well-tutored beginner needs to be reminded that this is in direct contradiction to the usually accepted definition of these structures. Finally, on p. 152, the *Duckweed* is referred to, and that in the most unfortunate manner conceivable, as a Dicotyledon. Misstatements such as these show the author to be ignorant of some of the most elementary truths dealt with in the most didactic handbooks in contemporary English literature. More the pity that the author should parade his indebtedness to the works of foreign writers.

One of the most conspicuous features of the book is the employment of a new nomenclature. The author was struck, early in his career, with the shortcomings of our conventional terminology; and, bolder than his fellows, he forthwith resolved to revolutionize the same. Order appears to dawn with the correlatives "Protozoa and Protophyta," "Metazoa and Metaphyta," but, when examined in detail, most of the author's substitutes are seen to be no better than their predecessors, and they consequently only complicate matters unnecessarily. We protest against this reckless use of new words. New and comprehensive terms are only to be accepted as landmarks in general advancement. Attempts to uproot a classical and time-honoured nomenclature, which are, like those before us, begotten only of youthful ambition, deserve no encouragement.

There would appear to be something seriously wrong in connection with the system which repeatedly produces books like that before us. Catering, as it does, for a prescribed curriculum, this one, the latest of its kind, will be eagerly sought by the examinees; and in their interests, if in none higher, it is time that something should be done to stem the tide. Similar complaints reach us from other sources, and it has been suggested in the pages of this journal (vol. xxxvii. p. 268) that the difficulty might be met by the establishment of "an Association to prevent the further publication of elementary works other than such as had been carefully revised and

[1] Defined by the author (pp. 336–40) as *consisting*, in the animal, of the processes of mastication, digestion, absorption, circulation, and assimilation.

approved of by a Publication Committee of the Association." No body of men have any such right to interfere with private enterprise; and the remedy proposed is wholly unscientific in principle, inasmuch as it would lay a sure foundation for systems of cliquism and popery, whose issue would be fatal to legitimate progress. Others there are who would seek the solution of the difficulty in an occasional substitution of the types chosen for teaching, and, in fact, such a change is already premeditated. This proposal cannot fail to meet with general approval, but it does not solve the problem; for, while no doubt it may, at the outset, insure to the examinee manuals of the better class, it will only prolong the evil day of publication of yet other inferior ones. The fault appears to us to lie not in systems, but in the individual. We are not yet rid of the old delusion that anybody can keep the children quiet. The infant-class is too often entrusted to the care of a novice, and with what results past systems of training have shown. "*Qu'est-ce qu'une grande vie?*" wrote de Vigny, with the rejoinder, "*Une pensée de la jeunesse exécutée par l'âge mûr.*" No one knows better than the English student that the production of an elementary text-book may constitute a leading feature in a great life. Such a work should be other than a medium in which the author airs his knowledge of fads and phantasies (most of which are sure to be wrong in the end) to the exclusion of fact and common-sense, and we hold its construction to be one of the most arduous of all possible tasks. It is, moreover, one for which a man is not fitted until ripened by long experience and meditation, and to none but the most experienced teacher would we intrust that awakening of the "thought of youth," which, if distorted at the outset, leads to certain failure. Here, to our thinking, lies the clue to the whole position. The matter is one for individual consideration. Upon the mind of the author of this work there has dawned the *pensée de la jeunesse*; in following it up, he has acted prematurely. Had he kept his ideas well in hand, others would have intersected them in the course of time, as his knowledge of (elementary) fact increased and as his experience ripened. He has done otherwise, and, in his eagerness for notoriety, has piled up, upon a flimsy foundation of words, a scant superstructure, the materials of which are ill-chosen and defective, and badly put together. G. B. H.

UNITED STATES GEOLOGICAL SURVEY.

Monographs of the United States Geological Survey. Vol. XII. *Geology and Mining Industry in Leadville, Colorado.* By S. F. Emmons. Pp. 747, with Atlas of 35 folio Plates. (Washington Government Printing Office, 1886.)

THE operations of the United States Geological Survey, under the charge of Mr. S. F. Emmons, in the Leadville mining district, have become known to some extent to many geologists in Europe by personal examination on the ground, and more particularly from the valuable summary of their results which appeared in the Report of the Director of the Survey a few years back. But even those most familiar with the thorough manner in which work is done in the office of Mr. Emmons's division at Denver can scarcely have been prepared for the mass of information which is presented to them in the present volume. Although the actual productive area of Leadville at the date of the survey was estimated at about one square mile, the study of a considerable part of the adjacent mountain districts was necessary in order to arrive at any general conclusions likely to be of value for practical purposes in regard to the mineral deposits; and therefore a district of about 15 to 20 miles of the western or Musquito Range of the Rocky Mountains has been surveyed, and mapped in very full detail on a scale of 2 inches to a mile. The interior parts of the mining region proper are treated more minutely on a scale of about 6 inches to a mile, and the geology and mine works on the three districts of Iron Hill, Carbonate Hill, and Fryer Hill are given on a scale of 1 to 1920.

From the maps and the sections which accompany each set, and which, in accordance with the excellent custom of the founder of the Geological Survey of Great Britain, the late Sir H. De la Beche, are constructed to the same scale both for heights and distances, it appears that the country described consists essentially of a series of ridges and furrows of sedimentary rocks resting upon an Archæan foundation forming the central mass of the Rocky Mountains. The most important member of this sedimentary series, the blue or metalliferous limestone, is a blue-gray dolomite of Lower Carboniferous age, which, at or near its contact with an overlying igneous sheet, known as the white or Leadville porphyry, is changed over considerable areas, but in an extremely irregular fashion, into a mass of clay and quartz charged with carbonate and sulphide of lead, chloride and bromide of silver, manganese and iron ores, which are obviously of secondary origin, and derived from the alteration of metallic sulphides. The upper surface of the deposit, being formed by the base of the porphyry sheet, is comparatively regular; but below, the boundary is exceedingly ill defined, the metalliferous mass—which in the principal mines resembles a brown garden-mould, mottled with dark-coloured patches in places—shading off into the unchanged limestone; it being, in fact, a pseudomorphous change of the latter rock by infiltration of metalliferous salts from the weathering of the overlying porphyry subsequently to the intrusion of the latter, and before the elevation of the Musquito Range—events which have been placed, as the results of detailed geological study, about the close of the Cretaceous period.

Since the year 1881, when the present Report was substantially completed, the ores of the Leadville district have changed very considerably, the more tractable carbonates and chlorides originally met with having given place to unchanged sulphides, with the result of complicating the processes of reduction. This circumstance detracts a little from the interest of the last section of the work, which is devoted to a very detailed description of the smelting processes as carried on at and near Leadville in 1880. This is due to the labour of M. A. Guyard, of the École des Mines, and for some time an assistant to Messrs. Johnson, Matthey, and Co. Unfortunately, he did not live to see the result of his work in print. Subsequently to the date of the Report, some of the larger smelting establishments were removed to localities closer to the fuel-supplies on the eastern side of the Rocky Mountains, and con-

siderable improvement has been made in the character of the smelting appliances. Some curious points are, however, brought out by M. Guyard's researches, especially as regards the speiss or arsenical regulus formed in the lead furnaces, which he finds collect by preference nickel and molybdenum from the ores, while cobalt, if present, is carried off by the lead, and may be found in the skimmings taken from the bath before casting it into pigs. Another point of interest is the occurrence of chlorine, bromine, and iodine in notable quantities in the furnace fume, which are due to the corresponding silver salts of the ores. The general chemical problems arising in the study of the ores and the containing rocks have been treated in a separate section by Mr. W. F. Hillebrand. The mass of analytical material contained in these sections is very large and important, but in some instances their value is diminished by an unnecessary striving after accuracy. Analyses reported to six decimal places seem to be scarcely suitable for practical purposes. The petrography of the district has been studied by Mr. Whitman Cross, his results being illustrated by heliotype prints of microscopic sections which though good in their way are decidedly inferior to the excellent drawings given by Prof. Irving and others in the earlier volumes of the same series before photographic illustrations were used. In his preface Mr. Emmons very handsomely acknowledges the "continuous and unwearied service" rendered by Mr. Ernest Jacob, an old student of the Royal School of Mines, who, we are sorry to say, has been compelled by reasons of health to retire from a service in which in a comparatively short time he was able to do a large amount of excellent work.

H. B.

OUR BOOK SHELF.

Practical Inorganic Chemistry: the Detection and Properties of some of the more Important Organic Compounds. By Samuel Rideal, D.Sc. (Lond.), F.I.C., F.C.S., F.G.S., Fellow of University College, London. (London: H. K. Lewis, 1889.)

THIS little book, as we learn from the preface, is designed to meet the wants of the medical student in his higher examinations, and we may say at once that it fulfils that purpose admirably. The syllabus of the University of London has been duly considered; all the substances therein mentioned are discussed at length, and their reactions fully given: and not only this, but Dr. Rideal has helpfully distinguished in each case the most characteristic reaction by an asterisk. With the aid of this book, and with ordinary application, the average candidate may fearlessly confront the Sphinx of Burlington Gardens (and *a fortiori* all minor Sphinxes), and attempt her riddles without risk of being torn in pieces.

All this is excellently done, and only one regret crosses the mind of the reviewer. Of course, a medical student is not intended to be an organic chemist, or, necessarily, a specialist of any kind; and it would be absurd to expect from him the knowledge of a specialist. But the training of a medical student is calculated to make him regard himself as the depositary of universal scientific knowledge—a belief which he frequently carries with him through life. Would it not be possible to convey the salutary notion that all this testing for organic substances has about as much relation to real practical organic chemistry as, say, the "use of the globes" to practical navigation?

Scottish Moors and Indian Jungles. By Captain J. T. Newall. (London; Hurst and Blackett, 1889.)

MANY years ago, in India, Captain Newall was unfortunate enough to suffer from an accident by which the spine was fractured. To some extent he recovered his health, but he has never since been able to walk or even to stand. Yet he has contrived—by an ingenious device which enables him to be carried about in a chair, in an easy position, by several men—to obtain a good deal of wholesome exercise in the open air. In the year 1880, in conjunction with his brother, he took the little shooting of Scaliscro, in Lewis; and in the first part of this volume he describes the incidents of sport and out-door life there during the seasons of the following four years. The second part of the volume is devoted to a record of more or less exciting sporting experiences in India at a time when the writer had full use of his limbs. The book may be read with pleasure not only by sportsmen but by others, for it is written in a bright and attractive style, and Captain Newall is always careful to give as vivid a picture as possible of the surroundings in the midst of which the incidents of his narrative took place. His account of autumn life in Lewis is particularly fresh and interesting. There are twelve very good illustrations from sketches by the author.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Inheritance of Acquired Characters.

WILL you allow me to say, in reference to Prof. Hartog's interesting letter, that there is no ground for regarding the word "Lamarckism" as a nick-name? There can be no desire to "nick" him or anyone else, should it appear that the views they advocate are to be classified with those of Lamarck. Lamarckism is as reputable a denomination as Darwinism, and no reasonable man can possibly regard with anything but respect and sympathy the attempt to bring forward solid evidence in support of Lamarck's fundamental assumption, viz. that acquired characters are transmitted by heredity.

It is not unusual for children to rest the head on the left forearm or hand when writing, and I doubt whether much value can be attached to the case described by Prof. Hartog. The kind of observation which his letter suggests is, however, likely to lead to results either for or against the hypothesis of transmission of acquired characters. An old friend of mine lost his right arm when a school-boy, and has ever since written with his left. He has a large family and grandchildren, but I have not heard of any of them showing a disposition to left-handedness.

E. RAY LANKESTER.

45 Grove End Road, N.W.

IT would be difficult to overrate the importance of the instance given by my friend Prof. Hartog in the last number of NATURE (p. 462) of the inheritance of a character acquired by habit; but the explanation which he offers of the non-inheritance of characters produced by mutilation, so far from being flat Lamarckism, appears to me to flavour of ultra-Darwinism, and the following hypothesis, resting more directly on mechanical principles, might be suggested. It is well known that many of the lower animals possess a remarkable power of regenerating lost parts. The garden snail offers a familiar instance: if the eyes be snipped off from their tentacles, they are in a short time reproduced, usually with a structure as perfect as that of their predecessors, as may be proved by a histological examination of thin slices. This power appears to be possessed to an unlimited extent, for one of my former pupils, Mr. Trevor Evans, performed the experiment twenty times in succession on the same snail, and the last eye was as perfect as the first; he then relinquished the research, being persuaded that the power of regeneration would

only terminate with the life of the unfortunate subject. This power of growing afresh so complex and specialized an organ as an eye is certainly at first sight not a little astonishing, but it appears to be capable of a very simple explanation: the cells terminating the cut stump of the tentacle are the ancestors of those which were removed; a fresh series of descendants are derived from them, similarly related to the ancestral cells as their predecessors which they replace; the first generation of descendants become in turn ancestors to a second generation, similarly related to them as were the second tier of extirpated cells; and this process of descent being repeated, the completed organ will at length be rebuilt. The possibility of this arises from the fact that in the snail the embryological course of development is capable of being repeated by the adult structure. In higher organisms this possibility does not as a rule exist, and mutilation is not followed by regeneration; but even in their case the ancestral cells remain, and when the embryological development is repeated their representatives in the embryo are present to give rise to descendants of the normal type in the normal fashion. It follows from this view, which leaves pangenesis out of account, that mutilations cannot possibly be inherited, and this for the reason that the cells forming the organism at each stage of its development must be regarded as the ancestors of those of the next stage; thus finally we are brought round to something which looks very like Weismannism.

W. J. SOLLAS.

Trinity College, Dublin, March 15.

P.S.—The foregoing completely accounts for the non-inheritance so often referred to of the character produced by circumcision. In the case of a snail it might be presumed that circumcision could not produce any persistent result; in the human subject what is remarkable is not the reappearance of the prepuce in the descendant, but that no regrowth beyond healing takes place in the subject.

MR. MARCUS M. HARTOG's letter of March 6 inserted in last week's number (p. 462), is a very valuable contribution to the growing evidence that acquired characters may be inherited. I have long held the view that such is often the case, and that I have myself observed several instances of the, at least I may say, apparent fact.

Many years ago there was a very fine male of the *Capra megaceros* in the gardens of the Zoological Society. To restrain this animal from jumping over the fence of the inclosure in which he was confined, a long and heavy chain was attached to a collar round his neck. He was constantly in the habit of taking this chain up by his horns and moving it from one side to another over his back; in doing this he threw his head very much back, his horns being placed in a line with the back: the habit had become quite chronic with him, and was very tiresome to look at. I was very much astonished to observe that his offspring inherited the habit; and although it was not necessary to attach a chain to their necks, I have often seen a young male throwing his horns over his back and shifting from side to side an imaginary chain. The action was exactly the same as that of his ancestor. The case of the kid of this goat appears to me to be parallel to that of child and parent given by Mr. Hartog. I think at the time I made this observation I informed the late Mr. Darwin of the fact by letter, and he did not accuse me of "flat Lamarckism."

J. JENNER-WEIR.

Chirbury, Beckenham, Kent, March 16.

Hertz's Equations in the Field of a Rectilinear Vibrator.

IN Dr. Oliver Lodge's valuable communication to NATURE of the 21st ult. (p. 402), giving Hertz's equations for the field of a rectilinear vibrator, may I suggest the following very slight change, in order to bring the formulæ into complete accord with those of the Maxwellian theory.

Hertz has, with $A^2 = \mu K$,

$$A\frac{dL}{dt} = \frac{dZ}{dy} - \frac{dY}{dz}, \quad A\frac{dM}{dt} = \frac{dX}{dz} - \frac{dZ}{dx}, \quad A\frac{dN}{dt} = \frac{dY}{dx} - \frac{dX}{dy},$$

$$A\frac{dX}{dt} = \frac{dM}{dz} - \frac{dN}{dy}, \text{ \&c.},$$

whence he obtains the suitable solutions—

$$X = -\frac{d^2\Pi}{dxdz}, \quad Y = -\frac{d^2\Pi}{dydz}, \quad Z = \nabla^2\Pi - \frac{d^2\Pi}{dz^2},$$

$$L = A\frac{d^2\Pi}{dydt}, \quad M = -A\frac{d^2\Pi}{dzdt}, \quad N = 0,$$

where Π satisfies the equation—

$$\nabla^2\Pi = A^2\frac{d^2\Pi}{dt^2}.$$

The corresponding Maxwellian equations would be—

$$\frac{dL}{dt} = \frac{dZ}{dy} - \frac{dY}{dz}, \quad \frac{dM}{dt} = \frac{dX}{dz} - \frac{dZ}{dx}, \quad \frac{dN}{dt} = \frac{dY}{dz} - \frac{dX}{dy},$$

$$A^2\frac{dX}{dt} = \frac{dM}{dz} - \frac{dN}{dy}, \text{ \&c.},$$

with the solutions, X, Y, Z, as before, and

$$L = A^2\frac{d^2\Pi}{dydt}, \quad M = -A^2\frac{d^2\Pi}{dzdt}, \quad N = 0.$$

The more general solutions of the field equations would be—

$$X = \frac{d}{dy}\left(\mu\frac{d}{dx} - \lambda\frac{d}{dy}\right)\Pi + \frac{d}{dz}\left(\nu\frac{d}{dx} - \lambda\frac{d}{dz}\right)\Pi;$$

$$L = A^2\frac{d}{dt}\left(\nu\frac{d}{dy} - \mu\frac{d}{dz}\right)\Pi;$$

with corresponding expressions, *mutatis mutandis*, for Y, Z, M, N; where λ, μ, ν, are arbitrary constants, coinciding with Hertz's results when $\lambda = 0$, $\mu = 0$, $\nu = -1$.

H. W. WATSON.

Alternative Path Leyden Jar Experiments.

IN your issue of Feb. 14 (p. 380) there is an "Electrical Note" which is very misleading. You will perhaps allow me to say, therefore, that Mr. Acheson's photographs show no evidence of oscillation whatever; that his experiments are aimed at practical questions connected with lightning protectors, and confessedly were not made in such a way as to have any theoretical importance; that in so far as Mr. Acheson thinks he is expounding a new theory by calling self-induction "extra currents" he is, in my opinion, mistaken; and finally, that the author of the note, in speaking about "the errors due to charging which vitiated Prof. Lodge's early experiments," is talking about something which has no existence. OLIVER J. LODGE.

The Celluloid Slide-Rule.

CELLULOID has been applied to so many purposes, that one is never surprised to see one or other of its many valuable properties turned to account in some new way. A slide-rule is now made, in which the surfaces on which the divisions are engraved consist of thin sheets or veneers of dead white celluloid. The divisions are beautifully sharp and distinct. If these veneers do not come unfastened, and the rule does not lose its dead white surface with use and exposure, this new application of celluloid will be found a most valuable one. The rule examined is one almost identical in pattern with the well-known Gravet instrument, and, if one may judge by the scale, the accuracy of the divisions, and the smoothness of the motions, it is made by the same machinery. The differences are mere differences of detail. Mahogany takes the place of boxwood. The cursor runs on an improved form of slide. Chisel-edges, instead of cross-lines, on the cursor are used to transfer readings. There is only one opening at the back, so tangents cannot be read without reversing the slide. The millimetre scales at the two edges are replaced by scales of inches. It is a pity that one of the scales of millimetres has not been left. The agents are John Davis and Son, of Derby and 118 Newgate Street, and the price is rather less than that at which the ordinary Gravet can be obtained in this country.

C. V. BOYS.

The Philosophical Transactions.

YOUR correspondent "S." seems to be unaware that what he asks for has been already done. The abridgment of the Philosophical Transactions, which was brought down to the year 1800 by Charles Hutton, George Shaw, and Richard Pearson, was continued in octavo form, by order of the President and Council of the Royal Society, under the title of "Abstracts of the Papers printed in the Philosophical Transactions of the Royal Society of London." This series extended to six volumes, bringing the abridgment down to the year 1854. At the seventh volume the title was changed to "Proceedings of the Royal Society of London," a publication which still exists, and which contains abstracts of all the papers in the Philosophical Transactions and a good deal besides. H. R.

Japanese "Koji."

IN the current number of NATURE (p. 469) is a note upon the preparation of Japanese *koji*, taken from the American Consular Reports.

In NATURE, vol. xxiv. p. 468, will be found a report of a paper read before the British Association on this subject, supplemented in the following number (p. 509) by a letter from the author giving more details. The whole subject was exhaustively treated in a paper on "The Diastase of Koji," read before the Royal Society in 1881, and also in a memoir on "*Sake*-brewing," published by the University of Tokio in the same year. Further, an abstract of the latter appeared in the *Chemical News*, November 11, 1881, p. 230.

I shall feel obliged if you will insert this letter, as most people, on reading the note in NATURE, would be led to think that Prof. Georgeson had made observations which were previously unknown. This is not the case. R. W. ATKINSON.

44 Loudoun Square, Cardiff, March 18.

THE TOTAL SOLAR ECLIPSE OF JANUARY 1.

BY the kindness of Mr. Todd we are enabled to give a drawing summarizing in a general way the phenomena observed during the last total eclipse. A comparison of this with the records at the two preceding sun-spot minima indicates very clearly that we have now very definite information concerning the corona of the sun as observed at the minimum period of sun-spots.

Everything written relating to the form of the corona in 1878 is now strengthened by still another critical observation at the succeeding minimum. It remains to be seen whether the same marked absence of bright lines in the coronal spectrum has been noted.

Here is an extract from what I wrote in 1878:—

"The utter disappearance of the large bright red corona of former years in favour of a smaller and white one in this year of minimum, struck everybody. Indeed it is a remarkable thing that after all our past study of eclipses, this last one should have exhibited phenomena the least anticipated. It isolates the matter that gives us a continuous spectrum from the other known gaseous constituents. The present eclipse has accomplished, if nothing else, the excellent result of intensifying our knowledge concerning the running down of the solar energy. With the reduction of the number of spots or prominences for the last four years, the terrestrial magnetism has been less energetic than it has been for the preceding forty years, while at both ends of this period we have had famines in India and China.

"As the sun is the great prime mover of earth, and as every cloud, every air current depends upon it, its present quiet condition is worthy of the most minute study.

"The absence of lines from the corona spectrum shows a great reduction in the temperature of the sun, and such a marked change in the sun should produce a corresponding change on the earth, so that the associated terrestrial phenomena should be carefully observed. Hence I regard this eclipse as the most important that has been observed for many years, as it throws much-

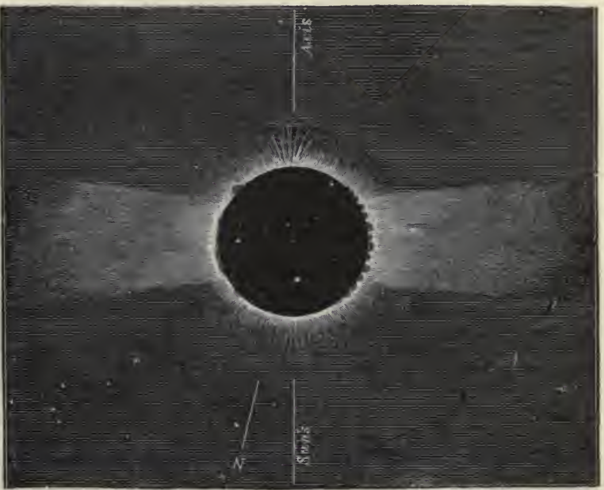


FIG. 1.—The equatorial extension and Polar tracery observed at the minimum of 1867.

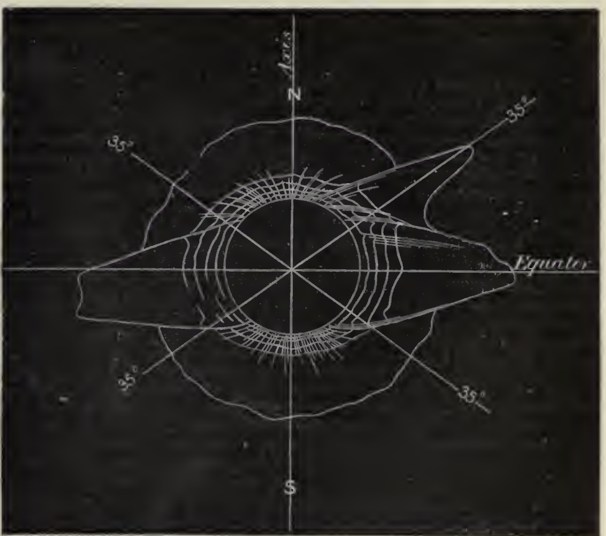


FIG. 2.—Tracing of the results obtained by the cameras in 1878, showing inner portion of equatorial extension, and how the surfaces of it cut the concentric atmosphere in lat. 35° N. and S., or thereabouts.

needed light on many points hitherto obscured in doubt."

The similarity of the coronas of 1867 and 1878 was one of the points relied upon when I subsequently discussed (see "Chemistry of the Sun") the possible meteoric origin of many solar phenomena, and pointed out that if this were so, there must be an equatorial ring to produce them. The recent development of the meteoric theory suggests that

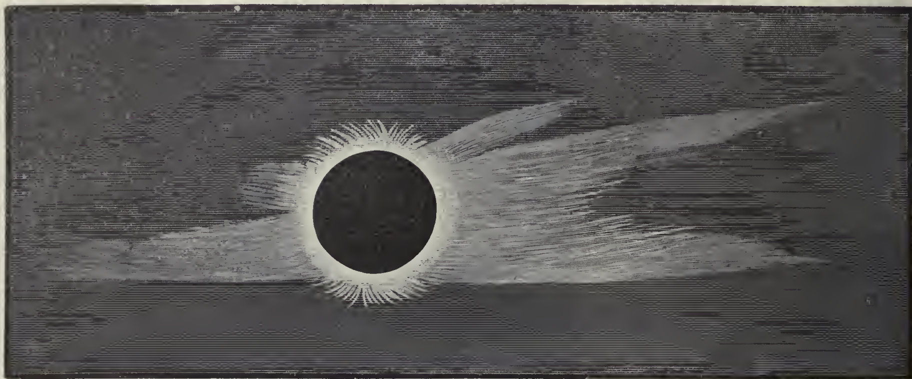


FIG. 3.—Phenomena observed during the total solar eclipse of January 1, 1889 (see NATURE, March 7, p. 436).

among the most important observations to be made at future eclipses will be a direct comparison of the spectrum of the corona with the low temperature spectrum of meteoritic dust. It may be that some of the photographs taken during the last eclipse may give us some information on this point, but so far nothing is known.

J. NORMAN LOCKYER.

ON THE GRADUAL RISING OF THE LAND IN SWEDEN.

ALTHOUGH we find in a work by Urban Hjärne, printed in 1702, some remarks on the level of the sea in the Baltic, and the old shore-lines of the island of Gothland, the honour of having for the first time raised this question seriously, and of having subjected it to scientific investigation, belongs to Emanuel Swedenborg, who, in 1719, published a work entitled "On Proofs from Sweden of the Level of the Sea, and the Past World's Strong Flood and Ebb." From the condition of the rocks in West Gothia; from fossils in horizontal chalk and marl beds; from shell-banks situated high above the actual level of the sea; from skeletons of whales and wrecks of ships found far inland; from the structure of the sand-hills and from the round stones found therein; from erratic blocks (or, as Swedenborg calls them, "stones that are spread all over the world"); from giants' bowls; from the shore-lines on Halle and Hunneberg (mountains in West Gothia); from the species of fishes existing in lakes at great elevations; and, finally, from the many proofs of the fall of the sea-level in the Baltic,—from all this, Swedenborg drew the conclusion that the former level of the sea in Sweden was some 400 feet above the present one. These changes he attributed in part to an alteration in the velocity of the earth's rotation and the period of rotation of the moon, whereby the water at the Pole is forced towards the equator. He also assumed that as the Baltic lies at a higher level than the Western Sea, the water therein gradually decreases.

Swedenborg's work, which suggested many points respecting the history of the world that have been hotly debated to the present day, was at first not understood, and continued to be almost ignored by the scientific world. But it was the immediate cause of analogous researches begun by Prof. Anders Celsius, in 1724, along the coast of the Baltic, the results of which he embodied in a paper published, in 1743, in the Proceedings of the Royal Academy of Sciences, entitled "Remarks on the Decrease of the Water in the Baltic, as well as in the Western Sea."

Setting aside all other periods dealt with by Swedenborg, Celsius devotes himself exclusively to the changes in the sea-level which have taken place in historical times. He adduces several examples, tending to show that harbours and roadsteads on our shores have become shallow, and that rocks have gradually risen above sea-level; that ample depth for seine-fishing existed where there is now a shallow; that the appellations "island" and "holm" are frequently used on the coast for uplands surrounded by lowlands, the name "sound" for bights or dry land; that flat rocks at the level of the sea, formerly valuable on account of seals gathering therein, have become valueless by having risen too high above the sea; and that anchors and wrecks have been found in inland peat-bogs. He further compares measurements of the rising of the land extending over 168 years, and comes to the conclusion that at Gefle (on the Baltic), during 100 years, the land rose from 41 to $47\frac{1}{2}$ inches, and, on the opposite side of the Bothnia Sea, from 41 to 50 inches, being an average of 45 inches. Celsius also proves a similar rising on the west coast, and from these facts he infers that the entire Scandinavian peninsula is gradually rising. Celsius further calculates the area of the land thus won from the sea since the days of Pytheas, and finally, for the benefit and instruction of coming generations, has a rock at Löfgrundet, off Gefle, carefully marked, this being the first scientific water-marking of the rising of the land in Sweden. As regards the fall of the sea, Celsius is of opinion that it is partly due to the transformation of water into earth through plants, and partly to the flowing

of water into "abysses" in the earth. It may be pointed out that the former theory could hardly be considered absurd at a time when most physicists believed that water could be transformed into earth; and as to the latter, we are still discussing the possibility of water being "absorbed by underlying strata."

Celsius found an ardent supporter in Linnæus, who, returning to Swedenborg's theory, connected the decrease of water with the presence of mussel-shells and *petrificata marina* in strata now situated high above the sea. Linnæus wholly rejected the theory that their presence was due to the Flood. He also held that "endless ages" must have elapsed since the earth began to be inhabited by plants and animals.

The views of these two great naturalists were at first accepted and defended by a number of distinguished scholars. But from 1755 they met with the warmest opposition, especially from the Bishop Johan Browallius, who, from the theological point of view, in a celebrated and learned work refuted and condemned the theories of Swedenborg, Celsius, and Linnæus. When some years later Colonel Carl Fredrik Nordenskiöld presented a paper to the Academy further elucidating the question, four years elapsed before it was published, and even then a "note of apology" for its appearance was appended.

But with the opening of the present century a new controversy arose. The theory of the structure and history of the earth had, during the preceding era, not only become developed into a special science, but students had already, by different opinions on certain fundamental scientific points, become divided into two schools, the followers of which, under a fierce, but to science beneficial, contest, each attempted to prove their views by searching old and collecting new records. Hardly had this strife begun when it became evident of what importance the old question of the rising of the land or the fall of the sea would be for the determination of the matters in dispute. One of the founders of the plutonic school, John Playfair, in 1802 advanced the theory of a connection between the rising of the land in Sweden and the volcanic forces in the interior of the earth, and some years later this view was further developed by the most ardent and gifted champion of plutonism, Leopold von Buch, who himself had had the opportunity during a journey in Scandinavia, 1805–6, by personal observations and by intercourse with Swedish men of science, of learning that at all events most of the observations on which the assumption of a change in the sea-level of the Baltic were founded had been carried out with the greatest care and conscientiousness.

Some of the opposite views, on the contrary, were revived, after a careful study of the literature appertaining thereto, by K. E. A. von Hoff, in an excellent work printed in 1822, entitled "Geschichte der durch Ueberlieferung nachgewiesenen natürlichen Veränderungen der Erdoberfläche"; but it should be added that the views defended in this work were retracted, twelve years later, after a careful discussion of the researches respecting the land-rising carried out in 1820–21 by Herr N. Bruncrona, Director of the Swedish Pilot Service, and the observations of Lieut.-Colonel C. P. Hällström, recorded in the Proceedings of the Academy in 1822. Hoff then acknowledged that the theory of the rising of the land formed one of the most important and instructive parts of modern geological science. Hällström, by the way, demonstrated that a considerable rising of the land takes place on the east as well as the west coast of Sweden, that the rising differs in magnitude in various localities, and that no rising exists on the coasts of Halland and Scania in the extreme south.

At this stage of the discussion, the closing word was spoken by the famous English geologist Lyell. He had at first doubted the assertion of the rising of the Scandinavian peninsula, but having, in the summer of 1834, paid a visit to Sweden for the purpose of investigating the question, having examined many of the statements bearing upon it, and having obtained valuable information from Berzelius and others, he published, in the following year, a paper in the Transactions of the Philosophical Society, entitled "On the Proofs of the Gradual Rising of the Land in Certain Parts of Sweden." In this paper Lyell accepts unhesitatingly the views held by the Swedish men of science. He especially points out the theoretically important and instructive fact (already, however, demonstrated by Hällström) that the rising varies much in different localities, and even that in some places in Southern Sweden no rise has taken place within historical times. Lyell's paper remained the final word upon this question for a long time, during which no opposition was raised to the fundamental principle. Efforts, however, were made by fresh measurements on the Swedish coast, to obtain fuller material for research, valuable contributions being rendered by Sven Nilson, P. A. Siljeström, A. Erdmann, Sven Lovén, J. G. Forshammar, G. Lindström, Lord Selkirk, A. G. Nathorst, and others. From these new researches it became evident that it was often difficult to establish harmony between observations made in places very close to each other, a circumstance which indicated that the phenomenon was far more complicated than at first supposed, and which again threw some doubt upon the matter, and caused fresh opposition. Even Lyell himself, in the eleventh edition of his "Principles of Geology," published in 1872, speaks with far less confidence of the land rising; and in a newly published important work the celebrated Austrian Professor, Suess, wholly denies the rising as well as local changes of the shore-lines. The old view of Urban Hjärne is adopted, viz. that the Baltic may be considered a lake, in which the height of the water chiefly depends upon the proportion between the water conveyed into it and the water lost by evaporation and outflow. Space does not permit me to enter further into the ingenious arguments of the eminent Austrian geologist. Hardly had his work left the press before the views advanced were refuted by Dr. Holmström in an elaborate paper, published in the Transactions of the Royal Swedish Academy.

Holmström's researches were begun in 1867 at the instigation of Prof. Otto Torell, so that his paper is founded upon studies extending over a period of thirty years. During that time Dr. Holmström, partly at the expense of the Pilot Service, visited and re-measured most of the old water-marks along the coast of Sweden. New ones have also been cut in the rocks, and for the guidance of future researches the old as well as the new markings have been carefully drawn and described.

The following important synopsis is the result of Dr. Holmström's prolonged studies.

The twenty-four hydrographical rock-marks along the *west* coast of Sweden show that the land in that part has risen about 0·5 centimetre during the last half-century. The rising is incontestable, but varies in different localities, amounting, for instance, at Nordkoster, to almost nothing, but at the Väderö to more than 1 metre in the century.

This result of the west coast researches is very important, inasmuch as no doubt can be entertained that the average water-level there corresponds with that of the North Sea, and that the rising of the land thus demonstrated cannot possibly be caused by a gradual fall of the water in the Baltic.

The two rock-marks on the south coast also indicate a rising during recent years, but as the time between the registration and the cutting of the marks is hardly twenty years, this proof cannot be accepted with certainty.

On the east coast of Sweden, as far as Stockholm, some twenty water-marks have been examined, and here, too, a rising is perceptible in most places, but at Säfvö

and Rödskär, and some other places, a *sinking* has taken place during the last twenty years, amounting to 0·7 centimetre a year. This sinking is, however, at all events at Säfvö, as far as can be ascertained from personal observation, of a quite local nature. At Calmar, on the other hand, as already shown by Dr. Siljeström, no change whatever has taken place since the beginning of the century.

From Stockholm northwards there are about thirty water-marks, and here, too, the rising predominates, although it varies often in localities very near each other; but a careful discussion of the observations seems to have established that the rising has been on the decrease during the last century. During the last period it amounted at Stockholm to 0·5 centimetre a year; at Celsius's old water-mark, at Löfgrundet, off Gefle, to 0·9 centimetre; at Chydenius's mark at Ratan to nearly 1 centimetre; at Bergö, on the Finnish side of the Gulf, to 1 centimetre; and at the mark cut by Augustin Ehrensvärd, August 21, 1754, on a rock at Hangö (Finland), to 0·6 centimetre. Therefore, a considerable rising, varying from 0·5 to 1·1 metre per century has taken place in this part of the Baltic.

The above facts, gathered by Dr. Holmström, form the last contribution to our practical knowledge of the old water decrease problem, and many decades must now elapse before fresh data can be obtained for further observations upon these changes in the earth's crust—changes which appear to us to take a long time, but which, geologically reckoned, are very rapid.

If the problem of the land-rising is taken in the same extent in which it was first raised by Swedenborg, it may be divided in two problems, certainly related, but widely separated, and both of pre-eminent importance to the geological history of the earth, viz. (1) the question of the changes of level to which the sea or the hydrosphere of the earth has been subjected in historical times; (2) the question of changes in the level of the sea during the immense length of the geological ages.

As regards the first, it must be considered fully demonstrated, to the student free from preconceived opinions, (1) that in several places along the coast of Sweden, during the lapse of a few generations, a considerable rising of the land has taken place, not only in the Baltic, but also on the west coast; (2) that this rising varies in different localities, and is in some places entirely wanting.

These facts cannot possibly be explained, as some students have attempted, simply by maintaining that the sea-level of the Baltic lies above that of the seas beyond, and that a gradual levelling takes place; as in that case no rising could take place on the west coast. If we, therefore, as is always advisable in natural researches, take the actual observations as basis for our theories, we are compelled to assume that in some places on the Swedish coast a gradual upheaval of the solid fundamental rock really takes place, although some portion of the apparent rising may no doubt be ascribed to a decrease of the water in the Baltic. That such a decrease does occur is probable, but when the student of science refers to it, he should bear in mind that it is only an hypothesis, as yet far from being proved.

Several circumstances seem certainly to speak against the trustworthiness of the observations founded on the water-marks. Prof. P. A. Gadd, for instance, remarks that often there have been found, close to a mark indicating a rising of the land from 3 to 4 feet in a century, trees 300 years old standing close by the water's edge—*i.e.* in places which, when the tree was only a shoot, would have been several feet under water, and this argument has been repeated without contradiction by Lyell, Erdmann, Suess, and others. But it is forgotten, when this is used as an argument against the land-rising theory, that the tree during a period comprising centuries may have sunk through its own weight and through the washing away by rain of the earth at its roots, peculiarities which cannot be unknown to the horticulturist who has planted trees on earthy eminences in parks. Indeed it is self-evident that among the thousands of trees by our shores there must be some which could strike their roots and thrive just in such a spot. Viking mounds, memorial stones, and buildings by the shore might be subjected to similar sinking, the water-marks carved in the solid crystalline rock only being trustworthy as evidence in questions about secular changes in the earth's strata.

Another argument against these observations on the coasts of Scandinavia is that these changes of level, if they do really take place, cannot possibly be confined to this country alone, but must be observable in other parts of the world. But any certain counterpart to the land-rising in Sweden is not known anywhere, not even on the North Sea coast of Norway, nor along the Atlantic border, where foundations of monuments dating from the time of Cæsar still remain intact. The weight of the latter argument is, however, greatly reduced when we bear in mind that observations such as those made in Sweden could not possibly be effected on a coast exposed to the ocean, where, in consequence of the tide, the level of the sea varies diurnally and alters with the direction and force of the wind—changes which so far exceed the land-rising here referred to that they would entirely obscure it. Besides, as regards old buildings, only those built carefully of granite and resting on the solid rock can be taken into account in this discussion. It is, moreover, possible that such an elevation of the land as that in Scandinavia takes place only in districts where the rocks consist of granite or crystalline schist. The Mediterranean being a sea with a mouth far more narrow than that of the Baltic, is unsuitable for the settlement of such questions. On the west coast of Europe, again, the tide is so great that similar observations there would be very difficult. Neither are the sandy shores of Holland and Germany suited for such observations, and the east coast of America has hardly a history long enough for such researches. This applies in a still greater degree to the west coast of America and the coast of Australasia. The volcanic shores of the Pacific Ocean are but little suited for the observation of such changes of the level of the sea. That no rising of the land along the coasts of the oceans has been observed is therefore capable of explanation.

But even accepting the theory that a slow secular disturbance of the level of the shore-line does take place in some localities we are obliged to confess that the geologists at present cannot advance any certain proof of a general change in the level of the sea having occurred in historical times. Any general decrease or increase of the volume of water in the sea in historical times has not been proved. The case is, however, different when it is a question of changes during ages not measured by those of man, but by those of the earth, *i.e.* by the measure of time, which no doubt bears the same proportion to our years and centuries as our terrestrial measures bear to astronomical distances. For in all parts of the globe, as well at the equator as near the Poles, we find rocks which incontestably have, during former geological ages, been formed below the sea, although now lying above it. There is not a shade of doubt about this. And one of the reasons why the geologist has with such great interest studied the question of land-rising in Sweden is that he hopes to derive from the small changes that take place before our eyes an insight into the causes of the great ones. With regret, however, we must confess that our success has yet been very slight. There does not yet exist any satisfactory theory of the origin of the beds of chalk and clay, a thousand feet in thickness, containing fossils of unmistakable sea animals, which are found high

up on the slopes of the Alps and far in the interior of high continents.

The marine deposits which are encountered all over the globe high above the present sea-level are stratigraphically of two very different kinds: viz. marine layers which have been greatly disturbed from their original horizontal position, upheaved and thrust up by the side rocks; and marine layers which, lying perfectly horizontally, form the upper strata of the high plateaus, or of the table-mountains.

That the former, after having been deposited as mud below the level of the sea, and afterwards hardened into more or less solid rock, have been dislocated from their original position by mechanical forces, and raised high above the level at which they were formed, pressed together, and thrown above each other,—about this all geologists agree. Formerly the opinion prevailed that the volcanic forces in the interior of the earth had accomplished all this, but we may assume that most geologists are now inclined to seek the cause of the changes indicated in *side pressure*, dependent upon various causes—a theory advocated by me twenty years ago, but then little heeded.

However, this explanation is no longer applicable to marine layers which have not been disturbed in the least degree from their original horizontal position, although they at present form high plateaus several thousand feet in depth and several thousand square kilometres in area. Such formations are, as is generally known, found in all parts of the globe, and from all geological ages. On the west coast of Norway, where no such rising of the land in historical time as in Sweden has been observed, one finds in many places, particularly in the north, terraces or ledges which run perfectly horizontally, irrespective of the geological structure of the coast, for miles along the shore. Since attention was first drawn to these terraces by Urban Hjärne in Sweden, and by Keilhau and Bravais in Norway, they have been the subject of careful study, and of a literature as voluminous as that relating to the land-rising question. No geologist will now venture to deny that we have here before us old shore-lines, indicating that the sea even during the very last geological epoch, but still long before, very long before, historical times, stood far above its present level; whilst the horizontal position observed everywhere, apart from purely local exceptions, appears to contradict the view that this is due to local upheavals. Similar formations are also found in other parts of the world, as for instance at the Cape and the southern part of South America, proving that even there great changes in the level of the sea have taken place since the beginning or middle of the last geological epoch.

Of layers from the Tertiary period we have, in consequence of the erosion during the Glacial age, only traces in Scandinavia; but further north, in Spitzbergen, we find Tertiary strata thousands of feet in thickness. Near the west coast they are much disturbed, but further inland they form almost horizontal strata of sand and clay, here and there containing small coal-seams and schists, rich in splendid fossil remains, bearing witness not only to a magnificent vegetation having once existed in these parts, now ice-covered, but to the fact that the sea at Spitzbergen when they began to form hardly stood higher than at present. When therefore Prof. Nathorst, during one of his expeditions to Spitzbergen, on the highest plateaus of one of these high but horizontal Tertiary beds, found a mighty layer of marine fossils, we obtained proof that during the Tertiary period, geologically speaking so near us, the level of the sea had varied to the extent of several thousand feet. Even here the perfect horizontal position of the strata from Advent Bay by the Ice Fjord across the Storfjord to Franz Joseph's Land, excludes the possibility of these Tertiary marine beds being raised to their present level by volcanic forces. And if we proceed from the Tertiary beds of the Arctic regions to those on which Paris rests, or to those of the United States or of Patagonia, we encounter everywhere proof that the level of the sea has changed many times during the Tertiary period. Analogous observations may be made about the strata from the Trias, the Jura, and the Chalk periods in different parts of the globe. Again, the geologist finds that the level of the sea for some reason or another during those epochs has changed by many thousands of feet, in most places without its being possible to connect this change with the oft-adduced reaction on the earth's crust of the supposed red-hot interior; and the same applies also to layers from the Palæozoic period, from the period during which the rocks of West Gothia, referred to by Swedenborg, were formed.

Independent of all observations on the land-rising in Sweden, and independent of all theories, the fact remains that since the earth became an abode for animals and plants, the level of the sea has changed many times.

But we must confess that up to the present no acceptable theory explaining the *cause* of these changes has been proposed. Some have re-adopted Swedenborg's ancient idea that a change in the rotation of the earth caused a change in the form of the hydrosphere; others have discussed the great influence exercised by heavy mountains on the water-level of the adjacent seas, calculating that under favourable conditions this may amount to a great deal, *i.e.* that the sea-level on coasts that are engirdled by great mountain ridges is several hundred metres above the main level of the ocean in the same latitude; others, again, have sought the explanation in the hypothetical, and to those who are familiar with the Arctic regions wholly arbitrary, assumption, that huge masses of ice are periodically heaped up at one or another of the Poles, and by their attraction cause notable changes in the sea-level; and, finally, some maintain that the rising of the level depends on dust and *débris* being either blown or washed into the sea, and that the sinking depends on water being absorbed by strata in the interior. But to the student reckoning with figures, and who bases his researches on actual observations and not on assumptions, none of these causes explains fully and satisfactorily the great and probably simultaneous changes of the sea-level. To my mind the simplest explanation, and nearest at hand, has never been duly considered.

No doubt this neglect in some degree springs from the still prevailing belief in the quantitative unchangeableness of the heavenly bodies, which with the Aristotelian philosophy has penetrated the intellect of natural philosophers. Kepler, on account of the sun being obscured during three consecutive days in April 1547, most probably by cosmic dust, opposed this principle, declaring expressly, *Cœli materiam esse alterabilem;* but the belief in the old dogma was so little shaken thereby that the scientific ban went forth from more than one quarter against Chladni when he attempted to demonstrate that cosmic matter does really fall upon the earth. Now Chladni's doctrine is everywhere accepted, but even at the present day few geologists will assign to the cosmic matter that falls on the earth an important influence in the formation of new strata. Only a few quantitative studies of the phenomenon itself, and an unprejudiced estimation of the length of the geological epochs, are needed to convince anyone how unjustifiable this is. To my mind it seems fully proved that solid matter, as well as gaseous, and fluid at a temperature above 0°, is daily in great quantities brought to the earth, and that through this fall, and by the masses of *débris* carried by rivers and wind into the sea, the latter must during geological ages have become filled, and its level raised in a manner which would be totally opposed to actual facts, if there were not other causes to counteract it.

Such a cause might be found in the circumstance that,

just as fresh matter is hourly brought to the earth by meteors, it steadily loses during its orbit in the solar system some of its gaseous constituents; and the near maintenance of a *status quo* during ages partly depends upon the circumstance that gain and loss balance each other, and partly also upon the gain and loss, during the record of mankind, being so infinitesimally small in proportion to the gaseous and fluid matter surrounding the lithosphere of the earth. But during the geological ages even this "little" may be appreciable: long periods may have passed when the gain has been in excess, which has caused a rise in the volume of the sea; whilst at others the loss has predominated, whereby a gradual fall of the sea-level has taken place.

That the atmosphere sometimes decreases through loss in space it is of course impossible to demonstrate by direct observation; but as we at present know fairly well the forces acting upon a gas or dust molecule in the outer strata of the atmosphere, it seems that we may, from a theoretical point of view, be able to obtain an answer to the question raised here. However, we must, to avoid erroneous and hasty conclusions, here take into consideration so many factors difficult to estimate that the definite answer probably for a long time will give rise to much controversy.

Therefore, although the problem of the rising of the land on the coasts of Sweden and Finland may now be said to have, in the main, been decided, the old question about the diminishing of the sea-level, and that just in the general form in which it was presented by Swedenborg and Linnæus, still remains an unsolved riddle of immense importance to the history of our earth.

A. E. NORDENSKIÖLD.

VARIABLE STARS AND THE CONSTITUTION OF THE SUN.

VARIABLE STARS.—The theory of variable stars set forth by Dr. Brester in a recent essay[1] somewhat resembles those suggested by Zöllner in 1865,[2] and by Dr. Lohse in 1877.[3] Briefly, Zöllner's theory regards variability as being due to the formation of scoriæ on the photospheres of the stars and their subsequent dispersion by the heat due to chemical combinations which take place in virtue of reduced temperature. Dr. Lohse substitutes absorption by the cooled atmospheres of the stars for the reduction of light by scoriæ as in Zöllner's theory, although both agree as to the cause of the removal of the light-obstructing agencies. The theory suggested by Dr. Brester is a little more ambitious than either of these, and, to give a translation of his own statement, "All the phenomena that variable stars present to us are the varied effects of one cause—the intermittent chemical combination at the cool external layers of that which had previously been dissociated by heat" (p. 1). All the explanations are based on the assumption that the stars are stratified spherical agglomerations of gaseous matter, the different layers having different compositions according to their distances from the centre.

Most stars are too hot to allow of the formation of compounds as we know them, but the stars most subject to variability (the red stars) are sufficiently cool, in their outer layers at least, for the formation of such compounds as hydrocarbons. It is in the obstruction of light by these compound vapours that Dr. Brester finds his explanation of variability. He aptly compares the cooling of a star to the running down of a clock-spring, and the intermittent chemical combination to the escapement which regulates it. These changes pass unnoticed in the hottest stars, because the periods elapsing between the coolings is very great, and the combinations formed exert no very great absorbing influence; but in the cooler stars only small changes of temperature are necessary, and the periods are correspondingly short.

Again, although the temperature may be low enough for a combination to take place, the combining substances may be so diluted by other matter that the combination is impossible, just as a mixture of oxygen and hydrogen will not explode if admixed with more than 7½ volumes of air (Bunsen). This condition Dr. Brester describes as a state of *surdissociation*. This state does not last long, because, as the combining molecules get nearer the centre, they get more concentrated, while the substances which prevent their union diminish in proportion. When the combination does take place, there is an "eruption of heat" and the clouds in the outer cool layers are dispersed, the brightness of the star consequently increasing.

So much for the general theory, which Dr. Brester believes to be competent to explain every description of variable star, even such diverse ones as Algol and β Lyræ. He rejects the eclipse explanation of the Algol type on the ground that it is impossible to conceive such large obscure bodies to travel at such an enormous rate, and that it is in contradiction to the recent work of Chandler and Sawyer, showing irregularities in the periods, especially in the case of U Ophiuchi.

Secondary maxima, such as occur in β Lyræ, he believes to be due to double combinations: the first substances which combine by the fall of temperature do not produce sufficient heat to reproduce the first maximum, whereas the next combination does, and these taking place alternately, the β Lyræ type receives explanation. Irregularities in the variability are, according to this theory, due to disturbances brought about by very rapid rotations.

"New stars" are believed by Dr. Brester to be produced by the sudden dispersion of the obscuring clouds which formerly surrounded the star, by heat due to a new chemical combination. On this supposition they must be at a very low temperature before they burst out. It is not easy to understand, however, how any such action as this could raise a star from the ninth to the second magnitude, as was the case with T Coronæ. The spectroscopic difficulty is a still greater one. How the spectrum of a new star just before its disappearance could, on Dr. Brester's view, be like that of a planetary nebula, is not easy to explain. For the present it seems more consistent with the facts to regard "new stars" as being due to the clashing together of two meteor-swarms in space.

In this theory Dr. Brester has attempted too much. Most astronomers are agreed that more than one cause of variability is at work, and it is certainly too much to expect one theory to explain all the various types. Dr. Brester does not seem to be aware that Algol is one of the hottest stars in the heavens, and that a recent photograph by Prof. Pickering shows the spectrum to be the same at maximum as at minimum. If one hot star be variable, why not all? Again, if variability is only manifest in the cooler stars, why does not every cool star give indications of variability? Further, the theory assumes that all variable stars are cooling, whereas Mr. Lockyer's recent work has shown that those of the Mira type are increasing in temperature. Dr. Brester's only objection to Mr. Lockyer's theory of variability is its limited application, but it was not set forth as being universally applicable. If Dr. Brester's theory had been limited to the variables of Group VI. (Vogel's Class III*b*), it would be more reasonable, but even then it would not be easy to understand why all the stars of the group do not exhibit variability.

The Sun.—The second part of the essay attempts to explain the various phenomena presented to us by the

[1] "Essai d'une Théorie du Soleil et des Étoiles variables," par A. Brester, D.Sc. (Delft: J. Waltman, 1889).
[2] "Photometrische Untersuchungen." p. 252.
[3] *Monatsber. der Akad. der Wissenschaften*, p. 826.

sun. The first great departure from prevailing opinions is the view that the sun is in a tranquil state, and is in no way subject to the violent storms which are commonly believed to disturb it. The stratified character of the solar atmosphere is set forth as evidence of this tranquil state. Dr. Brester believes that the apparent motions of the protuberances are not real, but simply indicate the displacement of the luminous condition brought about by chemical combinations in tranquil matter. This he believes to be quite consistent with the observed displacements of the prominence lines, and we see no reason to differ with him.

The next important divergence from prevailing ideas is the suggestion that pores, spots, and faculæ are all at exactly the same temperature because they are at the same level. The experiments of Spoerer and Langley have shown that the spots emit less heat than the other parts of the photosphere; but Dr. Brester states that this is not due to a difference in temperature, but to a difference of emissive power. He believes that spots are formed by the vaporization of the photospheric matter in the regions where they are formed, the luminosity being reduced, whilst the temperature remains the same. On this supposition, the photosphere bears the same thermal relation to the spot that ice does to the water formed as it melts.

The forms of the spots he believes to be due to the increase of pressure caused by the volatilization of the photospheric matter, the conical form being due to the fact that the nearer the centre the greater the resistance to the expansion.

That faculæ should precede spots Dr. Brester states to be essential to his hypothesis. They indicate the places where increased condensations are taking place prior to the "eruptions of heat" which will produce spots.

For an explanation of the periodical phenomena, and the increased angular velocities of spots near the equator, Dr. Brester assumes that, while the photospheric surface which we see is spherical, the different layers of the atmosphere must be ellipsoidal, owing to the rotation of the sun. He admits that this state of things is not easy to explain, but states that it is sufficient to know that the fact exists. This being taken for granted, the varying periods of rotation in different latitudes is not difficult to explain. For since the photospheric matter is formed by the condensation of the vapours of the ellipsoidal layers, the particles in equatorial regions have to descend a greater distance towards the centre than those in the same layers near the Polar regions; and since the linear velocity remains the same during the descent, the angular velocity is increased, and is increased more at the equator than away from it. Since the spots lie in the photosphere, they thus indicate an increased angular velocity in equatorial regions. It will be seen that this explanation is much akin to that suggested by Mr. Lockyer,[1] differing from it mainly in giving the whole photosphere the additional velocity, whereas, according to Mr. Lockyer's view, only the spot-forming material partakes of the added angular velocity.

The relation of spot spectra to the eleven-yearly period observed by Mr. Lockyer is also partially explained. According to Dr. Brester's view of the solar economy, the photosphere must have a special composition in each latitude, and since the latitudes of the spots vary with the period, the spectra would also vary with the period. The exact nature of the change—namely, from lines of iron and other known substances at minimum to unknown lines at maximum—is not explained.

The similarity of the spot zones on both sides of the equator, according to Dr. Brester's view, is due to the fact that the same atmospheric layer meets the photosphere in equal latitudes on opposite sides of the equator. The slight differences which do exist are regarded as simply the effect of chance, since an "eruption of heat" may either produce one large spot or several smaller ones.

[1] "Chemistry of the Sun," p. 424.

Dr. Brester also attempts to explain the cause of the eleven-yearly period, but his explanation is difficult to follow. Broadly speaking, his idea is that during eleven years the integrated effects of the various chemical combinations which have taken place are such as to very nearly restore the conditions which had existed at the commencement of the period. Slight differences would be produced each time, so that after a long interval, well-marked differences might be expected.

Although the theory explains many of the phenomena observed, an explanation of many more is still required. Thus, although it is not difficult to understand the absence of spots at the equator, the cooler layers there being at the greatest distance from the photosphere, we should be led to expect the greatest number of spots in polar regions, where the atmosphere in the neighbourhood of the photosphere is coolest, and where, therefore, chemical combinations would be most likely to take place. The question of the corona is reserved for a future essay, but Dr. Brester is confident that it will present no great difficulty. He also hopes to satisfactorily explain the phenomena of comets' tails, the zodiacal light, and the variability of Jupiter and his satellites.

In conclusion, Dr. Brester states that his theory, so far from being at variance with the laws of chemistry and physics, really strengthens them, and that it is not discordant with the observed facts. At the same time he admits that the difficulty of comprehending it in detail will prove a great drawback to its acceptance.

A. FOWLER.

THE RABBIT PEST.

MR. W. RODIER, of Tambua, Cobar, New South Wales, has forwarded to this Society a printed sheet, containing, as it appears to me, by far the best suggestion yet made for the extermination of rabbits—a subject to which my attention has been repeatedly called by various correspondents in the Australian colonies, where, as is well known, the damage done by these animals is enormous. Mr. Rodier states that his plan has been in operation at his station in New South Wales for about eight months "with the utmost possible success," and has cleared the country of rabbits. It is a very simple plan. Ferrets and nets are used in the usual way to capture the rabbits, but while all the females taken are destroyed, the males are turned out again uninjured.

The results of this mode of operation are that the male rabbits, as soon as they begin to predominate in numbers, persecute the females with their attentions, and prevent them from breeding. They also kill the young rabbits that happen to be born; and even, as Mr. Rodier asserts, when they largely predominate in numbers, "worry the remaining does to death."

This is all strictly in accordance with what we know takes place under similar circumstances in the case of other animals, so that we can readily believe it to be likely to happen.

The ordinary mode of trapping, as Mr. Rodier points out, is more likely to increase the number of rabbits than to diminish them. For reasons which he clearly explains, more buck rabbits are always killed by the trappers than does. Thus the does predominate in numbers, and, a few bucks being sufficient for a large number of does, are perpetually breeding and increasing the stock.

The plan advocated by Mr. Rodier is so simple and easy that I cannot doubt it will be widely followed when known. No disease that might otherwise cause injury is introduced, no other noxious animal is proposed to be

imported, but advantage is taken of the well-known natural laws which regulate the increase of life to effect in this instance a salutary decrease.

P. L. SCLATER.

Zoological Society of London,
3, Hanover Square, W., March 18, 1889.

NOTES.

THE number of candidates for the Fellowship of the Royal Society this year is seventy-one, being about a dozen above the average number.

THE contributions hitherto paid or promised in this country towards the intended statue of G. S. Ohm amount to £95 14s. 6d. from ninety-four subscribers. It is proposed that the subscription-list shall be closed at the end of the present month, and we are desired by the Committee to ask intending subscribers to send their contributions as early as convenient to the Treasurer, Dr. Hugo Müller, F.R.S., 13 Park Square, N.W.

A COMMITTEE was formed some time ago at Limoges for the purpose of securing the erection of a statue of the great French physicist and chemist, Gay-Lussac. The preliminary arrangements have now been made, and the task of preparing the statue has been intrusted to M. Millet, who expects to be able to exhibit it at the *Salon* next year.

WE have to congratulate the Fishery Board for Scotland upon the acquirement of the services of Dr. J. Beard, who for some years has been working on the Continent. Dr. Beard's researches into the development of fishes take rank among the leading recent contributions to the subject, and they augur well for the future work of the Board. We are pleased to see that the members of this body are now issuing their scientific Bulletins independently of their official Reports.

MR. W. E. HOYLE, late of the *Challenger* Office, has been appointed to the Curatorship of the Manchester Museum in the Owens College. The Museum Committee is fortunate in having secured the services of so competent a man.

A VALUABLE collection of photographs, representing Alpine and Caucasian scenery, taken by the late Mr. W. F. Donkin, is now being exhibited at the Gainsborough Gallery, 25 Old Bond Street.

WE regret to have to record the death, at a very early age, of Mr. J. Reynolds Vaizey, a promising member of the younger school of botanists at Cambridge. His best-known contributions to botanical science are his papers in the first volume of the *Annals of Botany*, on "The Transpiration of the Sporophore of the Musci," and on "The Absorption of Water, and its Relation to the Constitution of the Cell-wall in Mosses." Mr. Vaizey was subject to epileptic fits, and, during one of these, received fatal injuries from falling into the fire.

THE oldest botanical journal of Germany, *Flora*, hitherto published at Regensburg, under the auspices of the Bavarian Botanical Society, appears now, in its seventy-second year, under a new form, issued at Marburg, under the editorship of Prof. K. Goebel. In addition to original papers, it will contain a *résumé* of botanical work during the year in different departments. The first part under the new *régime*, for March, contains important papers by Goebel, Pfeffer, Ludwig, J. Müller, and others.

IN the March number of the *Kew Bulletin* there are papers on fibre industry at the Bahamas, hardy species of Eucalyptus, yam bean, West African rubbers, Phylloxera in Asia Minor, botanical station at Lagos, and Chiga bread.

AT the meeting of the Scientific Committee of the Royal Horticultural Society on March 12, corroborative information was received from Mr. Plowright, of Lynn, regarding the occurrence, described at the previous meeting, of boughs of various trees being broken off by the extraordinary growth of crystals of rime upon them. As no snow had fallen during the period, it was impossible to attribute the results to such a cause. There had been excessive fog before January 7; the rime forming upon the telephone wires was so great that they were broken down. The ice was deposited unilaterally like flat sheets of glass, 1½ to 2 inches in width on the south side. On the 8th was a thaw. The result of the frost was that a birch had a branch amounting to one-third of the tree broken off; the smaller branches particularly suffered. The elms were most injured, branches of all sizes being broken off, even large arms, one measuring 5 feet 6 inches in circumference and 1 foot 10 inches in diameter. To such an extent was the roadway covered with *débris*, that the market carts were greatly impeded. Oaks, willows, and poplars also suffered; but ashes and Scotch firs escaped. Mr. Plowright noticed that fracture without falling was a distinct feature of rime-injuries to trees, excepting to willows and poplars, the vast majority of whose branches fell to the ground.

THE Society established some months ago under the name of the Gesellschaft Urania has already issued the sixth monthly number of its excellent magazine, *Himmel und Erde*, which is edited by Dr. W. M. Meyer. The main object of the Society is to popularize the accurate knowledge of scientific matters, by practical demonstrations at the head-quarters in Berlin, and through the medium of its magazine. Astronomy, as it always did and always must, leads the way in this attempt to interest the general public in science. Since it is expounded by such authorities as Prof. Schiaparelli, whose illustrated article on Mars runs through the first three numbers, it is evident that the Society does not mean to sacrifice genuineness for the sake of popularity. Dr. Scheiner, of the Potsdam Observatory, contributed an admirable article to the January number, on the principles of spectrum analysis and their application to celestial physics. Astronomical articles have also been contributed by Prof. Foerster, Prof. Seeliger, F. K. Ginzel, and others. Other subjects, however, have not been neglected. A clear exposition of the proofs of subsidence and elevation afforded by the pillars of the Temple of Jupiter Serapis was given by Dr. Brauns in the November number. The January number also contains an excellent article on the aurora, by Dr. Weinstein. The magazine is got up in an attractive style, and is admirably illustrated. The Society has purchased a 13-inch refractor, several microscopes, and other apparatus for the demonstrations; and it is their intention to have models constructed to illustrate eclipses and other phenomena.

THE Educational Society of Japan has, says the *Japan Weekly Mail*, published and circulated a little volume containing its programme, organization, and a list of its members. It is worthy of note that, in the artistic device on the cover, women are conspicuous, and the fact that women are carrying on their studies side by side with men would seem to indicate that the Society is desirous of recognizing the equality of the sexes. No fewer than 5000 members have joined the ranks of the Association, and a kind of committee or parliament is elected by these for purposes of discussion and deliberation, consisting of 200 deliberative members, seven councillors, seven sectional presidents, and one president.

AN interesting step has been taken in Japan by the organization of a branch of the Anthropological Society of Tokio, to be called the "Maine Club," after the late Sir Henry S. Maine, having for its object the investigation of the ancient laws and

customs of Japan, and all matters connected with the development and progress of civilization in that country. Its members are chiefly members of the parent Society, but membership is not confined to these. It is proposed that there shall be monthly meetings at which papers will be read, and discussions held. Well-known scholars are to be invited to attend the meetings, and contribute to the discussions. Essays or speeches which are considered to be of sufficient importance will be printed either separately or in the volume of Transactions of the Anthropological Society.

AT the meeting of the French Meteorological Society on February 5, M. Lemoine communicated the information he had collected upon the rising of the Upper Rhone from the 3rd to the 5th of October last. The maximum rise was on the 5th at Lyons, where it reached 14 feet. The cause of the rise was a severe thunderstorm which broke out at 2 a.m. on October 2, with incessant rain over a large area lasting until the evening of the 5th.

THE Pilot Chart of the North Atlantic Ocean, issued by the Washington Hydrographic Office, for March, shows that gales were experienced in the western part of the Atlantic during the first three weeks of February. The most noteworthy was one which originated on the 10th, just north of the Bahamas; on the 12th its influence was felt, noticeably from Newfoundland to the West Indies, and from the American coast to the 45th meridian. More fog was experienced than is usual during February. The southward movement of ice was very backward, no field ice or bergs having been reported till February 6, in lat. 45° 35′ N., and long. 48° W. The chart contains brief rules for the use of oil at sea.

THE "Administration Report" of the Surveyor-General of Ceylon, for the year 1887, contains meteorological summaries for sixteen observatories, and monthly rainfall values for seventy-one stations. The mean temperature of the year was below the average at almost every station, and a comparison of the records since 1882 proves that there has been a fall of temperature throughout the island up to the present time. A map is given, showing the mean annual rainfall of the various districts, and a table showing the monthly means during different periods. The returns from Ceylon have been regularly published since 1869.

Two shocks of earthquake occurred at Bologna on March 9, but no damage was done. A severe shock was noticed at Aquila on March 10.

THE vapour-density of aluminium methide, $[Al(CH_3)_3]n$, has formed the subject of an important series of experiments by Dr. Quincke, of Göttingen, with the view of further elucidating the question of the valency of aluminium. A few months ago, Messrs. Louïse and Roux published an account of their experiments upon the same substance, from which they conclude that molecules of the constitution $Al_2(CH_3)_6$ are capable of existence. This result was in direct contradiction to the earlier observations of Messrs. Buckton and Odling, who showed that, even at the boiling-point itself, 130° C., the density was considerably lower than that required for this double formula; from the boiling-point upwards the density gradually diminished, until, at the temperature at which this organo-metallic body unfortunately commences to decompose, it had almost reached that required for $Al(CH_3)_3$. Hence a revision of the subject has been undertaken by Dr. Quincke at the request of Prof. Victor Meyer. The aluminium methide employed was a very pure specimen, of constant boiling-point, and solidifying, on cooling, in the form of magnificent tabular crystals. Analyses gave practically theoretical numbers. The vapour-density was determined in a Victor Meyer apparatus in an atmosphere of hydrogen. Of course, the all-important point to decide was the nature of molecular grouping just above the boiling-point, and, if such molecules were found to exist at all, to ascertain whether the value required for $Al_2(CH_3)_6$ remained constant for a sufficient interval of temperature. The experiments were therefore performed at the temperature of boiling xylene (140°), only 10° higher than the boiling-point of the methide. The mean value for the density, obtained from ten consecutive determinations, was 3·92; $Al_2(CH_3)_6$ requires 4·98, and $Al(CH_3)_3$ corresponds to 2·49. Hence it can no longer be doubted that molecules of the double formula are incapable of existence. Aluminium methide must therefore be represented by $Al(CH_3)_3$. Readers of NATURE will remember that only a fortnight ago an account was given in these columns (p. 447) of an analogous series of experiments by M. Alphonse Combes, upon a new organic aluminium compound, $Al(C_5H_7O_2)_3$, aluminium acetyl acetonate. It is supremely satisfactory that in this case the density, at a temperature only 45° above the boiling-point, was found to actually correspond precisely with that required by the triad formula, precluding again the possibility of the existence of molecules of the type Al_2R_6. Taking the mass of evidence now before us, it may fairly be granted that the stable molecules of aluminium salts are constructed upon the type AlR_3; and aluminium in this respect thus completely resembles iron, chromium, indium, and gallium.

A REPORT of Mr. D. Hooper, the Government Quinologist in the Nilgiris, says that efflorescent salts occur in nearly every district of India. When the salt is alkaline in its nature, the surface of the soil on which it collects is known as dhobies' earth, which has for ages been used in various manufactures, and for washing and dyeing. A large quantity of the efflorescence of Northern India, which is sold in the bazaars as *sajji mati*, is a mixture of salts, where the sulphate and chloride of sodium preponderate over the carbonate. Dhobies' earth consists principally of sodium carbonate and sand; the other ingredients are organic matters and sodium chloride, with traces of sulphate of clay, oxide of iron, and lime.

IN the Report on the Blue-book of 1887, the Colonial Secretary for Ceylon says that the operations of the Survey Department during the year were most important and varied. Not only was the ordinary work of surveying Crown land for sale carried on, but a large amount of surveying was undertaken in connection with irrigation schemes and forest reservation. The minor triangulation of the island was continued, and thirty new stations were established. The great triangulation of the northern part of the island, for the purpose of connecting the Ceylon system with the Madras coast series of the great Trigonometrical Survey of India, was completed, as described in these columns some time ago. Considerable advance was also made with the surveys of roads, streams, and lakes in all the provinces; and a vast amount of other work, including surveys of the coast-line, surveys of villages for forest reservation purposes, &c., was satisfactorily completed.

A RECENT American Consular Report contains a long account of the industrial school at Reichenberg, which was founded by the Imperial and Royal Ministry of Public Instruction as a technical school of the middle grade, with the object of educating young men for important industrial and manufacturing positions. The institution is divided into a high school and a workmaster's school, and each of these is subdivided into branches for architecture, mechanical arts, and chemistry. In the high school a very high standard of general education is maintained, and the students are prepared, by systematic courses of lectures, for practical work. The workmasters' school is open to persons who have already worked in architecture, or in some mechanical art, or in chemistry. Besides these

schools there is one at night for the benefit of persons who are compelled to work during the day, the instruction given relating chiefly to drawing and modelling. This institution is now being taken as a model for other schools which are in course of establishment by the Russian Government.

In the report, to the Foreign Office, of Sir A. Nicholson on the agriculture of Hungary during the past year, it is said that in November last a new agricultural school was opened in the Torontáler country, and at the end of the year there existed an academy of forestry at Chemnitz, a veterinary school at Buda-Pesth, an agricultural school in Hungarian Altenburg, four other agricultural academies in various parts of the country, and six schools. There were eight institutions for giving instruction in viticulture, three of which were in receipt of a fixed subvention. A proposal made to establish a high school for forestry and agriculture at Buda-Pesth has, for the present, fallen through. A Director-General has been appointed for all the agricultural schools; and a staff of travelling teachers for certain branches of agriculture has been formed. This staff, however, needs organization similar to that in Austria and France. Up to the present the members of the staff seem to have devoted themselves chiefly to instructing in agriculture and viticulture. Some years ago a body of engineers was formed under the control of the Ministry for Agriculture, with the object of assisting proprietors and farmers in irrigation, drainage, and other similar works, and of watching over water rights and fisheries. At present the number of these engineers is too small for the needs of the country.

A new stalactite cave has lately been discovered by accident near the little village of Erlach, in Lower Austria. It is about 60 metres long, and contains a shaft filled with water. The floor and walls are covered with stalactites and stalagmites picturesquely grouped, varying in colour from brown and green to white.

At the annual general meeting of the Linnean Society of New South Wales, on January 30, Prof. W. J. Stephens, in his Presidential Address, took occasion to discuss Dr. Waagen's ideas as to the supposed Upper Carboniferous glacial period, and its assumed bearing upon the correlation of various Upper Palæozoic and Mesozoic formations in India, South Africa, and Australia. Prof. Stephens's object was to show that no general glaciation had ever taken place in the temperate regions of the southern hemisphere, and that evidences of local glaciation, as of glaciers, floating ice, whether of icebergs or river ice-rafts, cannot be regarded as of any value in the determination of the question of the relative ages of members of geographically distant formations.

The new number of the *Mineralogical Magazine* contains, besides Mr. L. Fletcher's address on the renaissance of British mineralogy, the following papers: a mangano-magnesian magnetite, by Prof. A. H. Chester; on the zeolites of Rye Water, Ayrshire, and the minerals of the Treshinish Islands, by Prof. M. F. Heddle; elaterite, a mineral-tar in Old Red Sandstone, Ross-shire, by Mr. W. Morrison; analyses of various mineral substances, by Prof. I. Macadam; on the supposed fall of a meteoric stone at Chartres, Eure-et-Loir, France, in September 1810, by Mr. L. Fletcher; calcites from the neighbourhood of Egremont, Cumberland, by Mr. H. A. Miers; on the large porphyritic crystals of felspar in certain basalts of the Isle of Mull, by Mr. T. H. Holland. Mr. Allan Dick has a paper in which he describes a new form of microscope.

The United States Geological Survey has published the fifth volume of the series entitled "Mineral Resources of the United States," by David T. Hay, Chief of Division of Mining Statistics and Technology. This volume contains a summary statement of the mineral substances produced in the United States in the year 1887, and chapters showing the features of the principal mining industries during that period.

Several valuable Bulletins of the United States Geological Survey (Nos. 40–47) have just been issued. One of them (No. 44) consists of a bibliography of North American geology for 1886.

Vol. iii., Part 1, of the *Folk-Lore Journal* contains, amongst other interesting matter, papers on African folk-lore, by Edward Clodd; on Wexford folk-lore; and on superstitions of Scottish fishermen, by E. E. Guthrie. This volume also contains the Annual Report of the Council of the Folk-Lore Society, in which it is said that many new members have joined the Society. Amongst the losses by death during the past year have been Sir H. S. Maine, Mr. J. C. Morison, Mr. R. Proctor, and Mr. Gifford Palgrave.

The Geneva Society of Physics and Natural History has issued the first part of vol. xxx. of its Memoirs. It opens with an address by the President, M. Victor Fatio, giving an account of the work done by the members of the Society in the course of the year 1887.

Mr. John Anderson has compiled an interesting "History of the Belfast Library and Society for promoting Knowledge, commonly known as the Linen Hall Library" (Belfast: M'Caw, Stevenson, and Orr). It has been published in connection with the hundredth anniversary of the institution. Some valuable old maps of Belfast are reproduced in the volume.

Part 5 of Cassell's excellent "New Popular Educator" has been issued. Like the preceding parts, it is carefully illustrated.

Dr. A. B. Griffiths's "Treatise on Manures" will be published, in a few days, by Messrs. Whittaker and Co., of Paternoster Square.

Messrs. William Wesley and Son have issued No. 94 of their "Natural History and Scientific Book Circular." It contains a catalogue of works relating to the various branches of physical science.

The additions to the Zoological Society's Gardens during the past week include a Fruit Bat (*Cynonycteris*, sp. inc.) from India, presented by Mr. W. Jamrach; two Pine Grosbeaks (*Pinicola enucleator*), two Waxwings (*Ampelis garrulus*), a Nightingale (*Daulias luscinia*), British, presented by Mr. J. Young; a Great Eagle Owl (*Bubo maximus*), European, presented by Mrs. Morant; a Cactus Conure (*Conurus cactorum*) from Brazil, presented by Mr. W. H. St. Quintin; a Common Moorhen (*Gallinula chloropus*), British, presented by Mr. G. Hayward; a Rhesus Monkey (*Macacus rhesus* ♀) from India, two Nicobar Pigeons (*Calœnas nicobarica*) from the Indian Archipelago, deposited; a Buffon's Touracou (*Corythaix buffoni*) from West Africa, purchased.

OUR ASTRONOMICAL COLUMN.

Rowland's Photographic Map of the Normal Solar Spectrum.—Prof. Rowland has been engaged since the publication in 1886 of his first photograph of the spectrum, in endeavouring to perfect it, and has now completed a new map, which he considers much superior to the former. The entire work has been gone over again; a new dividing-engine has been constructed, and several concave gratings ruled by its means, some of which give especially fine definition. Much greater attention has been paid to the photographic manipulation, and the prints are, it is said, much finer and more perfect than those of the first series, which, however admirable as representations of the spectrum, as photographic works of art

left much to be desired. The present issue comprises ten plates, each 3 feet by 2 feet, and includes the entire spectrum, from λ 6950 in the B group up to the extreme limit of the ultra-violet. Of these, all but the first plate, which contains the most refrangible portion of the ultra-violet, are now ready, though plate *i*, λ 6050 to λ 6550, is not quite satisfactory, and may have to be replaced. The original negatives for this new edition show as distinct doubles, not only E, but even finer lines, such as those at λ 5276·1 and 5914·3, but it has not been found possible to exhibit all these as divided on the map.

THE CLINTON CATALOGUE.—We learn that the ownership of this most important work, which embraces the positions of over 35,000 stars, is in dispute; Mr. Borst, who with his sisters performed the principal part of the reductions, and prepared the manuscript of the completed Catalogue, having laid claim to it, whilst Dr. C. H. F. Peters, the Director of the Litchfield Observatory, of Hamilton College, Clinton, New York, at which Observatory Mr. Borst was an assistant, has instituted an action against him in order to regain possession of the Catalogue. The manuscript in question contains 3572 pages, 900 of which are of nearly double folio size, and shows more than 7,000,000 of figures.

SATURN'S RING.—Prof. Krueger telegraphs from Kiel that Dr. Terby, of Louvain, announces the appearance of "a white region on Saturn's ring, opposite the shadow of the globe" (*Dun Echt Circular*, No. 169).

THE O'GYALLA OBSERVATORY.—The tenth volume of the observations of Dr. N. de Konkoly's private observatory has just been published, in which he gives the results of the work done during the year 1887. The principal observations made were those of the sun, and of meteor-showers. In the former department, 137 drawings were made, showing 187 groups of spots. The relative spot number for the year was deduced as R = 10·35. The meteor-showers watched were those of the Aquarids principally, on July 25–27, the Perseids on August 8–12, and the Leonids on November 17. Some experiments with hydroxylamine as a developer, and the trial of a couple of object-glass prisms, with two or three observations of comets, complete the volume.

MOON-CULMINATING STARS, 1889.—M. Lœwy has prepared a comprehensive Catalogue of moon-culminating stars for 1889, which has recently been issued by the Bureau des Longitudes. Stars down to the seventh magnitude, 366 in number, have been taken and arranged in the order of their right ascensions. The R.A. of each star is given for every ten days during its period of visibility, the mean declination and sec D and tan D being also given. The Catalogue forms a valuable supplement to the tables already published in the *Connaissance des Temps* and the *Nautical Almanac*.

The ephemerides of eight Polar stars for 1889, all within 9° of the Pole, and none below the sixth magnitude, are given in the first part of the memoir.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *MARCH* 24–30.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 24

Sun rises, 5h. 54m.; souths, 12h. 6m. 16·5s.; sets, 18h. 18m.: right asc. on meridian, 0h. 14·9m.; decl. 1° 37′ N. Sidereal Time at Sunset, 6h. 28m.

Moon (at Last Quarter on March 24, 7h.) rises, 2h. 3m.; souths, 6h. 7m.; sets, 10h. 9m.: right asc. on meridian, 18h. 14·6m.; decl. 22° 8′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	5 25	10 36	15 47	22 44·4	10 15 S.
Venus ...	6 30	14 32	22 34	2 41·3	21 22 N.
Mars ...	6 34	13 30	20 26	1 38·7	10 7 N.
Jupiter ...	2 26	6 22	10 18	18 29·3	22 58 S.
Saturn ...	13 17	20 56	4 35*	9 6·5	17 49 N.
Uranus...	19 45*	1 11	6 37	13 17·3	7 28 S.
Neptune..	8 0	15 44	23 28	3 53·0	18 35 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Mar.	h.	
24	12	Jupiter in conjunction with and 0° 41′ south of the Moon.
25	—	Venus at period of greatest evening brilliancy.
29	12	Mercury in conjunction with and 2° 2′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Mar. 27,	4 36	*m*
Algol	3 1·0	40 32 N.	,, 26,	1 12	*m*
			,, 28,	22 1	*m*
ζ Geminorum	6 57·5	20 44 N.	,, 30,	1 0	*m*
R Canis Majoris	7 14·5	16 11 S.	,, 28,	20 56	*m*
			,, 30,	0 12	*m*
U Cancri	8 29·4	19 17 N.	,, 24,		*M*
U Coronæ	15 13·7	32 3 N.	,, 24,	19 57	*m*
S Sagittæ	19 51·0	16 20 N.	,, 26,	0 0	*M*
R Sagittæ	20 9·0	16 23 N.	,, 28,		*m*
T Delphini	20 40·2	16 0 N.	,, 27,		*M*
T Vulpeculæ	20 46·8	27 50 N.	,, 24,	4 0	*m*
			,, 25,	6 0	*M*
δ Cephei	22 25·0	57 51 N.	,, 26,	23 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near δ Boötis	228	33 N.	March 27. Very swift.
,, γ Libræ	233	15 S.	Swift; long paths.
Between ξ and ζ Draconis.	263	62 N.	March 28. Rather slow.

GEOGRAPHICAL NOTES.

AT the last meeting of the Paris Geographical Society, M. Ed. Blanc made a communication giving the results of his four years' researches among the oases on the south of Tunis. Referring to the question of the desiccation of the Sahara, and to the obliteration of formerly existing oases by the sand, M. Blanc said he did not believe it was due to the destruction of the irrigation works which had been established by the Romans, nor to any alternation of periods of drought with periods of humidity. Nor did he attribute it to the disafforestation or the depasturage of the country; forests, he believes, have very little if any influence on rainfall. The desiccation of the Sahara M. Blanc attributes to general geographical causes, resulting from modifications in the contour of the continents, such as the emersion of the steppes of Central Asia, the gradual disappearance of the snows which existed during the Glacial period in certain mountain masses of Europe and even of Africa, and perhaps also the elevation of a part of the depressions of the Western Sahara. These various causes, according to M. Blanc, have led to a deficiency of moisture in the air-currents which prevail over that part of Africa, and the equilibrium once broken between rainfall and evaporation, there results a progressive desiccation, more and more marked, and irremediable with the means at present at our disposal. The spread of the sand is a natural result of the exhaustion of the desiccated soil under the action of the winds. It is therefore a consequence of the above conditions, and could only be arrested by an alteration in the rainfall. Still, M. Blanc thinks that though one cannot alter existing conditions it is quite possible to palliate them, and especially by tapping underground supplies increase the number of oases, and restore to fertility others which have existed in former times, but are now overrun with sand.

MR. F. ARNOT, the young missionary who has spent seven years in South Central Africa, returns to Loanda in a few days. Mr. Arnot intends to proceed again to the Garanganze country to resume his labours. He means, however, to travel about the region of which Lake Bangweolo is the centre. The contour and position of that lake he will endeavour to settle, and also lay down as far as possible the courses of the various rivers that contribute their waters to the Congo. Mr. Arnot has during his stay at home been qualifying himself for taking exact observations, so that we may expect important contributions from him to the geography of Central Africa.

THE immense terrestrial globe which is being constructed for the forthcoming Paris Exhibition will have many points of

interest. It will be one-millionth the size of the earth; a millimetre on the globe will represent a kilometre on the surface of the earth. The globe will be about 30 metres (nearly 100 feet) in diameter. On this scale it will be possible in most cases to give geographical details their true dimensions; Paris will occupy just about 1 centimetre, and may serve as a unit with which to compare the dimensions of other features. All the great lines of communication by land or by sea can be shown in detail. In the enormous cupola under which the globe will be placed, it will be possible by means of a clock-work arrangement to turn the globe on its axis and convey a precise notion of the diurnal rotation of the earth. A point on the equator will move at the rate of half a millimetre per second. The many educational uses to which such a globe could be put are evident. The globe, we understand, is well advanced towards completion.

In the most recent number of the *Bulletin de la Société de Géographie*, M. Jules Marcou continues and concludes an account of his investigations into the origin of the name America. He rejects, for very many reasons, which will be found in the contribution in question, the ordinary derivation—that is, from the Christian name of Vespucci, the Italian navigator. Vespucci, he says, never took the name Amerrigo, or Amerigo, till after America was discovered, and, through vanity, he kept up the proud title. The name is an indigenous one, M. Marcou thinks, and means the country of the wind, or the land rich in gold. Four-fifths of the storms which are met with in the Atlantic come from America, and the gold put in circulation by Columbus's discovery explains the second meaning of the term. Between Lake Nicaragua and the Mosquito Coast is a range of mountains called the American Mountains, inhabited by a tribe of Indians, now very few in numbers, who bear the name, "Los Amerriques," and who have been, according to the President of the Nicaraguan Republic, who supports this view, continually in communication with the whole of the Mosquito Coast. Columbus, and not Vespucci, was the discoverer of America, and the country was named from this place and these people, and not from Vespucci, who was a man of no importance, until he either took the name of Amerrigo, or until it was given him.

Captain Cecchi, Italian Consul at Aden, sends to the *Bollettino* of the Italian Geographical Society some further particulars of Count Teleki's expedition to the north of Masai Land, and his discovery of the two lakes Samburu (Upper and Lower Narok) in that region. The explorer, who was accompanied by Lieut. L. von Höhnel, of the Austrian Navy, after ascending Mount Kenia, reached the Niems territory on November 21, 1887, and was detained there till the following February. On the 10th of that month the party continued their journey in a northerly direction, and on March 6 arrived on the southern shore of the large lake Samburu (Upper Narok), to which Count Teleki gave the name of his intimate friend, the late Crown Prince Rudolph. The much smaller basin of the Lower Narok, which was also re-named Stephania, in honour of the Crown Princess, was reached on April 4, after which the Expedition returned to the coast at Mombasa. At the northernmost point to which they penetrated (nearly 6° N. lat.) they were within about seventy miles of the Kaffa country, and they describe the two rivers flowing thence southward to the northern extremity of Lake Rudolph as "very important streams." This agrees with the results of Sig. Borelli's explorations in the region south of Shoa, and makes it highly probable that one of these two rivers is the Omo (Ghibieh), which in that case would be an affluent, not of the Juba or of the White Nile, but of the inland Samburn basin, lying between those two water-systems. To the same basin belongs Lake Stephania, which communicates with the northern extremity of Lake Rudolph, about lat. 4° 20′ N. The expenses of this important expedition were entirely borne by Count Teleki, who is a wealthy Hungarian nobleman. Captain Cecchi's letter is accompanied by a map of the two new lakes prepared from Lieut. von Höhnel's original sketch.

SIXTH ANNUAL REPORT OF THE FISHERY BOARD FOR SCOTLAND.

THE Report for 1887 is published in three separate parts—a plan which will be found convenient in many ways. Part I. contains the General Report; Part II., Report on Salmon Fisheries; and Part III., the Scientific Investigations. The product of the sea-fisheries of Scotland continues to be very large, but in 1887 the prices of most kinds of fish were much lower than they had been for a great many years previously; the fishermen only received a very small return for their capital and labour, and many of them were reduced to a state of extreme poverty. Of the different fisheries of Scotland, that of the herring continues to be by far the most productive and valuable. The fishing of 1887 was less so than that of 1886, but, owing to the great depression in the herring trade, it was not entered into nor carried on with the spirit and industry of more prosperous times. The summer's catches of herring were, speaking generally, much inferior to those of 1886, but the winter herring fishery was the most productive ever known. With yearly fluctuations, the yield of the herring fishery on the Scottish coast has, since the beginning of the century, gone on increasing in an extraordinary degree. The total quantity of white fish landed and sold for consumption fresh showed a large increase compared with 1886, being the largest landed in any year during which returns have been collected; whilst the shell-fish showed a considerable decrease. The total gross value of the sea-fisheries of Scotland for 1887 was £1,915,602 10*s*. The Board are much impressed with the beneficial results to fishermen and curers arising from increased telegraphic communication, which, when further developed, will be of immense value for the promotion of the fishing industry. It is satisfactory that thirteen new telegraph stations are to be opened in remote districts; also that a number of harbours are being improved or constructed. The marine police and fishery superintendence was carried on by H.M.SS. *Jackal*, *Vigilant*, *Firm*, and *Active*.

The inference from Mr. Young's Report on the oyster and mussel fisheries on the south-west coast of Scotland is that there are many places where the cultivation of these mollusks might be renewed or established, but that, to make this of any permanent use, such nurseries should be the property of someone whose interest it would be to make the concern profitable to the public and himself, of course under proper legislation. In the salmon fisheries, owing to the dryness of the season, the angling was very poor, but the nets, especially those in the tide-ways, were very successful. The total value was £282,523 10*s*.

The third part of the Report, consisting of 400 pages and 17 plates, contains the results of the scientific work. The text is divided into three sections, and is preceded by a general statement, which touches upon the scientific work done in 1887, and draws attention to the matters requiring special attention in the immediate future—viz. (1) how adequate supplies of bait for the line fishermen may be best provided. (2) What measures should be taken to improve certain exhausted fisheries of the shores and in-shore waters, such as mussels, lobsters, oysters, &c., by artificial cultivation, or otherwise. (3) The collection of all possible information bearing upon the influence of different modes of fishing, especially in the territorial waters, and in relation to the destruction of young fish. (4) The study, by means of the *Garland* and otherwise, of the distribution, migrations, and spawning periods of the edible fishes, and of the distribution and movements of the floating organisms which form a large portion of their food. (5) The extension of our knowledge regarding the physical conditions of the fishing-grounds and of the waters around the coast. (6) The collection of special statistics in relation to the fisheries of particular districts. The value and utility of such investigations and inquiries is manifest. It is very desirable that some measures should be taken with the object of providing a plentiful supply of bait for the line fishermen. The condition of certain of the shore fisheries is also a subject of considerable gravity. The oyster, as an article of commerce, is becoming slowly but surely extinct in Scotland, the total value of the yield for 1887 being only £965. The diminution in the numbers and in the size of the lobster has been referred to, and it will be an unfortunate circumstance if, for the want of active steps being taken, this important branch of the Scottish fisheries, now rapidly falling off, is allowed to follow the oyster in the process of practical extinction.

Section A of the appendixes contains the general scientific reports, the longest of which is that on "The Trawling Experiments of the *Garland*, and the Statistics of the East Coast Fisheries," Part 2, by Prof. Cossar Ewart and Sir J. R. G. Maitland, illustrated by three admirable charts, a map showing the chief areas investigated by the *Garland*, and about 130 pages of elaborate and accurate statistical tables. The influence of

excessive beam-trawling in the in-shore waters has formed the subject of several Parliamentary inquiries; but from want of trustworthy statistics or scientific evidence the conclusions arrived at have not always been in agreement. The result of the *Garland* experiments and the statistics collected shows that in those areas where trawling is prohibited, the fish, especially the flat-fish, have largely increased and the number of young fish is greater than formerly. The results of the *Garland's* investigations as to the distribution of edible fishes, their numerical variations at different seasons, and the proportional abundance of young and adults are also given, and the scientific statistics collected are discussed in detail. It is evident that by the systematic collection of scientific statistics regarding the productiveness of the fisheries and the relative influence of special modes of fishing, the migration, the spawning processes, and the general life-history of the fishes themselves, a great deal will be accomplished for the promotion of the fishery industries of Scotland. In this connection it will be seen that the investigations carried on by the *Garland* are calculated to have a high value, since the data thus collected are wide in their scope, systematic, and trustworthy. Without the use of a vessel specially adapted for the purpose such investigations would be impossible.

Section B contains the biological investigations. The paper on "The Scottish Lobster Fishery," by Prof. Ewart and T. Wemyss Fulton, M.B., discusses at due length the interesting question as to the best means of restoring this fishery to its old place in our seas and in our markets. Slowly but surely lobsters have been diminishing in size and number and rising in price; and our once famous home lobster market has to be supplemented by supplanting foreign supplies, simply because Scotch lobsters cannot be got in sufficient numbers of a marketable size. The older and most valuable lobsters have been cleared out, and the less mature forms are being drawn upon. Unfortunately also, the female lobster being much more valuable for cooking purposes, the "coral" is sometimes collected alone, and so, by the systematic destruction of its ova, the lobster is seriously handicapped in the struggle for existence. The use of "creels" or "pots," instead of the older-fashioned "rings," has been also an important factor in bringing about the "over-fishing." The legislation of lobster-fisheries is a very difficult matter, and in Canada, where the restrictions are very severe, they have failed in their aim. It is impossible to set one fixed close-time all over Scotland, as there is no general agreement among fishermen as to which time would be most serviceable. The institution of a minimum legal size is a regulation generally adopted, and it is proposed that the gauge of 8 inches should be raised to 9 inches. Further, that, as in Norway and the United States, artificial culture should be resorted to. By artificially hatching the eggs and rearing the young through the larval stages till they have reached a certain size, they are protected from their natural enemies, and if then transferred to the sea would be better able to take care of themselves. That the process is feasible has been shown both in Norway and the United States. A complete lobster-hatchery could be established for a comparatively small sum at some suitable point on the west coast. Unless some steps be taken, the lobster fishery of Scotland is likely to become the memory of a pleasant and profitable past. Mr. Thomas Scott gives in interesting detail, a "Revised List of the Crustacea of the Firth of Forth," which records 230 species, including 41 of Ostracoda, 42 of Copepoda, and 13 Schizopods. Of these species many are new to the district, and two Copepods—*Aztrotrogus papillatus* and *Cyclops ewarti*—new to science. This list is a valuable addition to our knowledge of the Crustacean fauna of this region, previously studied by Leslie, Herdman, Henderson, and others. Mr. Scott also supplies notes on the contents of the stomachs of herring and haddock, and on interesting fishes, &c., sent to the University of Edinburgh. The nature of "red cod," a fungoid condition sometimes met with in the preserved fish, is described by Prof. Ewart; Dr. Edington furnishing and figuring the results of a bacteriological investigation as to the nature of the organisms present, and the cause of the red coloration. Red cod was first noticed in America, but has since been observed in various places, including two as far apart as Algiers and the Hebrides. Used as food in this state, the results may be disagreeable or even dangerous. It was generally referred to the presence of a minute Fungus (*Clathrocytis roseo-persicina*), but Dr. Edington ascribes it to a special Bacillus (*Bacillus rubescens*), which also existed in the salt used for curing, and thus infected the fish. Mr. W. L. Hoyle reports on biological investigations in the sea to the west of Lewis during July and August 1887, and gives a list of the various forms obtained. Prof. McIntosh, F.R.S., gives Reports from the Marine Station at St. Andrews. These deal chiefly with the stages of development in several of the food-fishes, and with the Annelids and other forms used as bait. Mr. Calderwood furnishes notes on an intra-uterine specimen of the porbeagle (*Lamna cornubica*); and Mr. J. Murray on the fishing-grounds of the Stonehaven district, of which he is officer. A new and edifying feature of the Report is furnished by Dr. Wemyss Fulton's account of contemporary work relating to the scientific and economic aspects of the fisheries. The contemporary work carried on in England and Ireland, United States, Germany, Norway, Denmark, Holland, Italy, and Japan is also summarized.

Section C, which is devoted to the physical investigations, contains three papers: on the apparatus required for carrying on physical observations in connection with the fisheries, and on a physical and chemical examination of the water in the Moray Firth and the Firths of Inverness, Cromarty, and Dornoch, by Dr. John Gibson and Dr. H. R. Mills, the latter also reporting on the physical observations on the sea to the west of Lewis, taken during the cruise of the *Jackal*, as described previously by Mr. Hoyle. These papers are illustrated by ten graphic plates, with tables, charts, &c.

As may be seen from the foregoing abstract of the Report, the Scottish Fishery Board is doing good and trustworthy work in many directions, and, from a scientific and commercial point of view, it deserves every encouragement from the Government and the public. It would be an important aid if, by interchange of publications, the Board could be kept in touch with the important fishery organizations abroad. Unfortunately, at present the number of copies of the Report placed at the disposal of the Board renders this impossible. This should be remedied speedily, as, from an economical point of view, there should be the freest circulation of knowledge on questions touching such an important industry.

SCIENTIFIC SERIALS.

Revue d'Anthropologie, troisième série, tome iv. (Paris, 1889).—The Hottentots in the Paris Garden of Acclimatization, by M. Deniker. As the group consists of only six men, five women, and two children, the observations and anthropometric measurements made by the author cannot be regarded as contributing any very important facts to general ethnological inquiry. To French readers the subject has, however, the interest of novelty, since it would appear that the physical conformation of these South Africans has not hitherto been often made the subject of careful study among French ethnologists owing to their lack of opportunity for examining the living subject or measuring the cranial remains of the people, since the Museums of France contain scarcely more than thirty skulls in all, including both Hottentots and Bushmen. The observations of M. Deniker agree generally with those of Profs. Flower, Virchow, and Davis; and like them he believes that we must regard the Hottentot as belonging to the dolichocephalic type, while the Bushmen must be included under the mesocephalic group, the cephalic index being, however, nearly the same, 73, for both. The women of the party all presented the well-developed steatopygia, which is generally admitted to be a national characteristic of the sex, this condition being specially marked even in one of the elder women whose body was almost emaciated in all its other parts. A curious abnormality was noted in two of the men, and in one woman, who presented an interdigital membrane between the second and third toes, affecting both feet in one case.—M. S. Reinach in an article on the museum of the Emperor Augustus, whose collection of bones and arms is referred to by Suetonius, points out that owing to inexact interpretations the precise meaning of the writer has gradually been more and more distorted. M. Reinach thinks that we must seek for the site of this so-called museum at Capri, and not on the Roman Palatine, as M. Nadaillac supposes; while he believes that the "*gigantum ossa*" spoken of by Suetonius were fossil bones, popularly characterized as "*arma heroum*." This opinion, the author thinks, derives support from the story of Samson slaying the Philistines with a jaw-bone; animal bones having been found by primitive peoples to be more readily available as weapons than implements of stone, which required labour for their fabrication. There is no doubt, moreover, from a reference in the Vedic

hymns to the weapons of Indra, that animal bones were used among the early peoples of the East for purposes of offence and defence.—Continuation of Dr. Seeland's notes on Kashgaria and the passes of the Tian-shan. The author's description of the city of Kashgar, which lies on an extensive elevated plateau 3750 feet above the level of the sea, shows that the spot described in such glowing terms by Marco Polo is now nothing more than a confused network of foul, narrow, and tortuous streets, the houses of which lack every requirement of comfort, and almost of decency, as judged by our notions. The dwellings of the richer people have indeed large gardens filled with luxuriant fruit-trees, but the modern traveller would seek in vain for the shady groves, sparkling fountains, splendid mosques, spacious baths, rich bazaars, and lovely women, spoken of by the old Venetian writer. The ethnic type of the Kashgarians is clearly that of a deteriorated mixed race, in which the original Aryan or Turkish character has been nearly obliterated by repeated admixture with different Mongol races. The Chinese officials, under whose rule the people have long languished, effectually prevent all improvement in the country or the people, as is sufficiently shown by Dr. Seeland's account of the mode in which they govern this once fertile region. Indeed, nothing can be more deplorable than the account given of the personal appearance, character, and sociology of the Kashgarians, who exhibit the most marked slovenliness and incapacity, with a melancholy and passive temperament; and whose only pleasures are derived from an excessive abuse of narcotics, accompanied by a marked degree of sexual depravity, which they have possibly acquired through their intimate association with the Chinese. It fact, honesty seems to be the only virtue left to the Kashgarian. The account given by the author of the effect of Chinese rule in this part of Asia Minor agrees with the views of the late M. Prjevalsky, and certainly seems to warrant the conviction cherished by these travellers that the only apparent chance of ameliorating the condition of the people would depend upon the annexation of Kashgaria to the Russian Empire.—The race of Lagoa Santa of Brazil, by Dr. Soren Hansen. The bones found by M. Lund in the caves of Lagoa Santa were mostly deposited in one of these numerous recesses. These human remains were not associated with any animal bones, from which it could be determined with absolute certainty whether they were contemporaneous with a Tertiary or a Quaternary fauna, while the absence of every kind of implement left the antiquity of the race equally uncertain. These remains, which include fifteen more or less entire skulls, besides a very large number of bone fragments belonging to persons of all ages, are preserved in the Zoological Museum of Copenhagen. All the crania present remarkable uniformity with two skulls, respectively preserved in the British Museum and at Rio, which have been referred by M. Quatrefages to a Papuan type, and they appear to give support to his opinion of the existence over the greater part of South America of a primitive dolichocephalic race, which was subsequently intermingled with peoples presenting a brachycephalic character.

Notes from the Leyden Museum, vol. xi. No. 1, January 1889, contains twenty articles, chiefly descriptions of new genera and species of insects. The more important entomological papers are: Dytiscidæ et Gyrinidæ nouveaux ou rares, par M. Régimbart; Neue Coleopteren, beschrieben von E. Reitter; and a note on *Macronota apicalis*, G. and P., by J. R. H. Neervoort van de Poll.—There are two interesting papers by Dr. R. Horst, on a remarkable Syllis bud, with extrudible segmental organs (plate 1); and contributions towards a knowledge of the Annelida Polychæta (plate 2). This latter note, treats about the species of the genus Arenicola found at Naples: these are *A. claparedi*, Levinsen, possibly peculiar to the Mediterranean, but should be looked for on our southern coasts; *A. cristata*, Stimpson, originally described from South Carolina, but now found at Naples; *A. grubii*, Claparède, very common in the Gulf of Naples.—Dr. F. A. Jentink gives notes on a collection of mammals from East Sumatra, based on the collections made by Dr. B. Hagen. The orang-utan is to be found along the coasts of the northern half of East and West Sumatra, and among the other mammals, hitherto not recorded from Sumatra, though known to occur in Borneo, are *Arctogale stigmatica*, *Hemigalea derbyana*, *Herpestes brachyurus*, *Cynogale bennetti*, *Ptilocercus lowii*, and *Rhizomys dekan*.—Mr. J. Büttikofer gives notes on a new collection of birds from South-Western Africa (plate 4). The collection was made at Gambos, in the Upper Cunene region; it contained 267 skins, representing 103 species, of which number 49 were not mentioned in Mr. Büttikofer's previous list; two species are new, *Lophocercs alboterminatus* and *Francolinus jugularis*.

THE longer papers in the *Nuovo Giornale Botanico Italiano* for January are almost entirely floristic, relating to the phanerogamic or cryptogamic flora of particular districts of Italy.—Signor G. Arcangeli describes a remarkable monstrosity of *Narcissus Tazzetta*, in which the "corona" is divided into six petaloid leaves.—In the Reports of the meetings of the Societa Botanica Italiana, the same botanist follows up his account of the structure of the seed of *Euryale ferox* by a description of those of our common water-lilies, *Nymphæa alba* and *Nuphar luteum*. They all agree in the occurrence of a scanty endosperm, consisting of from one to four layers of cells, and a very copious perisperm.—Signor R. Pirotta describes the mode of fertilization of *Amorphophallus Rivieri*, which is effected almost entirely by a number of different species of Coleoptera.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 15, 1888.—"Observations upon the Electromotive Changes in the Mammalian Spinal Cord following Electrical Excitation of the Cortex Cerebri. Preliminary Notice." By Francis Gotch, Hon. M.A. Oxon., B.A., B.Sc. Lond., and Victor Horsley, B.S., F.R.S., Professor of Pathology, University College, London. (From the Physiological Laboratory of the University of Oxford.)

Hitherto pathologists have attempted the analysis of the epileptic convulsion by the graphic method—that is, by recording the spasmodic contractions of the muscles involved. Recent investigations of this kind have shown that the excitation of the cortex cerebri, whether by electrical or chemical means, or by the presence of certain pathological states, neoplasms, inflammation, &c., is invariably followed in the higher mammals by a definite and characteristic sequence of movements in the muscles. It is, however, obvious that such investigations have up to the present succeeded in determining the characters of the neural disturbance only when this has reached the peripheral terminations of the efferent nerves. Now since the excitatory processes originating in the cortex are conducted by the efferent channels in the spinal cord, presumably the pyramidal tracts, the problem of their relationship to the centres of the bulbo-spinal system cannot be determined by experiments which record the mechanical changes in the muscles. In order to ascertain what share respectively the centres in the cortex and those in the spinal cord have in the production of the characteristic epileptic sequence, the action of the latter must be eliminated. This can be done by investigating the nature of the excitatory processes in the cord when the efferent channels in the dorsal region for the lower limbs are made the subject of observation.

For this purpose we determined to obtain, if possible, evidence as to the nature of the excitatory processes of the epileptic convulsion in the spinal cord, as shown by "tapping" the cord and noting the electromotive changes which, as is well known, accompany functional activity in nerves. The results we have already obtained are so harmonious and demonstrative, that we venture to make this preliminary communication, reserving full details for a subsequent account.

PART I. *The Electromotive Change following a Single Excitation of the Mammalian Nerve.*

Our first experiments were made for the purpose of ascertaining to what extent we could detect an electromotive change following a single excitation of a mammalian nerve. Since the discovery by du Bois-Reymond of the fact that the excitatory process in nerve is accompanied by an electromotive change, the characters and time relations of this change have been investigated by various observers, notably by Bernstein, Hermann, Hering, and Head. The general result of their observations is to show that the change following a single stimulus is of very short duration, so short that the galvanometer gives little evidence of its presence, and the observers referred to were compelled to adopt the device first employed by Bernstein, which involves repeated excitation and consequent summation of effect, a method well known to physiologists as that of the repeating differential rheotome. For our purpose it was essential to obtain evidence of the effect following one stimulus only, and this we were fortunately able to do by using a sensitive Lippmann's capillary electrometer of quick reaction, made by Mr. G. F. Burch, and

belonging to Dr. Burdon Sanderson, who kindly placed it at our disposal. This instrument, when the capillary was magnified 400 times by the observing microscope, gave a perceptible response when connected through a resistance of 10,000 ohms for one-thousandth of a second with an electromotive difference of only 0·003 D. The amount of movement of the mercury was estimated by the divisions of a micrometer eye-piece, one division of which indicated an actual movement of $\frac{1}{160}$ of a millimetre. After we had found that the electrometer, when connected with the transverse and longitudinal surfaces of the sciatic nerve of the toad, showed a response of one division following the application of a single stimulus, whether electrical or mechanical, we proceeded to the examination of the sciatic nerve in the rabbit, cat, and monkey. For these experiments the animal was in every case kept under the influence of ether, which was maintained throughout the whole experiment, and the animal was killed before recovery. The sciatic nerve seemed for many reasons the most suitable of the mammalian nerves. It can be quickly prepared for 7 or 8 cm. of its length, its nutrition is well preserved, since the *arteria comes nervi ischiadici* can be left uninjured, and its diameter lessens the dangers of drying.

The nerve, having been rapidly prepared and bathed in warm saline solution, 0·6 per cent., was ligatured low down in the thigh, the ligature including the popliteal trunks. It was then divided on the peripheral side of the knot, and raised in air so as to be at right angles to the limb. One kaolin pad of a non-polarizable electrode was applied to the cut end, and another to the longitudinal surface at a distance of 1·5 cm. A pair of sheathed exciting platinum electrodes, 2 mm. apart, was then applied to the trunk of the nerve 6 cm. centrally from the nearest leading-off electrode, *i.e.* opposite the sciatic notch. The exciting stimulus was obtained by the break of the current of a single Callaud cell supplying the primary coil of a du Bois-Reymond inductorium graduated by Kronecker. The break shock produced in the secondary coil by this means was so feeble as to be barely perceptible on the tip of the tongue when the secondary coil completely covered the primary. The break was effected by the spring rheotome, which opened a fixed key at a definite point in its course. The electrometer was connected with the non-polarizable electrodes by a circuit which included the usual compensator. By means of a switch the electrometer could be cut out, and the circuit made to include a high resistance galvanometer, which also revealed the single variation. The two instruments could be thus readily compared. The excursion of the mercury of the electrometer was ascertained both by direct observation in terms of the divisions of the micrometer eye-piece, and by photographing the projected capillary upon a moving sensitive plate; in the latter case the capillary was magnified 100 times. The results of our observations are briefly as follows:—

The mammalian nerve showed a well-marked difference or demarcation current—that is to say, the electrode upon the longitudinal surface was notably positive to that on the cut end. The movement of the mercury corresponding to this difference amounted in some cases to 60 divisions of the micrometer, and was produced by an electromotive force which was estimated as from about 0·01 to 0·015 D. The passage of the single break induction shock through the platinum electrodes in either direction was followed by a small quick movement of the mercury, which was invariably in the opposite direction to that produced by the demarcation current. Its amount varied in different animals from 1 to 2·5 divisions of the micrometer eye-piece, and was photographed. After severing the nerve from the bulbo-spinal system above the exciting electrodes, the same effect was obtained; its character, as shown by the movement of the mercury, was, however, different, being as we believe much shorter in duration and less in amount. But, our experiments not being directed to the elucidation of this point, we will not speak positively with regard to it. After a time, varying in different cases from twenty minutes to three-quarters of an hour, the effect was no longer visible. We convinced ourselves that the movement we obtained and photographed was due to the electromotive change which accompanies the propagation of an excitatory state along the mammalian nerve when this state is evoked by the application of a single stimulus.

PART II. *Excitation of the Cortex Cerebri.*

A. *Mixed Spinal Nerve connected with the Electrometer.*—In two cases we have connected in the manner described in Part I. the sciatic nerve with the electrometer, and have then exposed by a small trephine opening the so-called motor cortical centre for the lower limb. This we then excited by a very weak but adequate faradic current. So far, however, we have not been able to detect any movement in the mercury, although the muscles of the investigated limb supplied by the anterior crural nerve were thrown into a state of active convulsion. It is probable that the character of the neural disturbances in the mixed nerve may be best studied by investigations which we shall shortly undertake upon the electromotive changes in the muscles.

B. *The Spinal Cord connected with the Electrometer.*—The experiments, the results of which are now to be briefly detailed, were made in the following manner:—

The spinal cord of the etherized animal (cat and monkey) was exposed in the lower dorsal region for about 4 cm., and as low down as the upper end of the lumbar enlargement. Great care was taken by bathing with warm saline to guard as much as possible against the dangers of error due to cooling and drying. The dura mater having been split longitudinally, a strong thread was passed round the spinal cord at the lower limit of the part exposed. It was tied firmly and the cord divided below the knot. By successive division of the two or three roots exposed in the intervertebral foramina, the cord was easily raised from the neural canal and suspended in the air without any great interference with the circulation in the longitudinal vessels.

One of the non-polarizable electrodes was then brought into contact with the cut end of the cord and the knotted ligature, while the other was connected with the longitudinal surface of the cord 2 cm. from the cut end by means of soft thread cables soaked in saline solution and tied loosely round the cord. In one experiment the connection was with one lateral column only. Mass movements of the electrodes upon the spinal cord were suitably guarded against, though it was found that the cord might be shaken without producing any effect in the electrometer.

On connecting these electrodes with the electrometer a considerable electromotive difference was found to exist between the contacts, the excursion of the mercury being so great, *i.e.* beyond the field of the microscope, that its amount could not be estimated in terms of the micrometer eye-piece. The cut surface was always negative to the longitudinal surface, and the amount of the difference as estimated by the compensation method was about 0·02 D. It appeared to be highest when the section passed through the dorsal region without involving the lumbar enlargement. A difference between the surfaces of the cord has been previously observed by du Bois-Reymond.

The cortex cerebri was now exposed and the exciting circuit prepared. The inductorium previously employed was again used with one Daniell cell in connection with the interrupter of primary coil and the Helmholtz side wire. The exciting electrodes had platinum points 2 mm. apart.

The demarcation current having been compensated, and the electrometer placed in connection with the non-polarizable electrodes, the motor area for the lower limb was excited. The results of the observations made upon four monkeys and several cats may be summed up as follows:—

(1) The application of the exciting electrodes to the cortex was without exception only followed by a movement in the electrometer when the area of representation of the lower limb was touched, and this even when owing to prolonged excitation of the arm area the upper limb was in violent epileptic convulsion. We found that when the exciting electrodes were moved over the surface of the brain the observer at the electrometer only gave notice of a movement in the instrument when the person exciting had crossed the margin of representation of the limbs. This shows that electromotive changes in the cord sufficient to affect our instrument occurred only when the motor area of the lower limb was excited. All error due to escape is thus set on one side, while at the same time this remarkable fact confirms the localization of function.

(2) The excitation of the motor area for the lower limb was accompanied and followed by characteristic movements of the mercury. The excitation by means of the interrupted current usually lasted for three seconds—that is, about 300 equal and alternately directed induction currents passed through the excited tissue. During this period the mercury showed an excursion opposed in direction to that of the difference between the longitudinal surface and cut end of the cord. This excursion persisted as long as the excitation lasted, and ceased when this was left off. Then after an interval of from one to three seconds there ensued a rhythmical succession of excursions each opposed

in direction to the resting difference, some apparently single and others multiple. These lasted from twenty to thirty seconds and suddenly ceased.

We have repeated this observation thirty or forty times, and feel ourselves justified in concluding that we have obtained evidence that during a cortical epileptiform discharge the electromotive changes in the spinal cord are exactly parallel as regards the character of their sequence to the convulsions of the muscles as recorded by the graphic method. It remains to be stated that after removal of the cortex we have obtained an effect in the electrometer when the corona radiata was stimulated. This effect was only present during the period of excitation, no rhythmical after effect ever being observed. Its character was prolonged, and resembled the first persistent stage referred to above.

In conclusion, we consider that, since by the method we have adopted the influence of the lumbar bulbo-spinal centres is excluded, the existence of the epileptic rhythm in the dorsal regions of the spinal cord points to its being almost entirely of cortical origin.

Physical Society, February 23.—Prof. Reinold, President, in the chair.—Dr. J. W. Waghorne read a note on the measurement of electrical resistance, showing that two resistances may be compared by joining them in series with a battery, and observing the deflections of a galvanometer connected successively with their terminals. The resistances are proportional to the currents which pass through the galvanometer in the two cases, provided they are large compared with that of the battery, or are not very different from each other. By using a rocking key, the method is rendered expeditious, and the galvanometer resistance need not be known.—On a new polarimeter, by Prof. S. P. Thompson. The author gave a *résumé* of the ordinary methods of determining the position of the plane of polarization, pointing out their advantages and disadvantages; and exhibited his new polarimeter, in which two black glass mirrors, placed at a small angle (about $2\frac{1}{2}°$), are used to polarize the light in two different planes. By using a modified Nicol as analyzer, the plane can be determined to one-tenth of a degree, when the substance examined does not absorb much light; but, for dark-coloured liquids, the author prefers to use one of his "twin prisms," described before the British Association in 1887, as polarizer, in which the planes of polarization are 90° apart. A method of dividing a polarized beam into two parts inclined at a variable angle, by means of a combination of quarter-wave plates of mica, was described, in which the two halves of the field are similarly coloured. Mr. Glazebrook considered Poynting's glass cell, with different thicknesses of active solution, a very convenient means of obtaining two beams polarized at a small angle, as, by altering the strength of the solution, the angle may be varied at will.—Prof. Thompson also read a note on the formation of a cross in certain crystal structures. Several specimens (including benzoic acid, stalactite, Eno's salt, &c.), which exhibit a radial structure, and show a cross when examined by polarized light, were thrown upon the screen, and the fact that the cross remains stationary when the specimens are rotated demonstrated. Similar effects were produced by mica sectors arranged radially, thus showing the stationary cross to be caused by the light not being analyzed in those directions.—On electrical measurement, by Prof. W. E. Ayrton, F.R.S., and Prof. J. Perry, F.R.S. In a paper on winding voltmeters, read before the Society in 1885, the authors showed, on the assumption that the thickness of insulating covering on wires was proportional to their diameter, that instruments wound with copper wire gave a less heating error than similar ones wound with German silver. Since then, platinoid has been introduced, and the electrical constants of phosphor-bronze determined. Further, a remarkably simple relation between the volts corresponding to a given deflection on a given type of instrument, and the resistance per unit length of the wire used in winding it, has been suggested by Mr. Crawley. Suppose F = number of ampere turns required to produce the deflection P, and U the half area of section and volume of the coil respectively, and d and D the diameters of the bare and covered wires, then—

$$A = \frac{F}{n},\ n = \frac{P}{D^2},\ r = \frac{4l\rho}{\pi d^2},\ \text{and}\ l = \frac{U}{D^2};$$

from these we get—

$$V = Ar = \frac{FU}{P} \cdot \frac{4\rho}{\pi d^2} = K \cdot \frac{4\rho}{\pi d^2},$$

(say) where K is a constant depending on the type of instrument. Since $\frac{4\rho}{\pi d^2}$ is the resistance per unit length, *the volts required to produce a given deflection are proportional to the resistance per unit length of the wire used, whatever be the material of the wire or thickness of the insulation.* Taking this into account, and using a more accurate value for the thickness of the covering, it is shown that the four metals above referred to arrange themselves in the following order of merit when used for high-reading voltmeters—platinoid, phosphor bronze, german silver, and copper; and for comparatively low-reading instruments, the last two change places. As a standard ammeter of great range, a circuit containing a Depretz D'Arsonval galvanometer is shunted by a wide sheet of thin platinoid, and by altering the resistance in the galvanometer circuit, the sensibility may be varied in known proportions. An instrument on this plan has been arranged to measure any current from 0·1 to 800 amperes to one-quarter per cent., and the same galvanometer in series with various resistance coils is used as a standard voltmeter of practically unlimited range. Whilst arranging these standards it has been found that the deflections are not generally proportional to the currents, and the discrepancy traced to the centre of gravity of the swinging coil not being in the line of suspension. By replacing the bottom torsion wire by a long thin spring the defect may be remedied. As relating to calibration curves of instruments, it was mentioned that in "Siemens's dynamometer" the "square law" is not correct, probably owing to distortion of the spring. Referring to "hot wire voltmeters," in which the sag of a wire heated by the current is measured by a magnifying spring, the authors remark that, in their original paper on the subject, they neglected the change of length due to change of stress in the wire, and subsequently their assistant, Mr. Bourne, found that maximum sensibility was never co-existent with minimum sag. The sag which gives maximum sensibility depends on the initial stress in the wire, and by altering the initial sag the instruments may be compensated for changes of temperature of the room. In the present paper the mathematical treatment is more rigorous, and the results are in accordance with experiment. A voltmeter intended for use with "electric welders," which deflects about 300° for 2 volts (direct or alternating), and is graduated to 1/100 of a volt, was exhibited, and used to measure the resistance of a storage cell. Dr. Thompson suggested that the want of proportionality of D'Arsonval galvanometers might be due to lateral displacement of the coil caused by the current in the torsion wires crossing a magnetic field, but from experiments with pivoted coils the authors thought this improbable.—Prof. Rücker read a note on the dimensions of electro-magnetic units, by Prof. G. F. Fitzgerald, F.R.S., which suggests that specific inductive capacity and permeability be *assumed* to be of dimensions $\left[\frac{T}{L}\right]$ (slowness); if this be done the dimensions of quantities expressed in electrostatic and electro-magnetic measure become identical. The author also states that it seems most likely that inductive capacities are related to the reciprocal of the square root of the mean energy of turbulence of the ether. Prof. Rücker remarked that in his recent paper on the subject he considered it important to retain K and μ as secondary fundamental units, and Mr. Blakesley did not concur with Prof. Fitzgerald's suggestion.—A photograph of crystal models, by Mr. R. T. Anderson, of Belfast, was exhibited at the meeting.

Chemical Society, February 21.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Note on the decomposition of potassic chlorate by heat in the presence of manganic peroxide, by Prof. H. McLeod, F.R.S. The author concludes from his experiments that the reaction which ensues when potassic chlorate is heated with manganic peroxide most probably consists in the formation of potassium permanganate chlorine, and oxygen in the first instance; that the permanganate as rapidly as it is produced is decomposed by the heat into potassic manganate, manganic peroxide, and oxygen; and that the resulting potassic manganate is acted on by chlorine generated by the action of the peroxide on some fresh chlorate, forming potassic chloride, manganic peroxide, and oxygen, so that the peroxide is being continually reproduced. The quantity of chlorine evolved corresponds to only a very small proportion of the manganic peroxide present, so, if the first action really takes place, the chlorine must be absorbed and employed in converting the potassium into chloride.—The vapour-density of hydrogen

fluoride, by Prof. T. E. Thorpe, F.R.S., and Mr. F. J. Hambly. Gaseous hydrogen fluoride, on being heated from a few degrees above the boiling-point of the liquid, shows a rapid decrease in density, owing to the dissociation of H_xF_x molecules ultimately into HF molecules, the course of the dissociation being similar to that observed in the case of nitrogen peroxide and acetic acid. The density of the gas at about 32° corresponds with that required for a molecule H_2F_2, but a careful study of the molecular breaking down of the vapour as it is effected by changes of temperature and pressure shows that there is no evidence for the existence of such a molecule. At a temperature of 26°·4, the lowest temperature observed, the density of the gas corresponds with a molecular weight of 51·2 ($H_3F_3 = 60$), and from this point the process of dissociation is perfectly continuous until the temperature increases to about 60°, when the density corresponds with that of a vapour consisting wholly of HF molecules. In the discussion which followed the reading of the paper, Prof. Ramsay said that Prof. Thorpe, in speaking of the analogy of the results obtained in the case of hydrogen fluoride with those of the brothers Natanson for nitric peroxide, had pointed out that these latter afforded insufficient proof of the higher limiting value of n in the formula N_nO_{2n}; and that this limiting value was also unknown in the case of acetic acid, of which the vapour-density also increased with fall of temperature. Now there were three separate lines of argument leading to a knowledge of the higher limiting formulæ of these bodies which had been pointed out by himself and Dr. Young, and of which the data were to be found in papers published in the Philosophical Transactions, in the *Philosophical Magazine*, and in the Chemical Society's Transactions. The first of these has reference to the alteration of density of the saturated vapour with fall of temperature and corresponding fall of pressure. It is argued that the density of the vapour of a substance must necessarily, at any given temperature, be higher when the vapour is on the point of condensation than when it is unsaturated. Hence, if it can be proved that the density of the saturated vapour of bodies like nitric peroxide and acetic acid shows no signs of increasing beyond those required for the respective formulæ N_2O_4 and $C_4H_8O_4$, such formulæ must denote the limit of complexity of the molecules, in the gaseous state at least. To ascertain such a limit, Dr. Young and the speaker constructed from the Natansons' data for the relations of volume, pressure, and temperature of nitric peroxide, and their own data for the vapour-pressure of that body, isothermal curves in which pressures formed ordinates and vapour-densities abscissæ. The terminal points of such curves are characterized by rapid increase of density without rise of pressure, and, in fact, denote that the substance is no longer in the gaseous state, the vapour-pressure of the liquid having been reached. The densities of the saturated vapour therefore will correspond with the angles of union of the isothermal curves with horizontal straight lines representing condensation to liquid under vapour-pressures constant for each temperature. By joining with each other such angles of union for each temperature a curve is obtained expressing the densities of the *saturated* vapour in relation to pressure. It is evident from inspection of such a curve for acetic acid, shown in a plate in the Transactions of the Chemical Society, 1886, 806, that the line of zero-pressure would be cut at the density 60, corresponding with the formula $C_4H_8O_4$; a similar curve can be constructed from the Natansons' results and Ramsay and Young's determinations of the vapour-pressures of nitric peroxide, and this intersects the line of zero-pressure at a point corresponding with the vapour-density 92, equivalent to the formula N_2O_4. The second argument is as follows: Representing the relations of temperature and pressure of a "perfect" gas for any given constant volume, $p = c \cdot t$, where c is a constant and t absolute temperature. This is the equation to a straight line; such a line is termed an *isochoric line* or *isochor*; its point of origin for a perfect gas is absolute zero of pressure and temperature. If a different volume be chosen, the slope of the line is different. Now it is clear that if a given volume of gas contains $2n$ molecules, pressure will rise with rise of temperature at twice the rate that it would if the given volume of gas contained n molecules. Constructing for nitric peroxide and for acetic acid, on the assumption that they are perfect gases, diagrams showing the relations of pressure and temperature for the formulæ NO_2 and $C_2H_4O_2$ at such volumes that 1 gramme occupies, say, 1000 c.c. in each case, the resulting straight lines will manifestly differ in slope from those corresponding to the respective formulæ N_2O_4 and $C_4H_8O_4$, the pressure in the latter case not rising so rapidly with rise of temperature owing to the smaller number of molecules in that volume. But we know that the actual behaviour of these bodies is not that of perfect gases. The line representing the relations of pressure and temperature, should at high temperatures, when the substances exist in the molecular states NO_2 and $C_2H_4O_2$, nearly coincide with the theoretical line for these molecular states; and at low pressures and temperatures with the line denoting the molecular condition N_2O_4 and $C_4H_8O_4$. The data of actual experiment show that such is the case. The S-shaped isochoric curve trends so that it is probable that it would become tangential with that expressing the behaviour of molecules of the higher formulæ, showing no signs of cutting it as it must needs do were still more complex molecules capable of existence. The third line of argument is derived from the application of Raoult's method to a solution of nitric peroxide in acetic acid, and the results obtained show that the molecular weight corresponds closely with the formula N_2O_4.—Contributions to the chemistry of lignification: the constitution of the jute fibre substance, by Messrs. C. F. Cross and E. J. Bevan. The authors describe the results of a fuller study of the lignocelluloses (cf. Chem. Soc. Trans., 1882, 90; 1883, 18).—The atomic weight of chromium, by Mr. S. G. Rawson. The atomic weight of chromium, as determined by converting a known weight of ammonium bichromate into chromic oxide, is found to be 52·061.

Linnean Society, March 7.—Mr. Carruthers, F.R.S., President, in the chair.—Mr. J. E. Harting exhibited specimens of a South American Bat (*Noctilio leporinus*) alleged to be of piscivorous habits, and which, through the kindness of Sir William Robinson, the Governor of Trinidad, had been forwarded from that island by Prof. McCarthy, together with a Report on the subject. From this Report, it appeared that the stomach of one specimen, opened within half-an-hour after it had been shot on the evening of December 29, "contained much fish in a finely-divided and partially-digested state." In three others procured at 6 a.m. the following morning, the stomachs were empty. On the morning of December 31, at 3 a.m., numbers of these bats were observed returning to their caves: two were shot, and "both contained considerable quantities of fish." Prof. McCarthy added that in the stomachs of other specimens examined by him fish scales were undoubtedly present. Of the specimens forwarded in spirits to this country, two had been skinned and the stomachs and intestines examined by Mr. Harting. The sac-like stomach was much less muscular than might be expected in a fish-eating mammal; but in one of them (the other being empty) fragments of a finely-striated and iridescent substance resembling fish-scales were found. A discussion followed, in which Prof. Howes and Mr. W. P. Sladen took part, the conclusion being that, although there was no *a priori* improbability in the alleged piscivorous habits of this bat, it could hardly be accepted as a fact until the fragments, supposed to be of fish, were really proved to be so by careful microscopical and chemical examination.—A paper was then read by the Rev. Prof. Henslow on the vascular systems of floral organs, and their importance in the interpretation of the morphology of flowers. The author drew attention to the importance of this class of observations, as supplementing development and teratology; for, by referring all organs back to their "axial traces," their real origins could generally be discovered. Taking the words metaphorically as "floral units," he explained how they can, as it were, give rise to axes as well as to all kinds of floral appendages. Quoting Van Tieghem's definitions of axial and foliar characters, the former was shown to be subject to exceptions. After describing the arrangements of the cords in peduncles and pedicels, in which endogens often have the cords as regularly placed as in exogens, the author explained the different ways by which pedicels and umbels are formed in each class respectively. The "chorism" and union of cords were illustrated and the effects produced. Considerable light was thrown upon the cohesion and adhesion of organs, and the interpretation of the "receptacular tube" and "inferior ovary" was shown to depend upon the undifferentiated state of the organs when in congenital union. The true nature of axile and free central placentas was revealed, so that in the case of the former, with scarcely any exception, the axis takes no part in the structure, all "carpophores," "stylopods," &c., being simply the coherent and hypertrophied margins of carpels. Similarly, the free central placenta of *Primula* received its interpretation as consisting of the coherent and ovuliferous bases

of fine carpels which have the upper parts of their margins coherent in a parietal manner. Illustrative diagrams were exhibited of nearly seventy genera typical of about thirty orders. The paper was favourably criticized by Dr. D. H. Scott, Mr. A. W. Bennett, and Prof. Marshall Ward.

PARIS.

Academy of Sciences, March 11.—M. Des Cloizeaux, President, in the chair.—Fresh experiments with hydrogen peroxide and chromic acid, by M. Berthelot. In previous communications (*Comptes rendus*, vol. cviii. pp. 24 and 157) it was shown that the reactions between chromic acid and hydrogen peroxide also took place with bichromate of potassium, and that this salt has the property of gradually decomposing an unlimited quantity of hydrogen peroxide, remaining itself unchanged. This continuous reaction was attributed to the formation of an intermediate compound incessantly destroyed and renewed throughout the process of decomposition, and the experiments now described tend to confirm this phenomenon.—On the cephaloid organs in the tendons of birds, by M. Ranvier. The organs to which M. Ranvier has given the name of "cephaloid" are here fully described, and their presence determined in the domestic fowl, pigeon, and duck, but not in the lapwing. Where found they invariably present pretty much the same disposition and structure.—Actinometric observations made in 1888 at the Observatory of Montpellier by MM. Houdaille and Mazade, and reported by M. A. Crova. These observations confirm the general laws established by the records of previous years (1883-87), showing that, while the epochs of maximum and minimum intensities vary with the meteorological conditions, the great maximum always occurs in spring, and the secondary in autumn.—On the solar spots, by M. Spoerer. These remarks are made in connection with the author's recent memoir on the periodicity of the solar spots since 1618 (Hallé, 1889), in which the law anticipated by Carrington is definitely demonstrated and formulated. But although the norma for the distribution of spots in heliocentric latitude is established for many past periods, great aberrations are shown to have prevailed during the period between 1672 and 1713. After the reading of the paper, M. Faye pointed out that according to his own theory the spots depended, like the pores, not on irregular eruptions of a volcanic nature, but on the alimentation of the photosphere, an essentially stable process, or at least subject only to infinitesimally slight variations. In the general complexity of the phenomenon, the pores with the faculæ and cloudy protuberances appear to be the more stable elements, and the spots and metallic protuberances more of an accessory character.—On the value of the revolution of the right ascension screw in a meridian instrument, as determined by the observation of the equatorial or circumpolar stars, by M. G. Rayet. This inquiry shows that the determination of the value in question by observing the transit of a circumpolar is not more exact than that resulting from the observation of equatorial stars, and that when practised in the ordinary way, apart from the phenomena of refraction, it leads to systematic errors, that cannot be neglected in researches needing great accuracy.—On the automatic gauging of an artificial feeder, by M. H. Parenty. The method invented by the author, and described by him in the *Comptes rendus*, vol. civ. p. 1427, has been applied with complete success for estimating continuously and automatically the irregular discharge of the Courpalet feeder of the Orleans Canal, all efforts to calculate which had hitherto been baffled by the varying size of the cuttings, its winding course, and almost imperceptible fall.—On transformations and equilibrium in thermodynamics, by M. Gouy. The method already described in the *Comptes rendus* for February 18, 1889, leads to the use in thermodynamics of a new function, which is here described, and which appears to present the advantage of being directly connected with the consideration of cycles.—Relation between magnetic rotatory power and the transmission of luminous waves by ponderable matter, by M. A. Potier. Here an explanation is sought in the views of Fresnel of magnetic rotatory power in reference to Rowland's electromagnetic theory of light.—Employment of sulphite of sodium for developing the picture in photography, by M. Paul Poiré. Numerous experiments carried out by the author with a solution of sulphite of sodium and pyrogallic acid as a developing bath show that the best results are obtained when the sulphite is in the proportion of 25 per cent. with 1·5 gramme of pyrogallic acid added. The development is slower but more intense and clearer than when the sulphite contains carbonate, and the bath may be used repeatedly and preserved for months in corked bottles.—On the monochloracetoacetic ethers α and γ; synthesis of citric acid, by MM. A. Haller and A. Held. It has already been shown that W. James's cyanacetoacetic ether is identical with that obtained by the authors by treating sodium acetoacetic ether with cyanogen chloride. This view is here confirmed by a fresh synthesis of this cyanide, prepared by making acetyl chloride react on sodium cyanacetic ether.—Papers are contributed by MM. Ph. Barbier and J. Hilt, on australene; by M. A. Müntz, on the fertilizing properties of the Nile waters; by M. Aimé Girard, on the cultivation of the potato; by M. I. Straus, on preventive vaccination against glanders; by M. S. Arloing, on the zymotic effects of the soluble substances contained in the cultures of *Bacillus heminecrobiophilus*; and by M. A. Bottarel, on the poisoning apparatus found in certain fishes.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Carl von Linné's Ungdomsskrifter, vol. ii. (Stockholm, Norstedt).—Mechanics and Experimental Science: Heat and Light: Dr. E. Aveling (Chapman and Hall).—Mechanics and Experimental Science: Magnetism and Electricity: Dr. E. Aveling (Chapman and Hall).—Industrial Education: Sir P. Magnus (K. Paul).—Chambers's Encyclopædia; new edition, vol. iii. (Chambers).—Practical Plane and Solid Geometry; revised and enlarged edition: J. S. Rawle (Simpkin).—Practical Iron Founding (Whittaker). The Orbit of the Planet Sappho (80): R. Bryant (Waterlow).—A Text-book of Pathology, vol. i.: D. J. Hamilton (Macmillan).—The Elastical Researches of Barré de Saint-Venant: edited by K. Pearson (Cambridge University Press).—Agricultural Canada: Prof. Fream.—Report of the Rugby School Natural History Society, 1888 (Rugby).—Philosophy and Specialities: G. Mallery (Washington).—Nineteenth Annual Report of the Wellington College Natural Science Society, 1888.—Journal of the Chemical Society, March (Gurney and Jackson).—Anales del Museo Nacional de Buenos Aires, Entrega Decimaquinta (Buenos Aires).—Proceedings of the Royal Society of Edinburgh, vol. xvi. pp. 1-64.—Annalen der Physik und Chemie, 1889, No. 4 (Leipzig, Barth).

CONTENTS.

THURSDAY, MARCH 28, 1889.

THE NEW TRAVELLER'S GUIDE TO SCIENTIFIC INQUIRY.

Anleitung zu wissenschaftlichen Beobachtungen auf Reisen. In Einzel-Abhandlungen, herausgegeben von Dr. G. Neumayer. Second Edition, Two Volumes. (Berlin, 1888.)

"TO provide the numerous German travellers and dwellers in foreign lands with a simple introduction to scientific investigation in the various departments of natural science" was, in their own words, the object of the promoters of the present work, and this notice is an endeavour to point out, in the first place, the mode in which it has been carried out; and, in the second, to consider how far the effort has been crowned with success.

In its new shape, the book appears with a somewhat smaller size of page, and the matter is so disposed in two volumes, that the one contains the physical, and the other the biological, articles. Thirty-one collaborators have contributed as many different chapters on the various subdivisions of the subject, and, strictly speaking, each of these ought to have a detailed *critique* to itself. Considerations of space, however, forbid this mode of treatment; and hence it must suffice to enumerate the headings of the articles, and to add a few remarks explanatory of their contents in those cases where it seems advisable, premising that no disparagement is implied regarding any which are passed by without comment.

The first chapter, on "Determination of Geographical Position," by Prof. Tietjen, is eminently practical in tone; the instruments employed are briefly characterized, and instructions given for using and correcting them. The same may also be said regarding Dr. Jordan's "Topographical and Geographical Observations." We are glad to observe that both these authorities advise the traveller to depend less upon the fine graduation of his instruments than upon his ability to estimate divisions: work accomplished by the latter means is incomparably more rapid and decidedly less liable to error than that carried out by the aid of more complicated apparatus.

A most important section is that on "Geology," by Von Richthofen, of which we need only now say that it is an abstract of an independent work by the same author, which was reviewed in these columns a few months ago (NATURE, vol. xxxvii. p. 603).

Prof. H. Wild, of the Central Observatory at St. Petersburg, gives, within the compass of about thirty pages, an adequate summary of the present state of our knowledge regarding "Terrestrial Magnetism," as well as a description of an apparatus suitable for travellers, with directions for its use. Observations upon land only are here discussed, those on board ship being reserved for special treatment in the sequel. The article "Meteorology," by Dr. J. Hann, of Vienna, is commendable, amongst other things, because it informs the traveller what he need not do. A passage of this nature comes upon the traveller, overwhelmed by the multitude of minute instructions, like an oasis in the desert. Nevertheless, the directions regarding what ought to be done are none the less explicit, and the concluding observations are dictated by sound common-sense:—to use only instruments by the most trustworthy makers; to enter in the note-book the *uncorrected* observations; and not to modify, even though it were apparently to improve, a scheme of observations once commenced.

Prof. Weiss treats of the zodiacal light, meteoric showers, refraction, and other "Phenomena which can be observed without the use of Instruments"; but his disquisition seems to us of rather needless length, and the more so because, as the author himself reminds us, "the circle of observations of this kind, which promise a profitable harvest to the traveller, is becoming continually smaller."

Concise, but thoroughly scientific, is the statement of the principles and practice of "Nautical Surveying," by Dr. Hoffmann. We cordially agree, amongst other things, with the dictum that instruments which serve a variety of purposes are always to be distrusted, and that, though they may be taken by land-travellers when considerations of space and weight are paramount, they are quite inexcusable on board ship.

In three succeeding chapters the subject of "Tidal Observations" is treated by Dr. Börgen, and the "Determination of the Amount of Water flowing in Rivers" by Von Lorenz-Liburnau; whilst a very useful general sketch, entitled "Hints for the Observation of the Intercourse of Nations," is contributed by Dr. Moritz Lindeman. A short, but interesting article, entitled "Some Oceanographic Problems," which, by a strange oversight, is omitted from the table of contents, has been written by Dr. O. Krümmel, whose work on the currents of the Falkland Islands is well known, and whose little text-book has been favourably noticed in these columns (NATURE, vol. xxxv. p. 6).

The editor's own contribution to the series is of a very modest character, and consists of a series of supplements to some of the chapters above referred to, showing under what limitations, and with what special precautions, such observations must be carried out on board ship. It is subdivided into two sections, the first of which is hydrographic in its scope, and handles some very interesting topics; for instance, the action of the wind upon the sea, the application of meteorology to navigation, materials carried by currents, and so forth. These are treated with great care and precision; and, as an instance of the way in which the whole is brought up to date, it may be mentioned that the Hon. Ralph Abercromby's invaluable photographic work upon clouds, though only published very shortly before the present volume, is duly noticed. The second subdivision is devoted to magnetic observations, and deals fully with the mode of making these on board ship, and also of determining the appropriate coefficients for their correction.

In the new edition this concludes the physical division of the work, for the chapter on "Earthquakes" has been omitted, on the ground that seismology has now developed into such a special and complex study that the traveller cannot be expected to undertake it. What is essential for him to know on this head has been incorporated in the chapter on "Geology."

The biological volume opens with an introductory essay by Dr. A. Meitzen, headed "General Topography, Political Geography, and Statistics," the bulk of which is

cast in a catechetical mould, furnishing useful hints to the traveller as to the best arrangement of his queries so as to elicit materials for a complete account of a country or district. Dr. A. Gärtner's chapter, on the anatomical, physiological, and medical investigations which may be undertaken by those practitioners who have the opportunity, is much to be commended for its conciseness and completeness. We should like to see it reprinted, and a separate copy placed in the hands of every military and naval surgeon. Agriculture receives the full share of treatment to which it is entitled. Prof. Orth, of Berlin, deals with the subject in general, with special reference to the composition of soils, whilst cultivated plants are separately handled by Dr. L. Wittmack.

The "Geographical Distribution of Plants" is discussed by Dr. O. Drude, upon the same limes as were adopted by Grisebach in the first edition of the work. Fifteen floral regions are recognized, grouped under six larger divisions, and the classification of plants, according to their general biological relationships, is reproduced with certain modifications. The "Geographical Distribution of the Sea-grasses" (which name is here restricted to the marine Phanerogams) is very fully set forth by Dr. Ascherson, all the species being enumerated and defined. The important topic of the modes of "Collecting and Preserving Plants of higher rank (Phanerogams)" has been committed to no less an authority than Dr. G. Schweinfurth. His mode of arranging the matter of his contribution in short numbered paragraphs makes it easy of reference, and the style is a model of terseness and perspicacity. The use of a portfolio in collecting is recommended, instead of the usual vasculum; and the relative merits of preservation in spirit, or by drying and pressing, are carefully considered, the preference being given (rightly, we think) to the former. The three sections just enumerated complete the botanical part of the work, which seems to us in its general arrangement the least satisfactory part of the whole. That such an insignificant group as the marine Phanerogams should have the same number of pages allotted to it as are given to all the rest of the higher plants, whilst the Cryptogams are entirely unnoticed, is hardly in accordance with the relative importance of these different classes of plants.

The editor of the volumes has not forgotten that "the proper study of mankind is man," for no less than four chapters, by as many different authors, treat of the investigation of the phenomena presented by the genus *Homo*. Dr. A. Bastian opens the series byan interesting dissertation upon "The General Scope of Ethnology," wherein a list is drawn up of eighteen different environmental factors, physical and biological, and a number of suggestive remarks are added upon each of them. The difficult subject of "Linguistic Inquiry" is next fully discussed by Dr. Steinthal, and that of "Numeration" by Dr. H. Schubert; whilst from the veteran pen of Prof. Rudolph Virchow, we have a masterly essay upon "Anthropology and Prehistoric Investigation," which is equally remarkable for its attention to practical details and its philosophic co-ordination of results.

The seven succeeding sections are devoted to instructions for the collection and preservation of various forms of animal life. The "Mammalia" have been undertaken by Dr. Hartmann. That this contribution is the work of an expert is obvious from numerous minutiæ, as, for example, when he warns the traveller, in a footnote, not to wear metal or mother-of-pearl buttons, or he will be continually annoyed by the natives begging for them. But though the matter is good, we cannot commend the style of this article: it is too verbose for a work of this character, and not only does the author perpetrate some choice examples of German prose composition, but his enthusiasm at times leads him into descriptive passages of tropical life and scenery. A welcome novelty in this edition is a chapter specially devoted to the "Cetacea," by Dr. H. Bolau, in which we notice with satisfaction that the desiderata of our museums are specially recorded. Dr. Hartlaub's treatment of the "Birds" is very full, and bristles with apt quotations from various authors: one, from the pages of Darwin's "Journal," might very appropriately stand at the head of the whole biological section of the present work: "It is better to send home a few things well preserved than a multitude in bad condition."

The section upon the "Collection of Reptiles, Batrachia, and Fishes," by Dr. Günther, of the British Museum, is quite a model of the way in which work of this kind should be done. The instructions are full and clear, but yet concise, and no extraneous matter is inserted. The "Mollusca," "Marine Invertebrata," and "Arthropoda" are communicated respectively by Dr. Ed. von Martens, Prof. Möbius, and Dr. Gerstaecker, whose names are a sufficient guarantee for the scientific value of the work they have undertaken. The practical nature of the articles, moreover, seems quite on a level with their zoological merit.

Dr. Gustav Fritsch completes the volume by a brief treatise upon two very important subjects—the microscope and photography. This latter has, owing to the perfection of the dry-plate methods, become so easy of practice that no scientific expedition is completely furnished without a photographic outfit; and it is satisfactory to see its various uses brought prominently forward in a work of this kind. Special attention may be called to the mode of recording the topographical features of a country by means of panoramic photographs taken from properly selected points of view, and to its peculiar advantages for the collection of anthropological data.

The editor is to be congratulated on the manner in which his task has been carried out. We have noticed a few, but not many, misprints uncorrected in the errata: "Du Petit-Thonars," for instance, appears in the same place in both editions. Compared with previous works of the same kind, this one is beyond comparison the fullest and most detailed: it contains, for example, about three times as many pages, and these more closely printed, than our own "Admiralty Manual of Scientific Inquiry," so that as regards quantity of information the two works are hardly to be placed in the same category. This, however, may not be altogether advantageous, since the German work, if read by an intending traveller, might not improbably deter him from any inquiries by seeming to exact too much. The English book might be termed a practical hand-book of the subject, whilst the German one is an encyclopædia. In our opinion it would best aid the objects it has in view by being published in the form of small separate works, and we should like to see it adapted into a series of such in the English language. W. E. H.

PLANT LIFE.

Pflanzenleben. Von Anton Kerner von Marilaun. Erster Band. Gestalt und Leben der Pflanze. (Leipzig, 1887.)

THIS is a book which deserves the warmest welcome from all lovers of plants. To give a general and at the same time a full and accurate survey of the natural history of plants, is at the present time a task of immense difficulty, and one which very few botanists could undertake with any hope of success. The task is daily becoming more difficult, as new additions are made to the already huge accumulation of facts, while its efficient performance is now a matter of more importance than ever, if botany is to be saved from becoming a close science, for specialists only. It may be said at once that the author has done his work with remarkable success. The book is a large one; only the first volume is before us, and this contains 734 large octavo pages. Yet it is scarcely an exaggeration to say that there is not a dull page from beginning to end of the bulky volume. On the other hand, inaccuracies are met with here and there, and some of these are serious, but the general excellence of the book is but little affected by these faults.

Before entering on a fuller account of the text, we must say a word about the illustrations, which are among the greatest merits of the book. In the text are 553 figures, many of which are pictures of great beauty. In addition to these there are twenty coloured plates, the first of which is histological, while all the rest represent various aspects of vegetation, both terrestrial and aquatic, in different parts of the world. Many of these plates are accompanied by an outline tracing of the individual plants shown, each figure on the tracing bearing a reference number, while the names are given below. This is an excellent plan (already frequently employed by French zoologists), and adds much to the practical value of the plates.

We will endeavour to give some idea of the plan of the work so far as it extends at present, but only a very cursory view will be possible. The present volume may be said to deal with general organography and physiology, especially of the vegetative organs. Comparatively little space is given to the organs of reproduction, which will no doubt receive full attention in the second volume, dealing with special morphology.

The introduction is headed, "The Investigation of the Vegetable World in Ancient and Modern Times." It contains a general view of the history of the science. This is very well done, and is calculated to rouse the interest of the reader.

Chapter I. is on the living substance of plants ("Das Lebendige in der Pflanze"), and may be described as an outline sketch of histology. The discovery of the cellular structure of plants by the naturalists of the seventeenth century is first narrated, and some of Nehemiah Grew's classical figures are here reproduced. Protoplasm and its movements next receive attention, and in this section there is some room for criticism. Thus the well-known motile granules of Closterium are wrongly described as being embedded in the protoplasm (p. 34), and the difficult question of the movements of Diatoms will scarcely find its solution in the theory here advocated, according to which Diatoms move in much the same way as mussels! In the following section the nucleus and chlorophyll-bodies are treated too much as if they were of the nature of secretions from the protoplasm, like oil-drops or crystals, whereas in all cases of which we have any definite knowledge they originate solely by the division of pre-existing bodies of the same kind. Probably, as regards the chlorophyll-corpuscles, the author wrote under the influence of the somewhat doubtful observations of Mikosch. The division of the nucleus is described by the author later on in the volume, but it would have been well to lay more stress on the process in this place. The remarks on the continuity of protoplasm through the cell-wall are acute and interesting, but it is rashly assumed, in opposition to the most trustworthy investigations on sieve-tubes, that this continuity exists from the first origin of the cell-wall. The author's attempt to identify the intercellular protoplasmic threads with the achromatin fibrils formed during cell-division is equally open to criticism.

The second chapter (pp. 51–246) deals with the absorption of food. This is a striking chapter, and presents in a very attractive manner a part of the science which is too often made to appear excessively dry. Attention may be called to one or two especially good sections, such as those on the nutrition of water-plants and of "stone-plants." The remarks on the correlation between the position of the leaves and the distribution of the roots, as affecting the water-supply of the plant, are of great interest, and are illustrated by excellent figures (pp. 85–92). Saprophytes, insectivorous plants, and parasites are all fully and vividly described. As regards the parasites especially, the account here given is the best general one with which we are acquainted, and the illustrations are as good as the text. Lathræa is reckoned among insectivorous plants as well as among parasites, and the author's peculiar theory as to the nutrition of this plant by means of alleged protoplasmic fibrils projecting from the surface of its glandular hairs, is again brought forward (p. 128). This view must now be regarded as more than doubtful. The section on the absorption of water (pp. 199–223), is perhaps the least satisfactory in the book. The most heterogeneous organs, extra-floral nectaries and chalk-glands among the rest, are classed, on the slightest possible grounds, among organs for the absorption of water, and thus their true functions come to be overlooked. It is difficult to understand how so forced and fanciful a theory can be maintained by any good observer. The short section on symbiosis is clear and satisfactory, and Frank's views on the vexed "Mycorrhiza" question are well put forward.

Chapter III. (pp. 247–343) is on the conduction of food. Root-pressure, transpiration, and the ascent of water through the wood are well discussed, Godlewski's views on the last-mentioned subject being provisionally adopted. The whole question is treated as clearly as is possible, in the present state of our knowledge, in a popular book. The detailed account of the structure of leaves as affecting transpiration is particularly good, and the illustrations here deserve the highest praise. The concluding sections of this chapter are concerned with the fall of the leaf, the relation of transpiring surface to water-conducting tissues, and the conduction of gaseous food-substances.

The subject of Chapter IV. (pp. 344–420) is the formation of organic substances from absorbed inorganic food. Assimilation (in the narrower sense) is fairly described, but the view taken of the action of light on the process has been, perhaps, too much influenced by Pringsheim's "screen-theory" of chlorophyll. Pages 380–393 contain an excellent series of figures to illustrate what is termed "leaf-mosaic," or the relation of the form to the arrangement of leaves, as insuring the exposure of the maximum surface to light. At the end of the chapter the adaptations by means of which assimilating leaves are protected against the attacks of animals are well described.

Chapter V. (pp. 421–475) treats of the metabolism and translocation of food-substances ("Wandlung und Wanderung der Stoffe"). The chapter begins with a few remarks on some of the characteristics of carbon compounds. The usefulness of such a very concise treatment of so vast a subject may be doubted, but the account appears to be good, so far as it goes.

The question of the first product of assimilation in green plants is clearly treated, and then the chief organic substances occurring in plants are described. Under the head of the translocation of food-substances, the structure of the phloëm and of laticiferous tissue is explained, and the anatomical anomalies of climbing plants are shortly described from this point of view. The figures given to illustrate the last-mentioned peculiarities of structure are, as so often happens in such cases, diagrammatic and unsatisfactory. The important subjects of respiration and fermentation are also included in this chapter, and the relation between these two processes is clearly brought out.

In the sixth chapter (pp. 476–544) the growth and construction of the plant are treated of. Under the former head we have an exposition of the mechanics of growth, and of the influence upon it of light and heat. The second part of the chapter includes an account of cell-formation. This section, unlike the rest of the book, seems to us insufficiently illustrated. Nuclear division is represented in a few figures taken from Guignard, but the subject is not treated with any completeness.

Chapter VII. (pp. 545–734), the last in the volume, is devoted to general organography ("Die Pflanzengestalten als vollendete Bauwerke.") The transition from unicellular plants to the most complex forms is first rapidly traced. Then we have sections dealing very fully with the modifications of the leaf, the stem, and the root respectively. In the first of these sections there is an especially good account of the cotyledons, and many interesting facts about germination are described. The section on leaves ends with a short account of the morphology of the flower. It is to be regretted that the author, after severely criticizing the artificial character of some former explanations of the morphology of the ovule, himself makes a laboured attempt to prove that the ovule is always homologous with a leaf or portion of a leaf (p. 603).

As regards the organography of the stem, special attention may be called to the excellent account of the stems of "lianes" (pp. 629–669), and to the clear explanation (founded on Schwendener) of the mechanical construction of upright stems. Here, however, as is usual in such explanations, the thickened stems of Dicotyledons scarcely receive their due share of attention. Under the heading "Hochblattstamm," the special forms of branching characteristic of inflorescences are explained.

The last section is on the construction of the root, and on its movements in response to external stimuli. Full justice is done to this very interesting subject, and the author is quite justified in emphasizing the unsatisfactory nature of those crudely mechanical explanations of these phenomena which are so often given in physiological treatises.

In the rapid survey we have taken it has been difficult to give a correct impression of the volume as a whole. It has been necessary to notice several defects, which have inevitably become more prominent in our review than they are in the book itself. The work is written throughout in a good clear style, and if the concluding portion fulfils the promise of the first volume, the treatise may certainly claim to rank as the best account of the vegetable kingdom, for general readers, which has yet been produced.

D. H. S.

PRACTICAL ELECTRICAL MEASUREMENTS.

Practical Electrical Measurements. By James Swinburne. (London: H. Alabaster, Gatehouse, and Co., 1888.)

THIS is a suggestive little book; and the pity is that the idea of the author, in writing the articles of which it is practically a reprint, has not been a great deal better carried out. The articles were evidently poor in style and excessively incomplete, even taken as newspaper articles; and, when put together in a consecutive form, the "nakedness of the land" becomes too painfully apparent. As it stands, the book consists mainly of remarks on almost every form of instrument known in electric lighting. It has no pretence to be a complete treatise, even of an elementary kind, on practical electrical measurements in general. Many of the most important branches of electric measurement are not even mentioned. What we do find is, partial descriptions of a multitude of instruments and machines, and a good deal of criticism, not always in good taste, and often pretty wide of the mark, of these instruments, and of the ideas of other "engineers."

The author commenced by setting himself the nearly impossible task of writing articles on electric measurement without the use of mathematical symbols. "The pedantic fashion," he says, "of dragging mathematical symbols into all electrical literature, and the respect commanded by an analytical investigation, even on false data, often lead writers to mar work otherwise good, by getting out of their mathematical depths, and writing nonsense to look learned." This, which is not unlike a good deal of the criticism throughout the book, sounds rather like putting on grandfather's spectacles to look sage; but, supposing that others do drag in more mathematical symbols than are absolutely necessary, it seems rather extreme to punish oneself by thrusting them aside altogether. To give really useful information as to the employment of electrical measuring instruments without quoting the formulas which are necessary in connection with them and with their errors and corrections seems to us to be leaving out the very crown of the whole; and as to the "respect commanded by an analytical investigation"

founded on false data, we trust it could never become a temptation to our present author.

But besides the difficulty of writing on electrical measurement without mathematics, there is, in our opinion, an attempt to catalogue and describe far too great a number of instruments and methods. In making this criticism, we cannot support it by mentioning names; but the author knows well, and everyone else knows, that many of the instruments and methods to which space is devoted are absolutely worthless; and it would be infinitely better to omit them, and thus both avoid confusion and save space, which might well be given to those that are of importance.

Altogether, the book requires re-writing, by which it could undoubtedly be made of very considerable value. The style is not good. With a sort of self-consciousness, Mr. Swinburne calls himself "the writer" throughout. Some of the criticisms—for example, that on the "B. A. Committee" (p. 22), and a remark on one of our most highly-valued scientific men (p. 110)—are altogether out of taste, coming from the pen of one who has his reputation still to win.

OUR BOOK SHELF.

Galileo and his Judges. By F. R. Wegg-Prosser. (London: Chapman and Hall, 1889.)

THIS work is a temperate discussion of the vexed question of the treatment of Galileo by the Pope and the Congregation of the Inquisition. The facts are not new; Mr. Prosser puts himself unreservedly in the hands of M. Henri de l'Epinois, whose article in *La Revue des Questions Historiques* is well known, and who has, Mr. Prosser says, gone to the trouble of consulting at first hand all the documents that could be found at the Vatican bearing on the subject. Mr. Prosser, in drawing his conclusions from the facts, adopts a kind of middle ground. He is a Catholic, and though he is too sensible a man to follow many of the Catholic writers in their conclusions, yet he seems to be shocked at the standpoint taken by a few of the Catholic writers who have condemned the treatment of Galileo. Thus he occupies a position between keen controversialists like the late Dr. Ward, on the one hand, who hold that not only did the Congregations act within their rights and their legitimate sphere, but that, looked at from the point of view of the early part of the seventeenth century, they acted wisely and prudently, and Catholic writers like Dr. Mivart, on the other hand, who assert (these are Mr. Prosser's words) "that the Church has no authority to interfere in matters relating to physical science, and that the issue in the Galileo case has proved the fallacy of her attempting to do so; that without entering into the discussion of what ought or what ought not to have been done in former times, we of the present generation have evidence sufficient to show us that scientific investigations should by right be free from the control of ecclesiastical authority." The first step taken by the Church against Galileo was in 1616, in censuring him for his teaching, and warning him of the consequences if he continued to teach the doctrines, first, that the sun was the centre of the universe, and therefore locally immovable; second, that the earth was not the centre of the world, and moved round itself diurnally. The first doctrine was declared by the Qualifiers—that is, the committee appointed from the Congregation of the Inquisition—to be foolish and absurd from a philosophical point of view, and heretical since it contradicted the meaning which had been given to certain passages of Scripture by the Church. Galileo promised to obey the warning, "*ut supra dictam opinionem . . . omnino relinquat, nec eam de cetero quovis modo doceat teneat aut defendat verbo aut scriptis.*" Mr. Prosser enters into a very long argument to show that this decree of 1616, though founded on reasons of doctrine, was merely disciplinary, and not given on a matter *de fide*, in which he is now and then rather casuistical. Galileo after this remained in peace till he was summoned to Rome to answer for the printing and publishing of his "Dialogue" in 1632. The heads of accusation are set out at length in the present work, but substantially they come to this, that he had disobeyed the order of 1616, and had continued to teach the same doctrines as those for which he was then reprimanded. It is impossible not to see that in summoning him to Rome the Pope was to some extent actuated by feelings of pique, for the fool of the "Dialogue," Simplicio, undoubtedly represents His Holiness. Mr. Prosser goes on to show that, having regard to the state of knowledge at the time, the Inquisition could have done nothing else but convict Galileo. The defence of the latter was threefold. In the first place, he said that Bellarmine had informed him that he might hold the Copernican doctrine as an hypothesis. This was undoubtedly the case; but it appears as something more than an hypothesis in the "Dialogue." Galileo answers to this that he had merely put the theory in the mouth of a speaker whose teachings were combated by the other speakers. Secondly, he maintained that he had not contravened the order given to him not to teach or expound that abominable doctrine in any way. This is hardly correct, as the "Dialogue" will show. Thirdly, he declared that he did not remember having been forbidden to teach it. But he could hardly have forgotten the terms of the order of 1616, which have been quoted above, nor the rebuke given him by Bellarmine by order of the Pope.

Observations on the Embryology of Insects and Arachnids. By Adam Todd Bruce, B.A. of Princeton College, Ph.D. of Johns Hopkins University. A Memorial Volume. (1887.)

THE subject-matter of this volume formed the thesis submitted by the author when he presented himself for the degree of Ph.D. at the Johns Hopkins University. After his lamented death, in 1887, the thesis was reprinted, exactly as he wrote it, as a memorial volume. He had made many additions to the work which is here recorded, but as the notes were unaccompanied by drawings it was impossible to make use of them. An account of the life and scientific work of the author is written by Prof. W. K. Brooks. The early death of Dr. Bruce, at the age of twenty-seven, prevented any very extensive amount of scientific research. It will, however, be clear to any reader of the careful and excellent work contained in this paper, that American biological science has lost an investigator of the very highest promise. Dr. Bruce had also very carefully studied, in conjunction with Prof. Brooks, the early stages of the development of Limulus, and it is much to be hoped that these results may be published at no distant date. A thorough study of the earliest stages of this most interesting form by so careful an embryologist would be extremely valuable. Prof. Brooks informs us that the work included "the segmentation of the egg, the formation of the blastoderm and of the germ-layers, and the anatomy of the young larva . . . illustrated by nearly a hundred drawings." I mention this in the hope that some means of publication may be found in this country, if the claims upon the space of the *American Journal of Morphology* are too great to admit of the appearance of a paper on so important a subject in what appears to be its appropriate position.

The volume contains an attempt to settle the most difficult questions concerning the earlier stages of the development of spiders, Lepidoptera, Coleoptera, and Orthoptera, while less complete observations were made

upon Neuroptera and Diptera. Among insects, the Lepidoptera were studied with especial care, the type selected being *Thyridopteryx ephemeraiformis*. The careful account of this embryology, together with the numerous excellent figures, entirely substantiate the author's claim that the study of this type, "if it has brought to light nothing new, has, in the opinion of the writer at least, settled some important points connected with the embryology of this group of insects." In the account of segmentation and the formation of the blastoderm, the author completely confirms Bobretzky's descriptions. The development of *Thyridopteryx* occupies twelve quarto pages: for the details the original must be consulted. The account of the embryology of Orthoptera, represented by *Mantis* and the grasshopper, and of the embryology of spiders, is also very complete.

At the end of the paper many interesting and suggestive conclusions are appended. Among these it is significant that a writer who has done so much work upon the early stages of Limulus should unhesitatingly regard this latter form as an Arachnid. The Trilobites he considers as "possibly the ancestral form of Limulus."

Only a short account of this excellent paper has been given here. All those interested in embryology, and the light shed by it upon morphological science, will, of course, make a careful study of this work. E. B. P.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Satellite of Procyon.

IT is well-known that Procyon, like Sirius, does not travel through space in a straight line, its motion undergoing changes similar to those which would result from the disturbing action of a large satellite. This explanation was advanced by Bessel in 1844, and though the satellite has not yet been *seen*, its existence has been placed beyond reasonable doubt by Dr. Auwers's investigations on the subject.

Does it not seem probable that this interesting body may be revealed by the aid of photography? It is now possible to photograph stars and nebulæ which are beyond the visual range of the most powerful telescopes; and if the companion of Procyon, like that of Sirius, be self-luminous, there would seem to be a good prospect of obtaining its image on the sensitive plate.

As the companion is doubtless in pretty close proximity to its brilliant primary, it will be necessary, for photographic purposes, to intercept the image of the latter by means of a suitable screen. Since the *direction* of the satellite at any time can be found from Dr. Auwers's elements,[1] there would be no uncertainty as to the position in which this eclipsing disk (or wire) ought to be placed, though its proper adjustment would be a somewhat delicate operation. Should the satellite be photographed, its position will become known from its configuration with respect to other stars recorded on the negative.

If a very large telescope were employed, the images of both components, as distinct and separate dots, might be obtained on the plate. For Procyon, a very short exposure would be requisite, and this could be secured by the use of a movable stop or screen, similar to that devised by Prof. Pritchard, and used for parallax work at the Oxford University Observatory.

It is scarcely necessary to dilate upon the interest which would attach to a photograph showing Procyon's companion. As the parallax of Procyon has been satisfactorily determined by Dr. Elkin and others—being 0″·266 according to the Yale College observations—we could ascertain the *actual* as well as the *relative* masses of the two components. And the brightness, or more strictly speaking the photographic magnitude, of the satellite might also be determined with some precision.

[1] At present the position-angle of the satellite is *about* 233°. Its *distance* probably amounts to but a few seconds of arc, and may be within 2″.

It may not be too late to obtain such results during the present season, but exposures of four or five hours, under good conditions, will not be practicable before next winter.

J. M. BARR.

St. Catharine's, Ontario, Canada, March 4.

"Les Tremblements de Terre."

THE issue of NATURE for February 7 (p. 337) contains a review of the little work on earthquakes published for me by Messrs. J. B. Baillière. The anonymous author of the article makes several criticisms on my book to which I desire to reply.

Your critic thinks it a grave fault not to have entered into a detailed description of the seismographs and seismometers at present in use. He reproaches me in particular for having but just mentioned Prof. Ewing's duplex pendulum seismograph; for having omitted to speak of the same inventor's horizontal pendulum seismograph; and especially for seeming to ignore the experiments made with these instruments by Prof. Ewing in Japan. I confess that I had not been struck by the excellence of the instruments in question, and that it was not through an oversight that I omitted to describe Prof. Ewing's observations in Japan, while I quoted in detail those of his *confrères*, Messrs. Milne and Gray, in the same country.

Your critic defends with some acerbity a certain class of seismographs, and wrongly accuses me of failing to appreciate the principles on which their construction is based. The objections which he makes to my treatment of M. Cavalleri's pendulums of unequal length are entirely refuted from a theoretical point of view by the learned note due to M. Poincaré, which is inserted on p. 46 of my book. I need not insist further on this point.

Your critic thinks I have not done sufficient justice to the work of the Italian *savants:* he forgets the limits necessarily imposed on a book destined especially to give to the general public an idea of the present state of an important question.

The writer of the article regards the seismographs of to-day as perfectly sufficient for all scientific needs. I am far from being the only person engaged in the study of earthquakes who does not share this opinion. Finally, he describes, and not very clearly, the experiments which I made with M. Michel Lévy to measure the rate of propagation of disturbances through the soil, and the registering apparatus designed for this purpose. According to the writer, these experiments constitute the only advances we have made in the study of earthquakes. If he had rendered justice to our work on the subject, we should have been content, and I should have raised no objections to his article. But your critic reproaches us with having given results which are masked by inevitable causes of error. A more careful study of the book, and especially of the extracts from our original memoir, published in the *Comptes rendus* for 1885 and 1886, would have preserved him from so inexact an assertion. In fact, he unjustifiably mixes up the *preliminary* experiments, made at Le Creusot by means of an apparatus exactly similar to that used by Mallet and Abbott,[1] with what really constitutes the basis of our work—I mean the determinations made with the aid of photographic registration and explosives. What your critic calls the "personal equation" of the instrument is here nearly negligible[2]; and the merit of our method lies precisely in this point, which distinguishes us from our predecessors.

We would beg the readers of NATURE to verify for themselves the truth of our statement. This verification will enable them to judge of the value of the article laid before them.

F. FOUQUÉ.

Finding Factors.

IT may add interest to Mr. Busk's ingenious method of distinguishing between prime and composite numbers to state the algebraic basis on which it rests.

Let N be any number, and n^2 the next higher square number, and let $N = n^2 - r_0 = (n+1)^2 - r_1 = (n+2)^2 - r_2 =$ &c. r_1, r_2 are formed successively from r_0 by the successive additions of $2n+1$, $2n+3$, . . . the increments being in arithmetic progression, so that $r_m = r_0 + 2mn + m^2$. As soon as r_m becomes a square, N is expressed as the difference of two squares, and its factors are found.

[1] See p. 219 *et seq.*

[2] The only subsisting cause of error is that due to the inertia of the mercury, which we have estimated and introduced into all our calculations (see p. 246).

If N is prime, it is expressible as the difference of two squares in only one way, viz. $\{\frac{1}{2}(N + 1)\}^2 - \{\frac{1}{2}(N - 1)\}^2$. To prove that N is prime by this method, the number of additions required is $\frac{1}{2}(N + 1) - n$, which is $\frac{1}{2}\{(n - 1)^2 - r_0\}$.

It may be noticed that when $n + m$ and r_m have a common measure, it must be a factor of N, and the additions need be continued no further.

For example, $N = 8131$, $n^2 = 8281$.

$$\begin{array}{rr}
n = 91 & r_0 = 150 \\
 & 183 \\
\cline{2-2}
n + 1 = 92 & r_1 = 133 \\
 & 185 \\
\cline{2-2}
n + 2 = 93 & r_2 = 518 \\
 & 187 \\
\cline{2-2}
n + 3 = 94 & r_3 = 705
\end{array}$$

94 and 705 have a common measure, 47; therefore 8131 is is divisible by 47, and the other factor is then found to be 173.

Mr. Busk's method of shortening, exemplified on p. 414 by his proof that $73 = 37^2 - 36^2$, depends upon the following:

Let $r_0 + 2mn + m^2 = (k + m)^2$, then $m = \frac{1}{2}(k^2 - r_0)/(n - k)$: since $k^2 - r_0$ is even, k is even or odd according as r_0 is even or odd; it is necessary only to try values of k descending by differences of 2; the greatest possible number of operations is $\frac{1}{2}(n - 1 - k_0)$, when k_0 is the value of k, with which we begin.

The process may conveniently be arranged as in the following example:

Let $N = 6667$, $n^2 = 6724 = 82^2$, $r_0 = 57$.

k		$82 - k$		$\frac{1}{2}(k^2 - 57)$		Quotient.
15	...	67	...	84	...	a fraction
				32		
17	...	65	...	116	...	,,
				36		
19	...	63	...	152	...	,,
				40		
21	...	61	...	192	...	,,
				44		
23	...	59	...	236	...	4

therefore $6667 = (82 + 4)^2 - (23 + 4)^2 = 113 \times 59$.

If N is composite, this method is not always shorter than the former. It will be shorter whenever $2m > k - k_0$, but it is not easy to see how to determine *a priori* whether this is the case.

The method by decreasing squares is not one of general application. For instance, the factors of 323,171 cannot so be found. It is the difference of two squares, each more than ten times as large as the first square used.

W. H. H. HUDSON.

King's College, London, March 15.

Dolomedes fimbriatus, Clerck, at Killarney.

IT may interest some of your readers to know that this rare and fine aquatic spider occurs on Cromaglaun Mountain, near Killarney Lakes. I first found it when collecting the little shell, *Limnæus involutus*, and though I had it two or three times in my hand, it was so active that it escaped, and I, not knowing its powers of diving, never thought of looking for it under water. The following year I again visited the little lake, which is called Crincaum, with some friends, and this time we fairly captured the spider, which I then easily identified as *Dolomedes fimbriatus*. There is a good account of it in Blackwall's "British Spiders," and also in Andrew Murray's "Economic Entomology—*Aptera*," but I am not aware that it had been observed in Ireland before I found it.

A. G. MORE.

March 18.

BEECH-WOOD.

IT is so characteristic of the science of to-day to find specialists narrowing their field of research, and confining their investigations to a deep narrow channel, that no surprise can be felt that two able men should devote their energies for two years to the examination of the biology and chemistry of the wood of a single tree. It is not so easy to avoid astonishment at the results of the two years' work, however, appearing as they do in the form of a large book[1] of 238 pages of close description and argument, interspersed with long tables of figures, abounding in interesting information when properly read.

The authors have divided their work very fairly, the botanist having set himself the task of elucidating in detail the histology of the wood, the distribution of water, starch, and other contents, the formation of annual rings, and the growth in thickness of the trunk, and a number of other problems throwing light on the growth of the beech in the forest; while the chemist has confined himself to the task of analyzing the timber, so as to discover (1) the quantities of total ash, water, nitrogen, &c., in different parts of the tree; (2) the percentage composition of the ash, and the manner of distribution of the individual constituents; (3) the absolute quantities of each ash-constituent in 1000 parts, and other chosen quantities of dry substance of the wood; (4) the annual in-take and out-put of these constituents on a hectare of beech forest; and (5) similar particulars for the nitrogenous constituents.

The authors have by no means spared their trees. It is enough to make one envious to read of the trees cut down at all ages from 15 to 150 years, and of the specimens selected at all heights from each; how the research was extended to good, bad, and indifferent soils, and how trees in shade and in the open, trees entire and trees pruned, &c., were all laid under contribution as required. More than 100 stems of all ages were thus employed.

The manner of utilizing this enormous mass of material is worth noticing, for every kind of determination was made that would yield practical information.

The height of the trees was found, as the best indication of the value of the situation; the number of stems on a given area, their surface, contents, &c., were also determined; the age of the trees, their physiological condition, &c., were all considered in due course. The selected stems were then cut up as follows: transverse disks were cut at the successive heights of 1·3, 5·5, 10·7, 15·9, 21·1, and 26·3 metres, and separate determinations made of the specific gravity, histological peculiarities, analysis, &c., and these not only for wood and cortex separately, but also for each 30 annual rings of the stem. The thickness, density, &c., of the annual rings were also tabulated, and attention paid to north, south, east, and west sides of the stem.

Not only are all these data given in detail in the tables, but other tables are provided showing the mean densities, cubic contents, &c., &c., of whole trees, or of the trees on given areas; and the patient compilation and ingenious methods here displayed reflect the greatest credit on the authors. It is, in fact, especially in the application of their measurements, &c., to the forest as a whole that the tables will find their greatest practical value. There is also much of more abstract scientific interest to be learnt from the results.

On examining the histology of the wood, several new facts were discovered. The curious dipping in of the annual rings where they cross the broader medullary rays, and the deposits of grains of calcium carbonate on the septa of the vessels, may be mentioned by the way; but the most important results are those relating to the length of the elements, the lumina of the vessels, and the relative numbers and distribution of the latter on a square millimetre of transverse section.

The wood of the beech consists of the usual elements—vessels, tracheides, libriform fibres, and wood parenchyma, with transitional elements difficult to classify under any one of these heads. As was long ago pointed out by

[1] "Das Holz der Rothbuche," by Profs. R. Hartig and R. Weber. (Berlin: Springer, 1888.)

Theodore Hartig, Sanio, and others, the length and breadth of the various elements differ in different parts of certain trees. Prof. R. Hartig has now worked out this subject in the beech for the first time, giving long lists of measurements at various heights, ages, &c., as before.

The recent vessel-segments, tracheides, and fibres in a five-year-old beech-tree are only half as long as those in a tree 120 years old, and this occurs in what at first sight appears a very curious and inexplicable manner.

The length of these organs at first rapidly increases, until the tree is about 60 years old; then they either no longer show increase in length, or do so very slowly, till the tree is about 120 years old. They then exhibit their maximum length. Henceforward the elements formed are shorter each year, and much so if the tree is growing free in the open.

Moreover, in the same tree, the longest elements occur at the base of the trunk, and shorter and shorter ones occur up to a height of about 5·5 metres; then the tracheides and vessel-segments are found to be longer again, until the height of 15·9 metres is attained. The lengths are much less in the crown. The libriform fibres decrease regularly in length all the way up.

Hence, put generally, the elements are short in young trees and in the upper (*i.e.* youngest) parts of older ones; their lengths increase afterwards year by year, but after 120 years only shorter and shorter elements are again found.

The lumina of the vessels also vary with the age of the tree and with the height of the part. Taking, for example, the vessels at a height of 1·3 metres, the average diameter is 0·05 millimetre during the first 30 years, but between 30 and 60 years they are larger (0·064 millimetre), and maintain this average afterwards to the end of the life of the tree. Still more striking are the changes at different heights in the tree: both in very young and in very old trees the vessels in the crown may be very narrow indeed compared with those elsewhere.

As facts of great importance in its bearing on the question of the specific gravity of wood, and the futility of comparing rough weighings, we may select the following. The three elements—vessels, tracheides, and libriform fibres—are distributed very differently on the transverse section of the annual rings according to the age of the tree and the level of the section. The rule is that, at the same level, the number of vessels per square millimetre increases as the tree ages. When it is shown that the numbers may range between about 60 to 80 at 30 years, and 200 to 220 at 100 years or more, the conviction arises that the question of specific gravity may be complicated by many factors.

As regards the level of the section examined, the rule is that the number of vessels per square millimetre increases as we go upwards. But it is found that the number of vessels in any one annual ring remains about the same: it is differences in the breadth of the rings which cause the close packing or otherwise, and the general tendency of the rings to be narrower upwards explains the above.

With respect to tracheides and fibres, it may be said generally that young trees form few tracheides (and chiefly near the vessels) but more are formed later; but again, in old age, in the open, the tracheides are replaced by fibres.

Some interesting observations follow on the micro-chemistry of the wood: vanillin and coniferin occur in the walls of the wood elements, and it is somewhat remarkable that they should show a cellulose reaction quite late. Relatively small quantities of tannin are found in the cells, and drops of "wood-gum" are abundant. It is interesting to note the infiltration of the walls with tannin, and this gives the deeper colour to wood exposed to air, owing to oxidation.

The dark (false) heart of the beech is not due to the presence of much tannin, and Hartig again insists that this wood cannot be divided into heart-wood proper as distinguished from sap-wood. The false heart is a pathological production, and nearly always contains Fungi.

But perhaps the most interesting facts in this connection are those bearing on the starch-grains and their movements.

In an old beech-tree, the quantity of starch diminishes from the periphery to the centre: little or none is found within the last 50 annual rings. In the winter the outer rings will be crowded with starch, every cell of the wood-parenchyma and medullary rays being full.

It is, of course, impossible to go into the details of Hartig's experiments and measurements, but he found that under ordinary circumstances the main mass of stored-up starch does not move at all: contrary to the received opinion that the starch is all, or nearly all, dissolved in early summer, and stored up again in autumn, the astonishing fact comes out that during the development of the current year's annual ring, the cambium only takes starch from the next inner ring (and sometimes the next but one) in June and July, and that before the middle of September it is all restored.

In other words, only the two preceding annual rings yield starch to start the cambium: the completion of the new ring, its stores of starch, and the restoration of the borrowed starch, are at the expense of the work of the leaves of the current year.

Light is thrown on the subject by the following experiment—an admirable instance of the progress which is being made in the study of the physiology of plants. Two trees were completely deprived of branches and leaves, and then allowed to stand otherwise untouched: one was felled at the end of twelve months, the other at the end of two years. In both cases it was found that during the first year after the mutilation a new ring was formed by the cambium, but the mass of wood in this was only about 5 per cent. of the normal increment which would have occurred if the tree had remained intact: no trace of further increment was observable in tree No. 2 during the second year.

This 5 per cent. increment was at the expense of all the starch stored in the medullary rays and wood-parenchyma of the stem; in other words, the quantity of starch held stored in each of these trees was equivalent to the quantity of woody substance in a ring containing 5 per cent. of the normal annual amount: in other experiments the amount rose to 15 per cent. or more, but never approached that of a complete normal ring. It is noteworthy that the cambium only acquires the power to attract the whole of this stored starch under such special conditions of hunger as are induced by stopping its supplies from the leaves.

Some similar experiments, with modifications in the special cases, led to the result that the starch which comes down from the leaves—even when only sufficient to partly fill one layer of wood—is rapidly distributed *over the whole sheet of wood*, both above and below.

The question, What are the stores of starch for, if not to feed the cambium? is answered by the following. Weber's analyses show that the nitrogenous substances decrease from without inwards in the wood, just as does the starch in a normal tree; but the total proteid substances remain practically unaltered (at least they suffer no diminution) because they are not used up in building the cell-walls. Any drain on the proteids by the cambium seems to be paid back in due course by the travelling of the proteids from cell to cell.

Now it is a well-known fact that the beech, like other similar forest trees, only yields seed after attaining an age of 50 to 60 years, and that what are termed good seed-years are separated by considerable pauses. It is also

well known that the production of fruit and seed "exhausts" the plant: in the case of annuals it completely drains their resources, and every apple-grower knows that the trees need "rest" after a good crop. In view of all the facts, then, it is most probable[1] that the stores of starch in the beech are put up in reserve for the enormous drain which the "seed-year" will involve, and we shall see that this idea is fully borne out by the chemical analyses, which show that certain valuable minerals are similarly stored for the seeds.

But this does not fully explain why the stores diminish inwards. Two causes are adduced for this. In the first place, a seed-year having exhausted nearly all the supply of starch, we have seen that succeeding deposits only occur in the outermost rings of wood, and so there is no restoration of the deposits deeper in the tree; secondly, some of the stored starch in the deeper layers gradually undergoes change into the drops of "wood-gum" (*Holzgummi*), of which mention was made above.

Some "practical results" of the above may now be noted, the most important being that the difference in weight between wood felled in summer and that felled in winter is, in effect, *nil*, contradicting a wide-spread assumption, and confirming a doubt which Nördlinger had already put forward. It thus follows that the want of durability in summer wood depends on other causes, and Hartig considers it due to the fact that winter wood has time to dry on the outside before the atmospheric influences are favourable for the development of Fungi, the spores of which are always about, but dormant in the cold of winter. No doubt there are other factors to be considered also, but the importance of the above has been too much overlooked or under-estimated.

Another interesting section of the work is that dealing with the formation of the annual rings. By cutting disks at equal distances apart on simultaneously-felled trees of 50, 100, and 150 years old, and measuring the breadth, &c., of the rings at eight points round the disks, some further discoveries were made. Generally put, it was found that (in the case of beeches near Munich, at any rate) the annual ring commences to form at about the end of May, the tree being already in full leaf; by the middle of June the ring is one-third its normal breadth, and is half finished early in July, attaining its normal complete state before the end of August. Hence the whole period of the activity of the cambium only amounts to about two months and a half.

As regards the parts of the tree, it is found that the active division of the cambium commences first in the twigs and small branches; it is later in the trunk proper, and begins at different parts, according to circumstances.

In the oldest trees (150 years) the cambium was found in an active state at 3 to 4 feet up, while parts above and below were still dormant; whereas in somewhat younger trees the process of ring-formation began simultaneously all up and down the trunk. In still younger trees the cambium was found to awaken first in the higher parts of the trunk. More investigation is still needed here, however, before several dark points can be regarded as explained.

Some generalizations as regards the growth in thickness of the beech deserve notice. In the crown, the annual increment—*i.e.* the quantity of wood produced by the cambium during one period of its activity—increases more or less rapidly as we proceed from the tips of the branches to their point of origin from the trunk; but this is by no means the case in the trunk itself, and several cases have to be considered.

In those trees which, owing to close crowding in the forest, have developed only feeble crowns, the annual increment is greatest just beneath the crown, and diminishes regularly downwards; and in very closely crowded trees the cambium in the lowermost parts of the stem may even *stop dividing altogether:* in such cases the ordinary mode of ascertaining the age of the tree would yield false results, for the number of annual rings at 3 to 4 feet high is less than the number of years of the tree's life. The physiological meaning of the above is, that the small leaf-area does not supply sufficient food-material to provide for the needs of the whole sheet of cambium, and the upper parts take all that is sent down, leaving none for those below.

[1] Hartig has since proved that this explanation is the correct one (*Botanische Zeitung*, December 28, 1888, p. 837).

In those trees which have well-developed leafy crowns, more exposed to light and air, the annual increment follows a rule exactly the converse of the last—the amount of wood formed per annum is greater as we proceed from the upper part of the stem to the lower. If we leave out of account the lowermost 6 to 12 feet, every gradation can be found, and in rare cases the breadth of the annual ring may be constant from above downwards.

Now comes in a remarkable discovery. If such trees as the above are suddenly exposed to full light and air, &c., by cutting down the neighbouring trees, the annual rings *in the lower parts of the stem* suddenly become much broader: no such stimulation of the increment occurs in the upper parts.

Now as to the explanation of these remarkable phenomena. There can be no reasonable doubt that the precedence shown by the upper parts of crowded trees is due to the rapid warming which they receive from the air in the spring sunshine: the lower parts of such trees, however, have to wait until the water which they absorb from the soil raises their temperature to the minimum cardinal point, and by the time the water of the soil is sufficiently warm for this, the cambium in the upper parts is far ahead, and working under such favourable circumstances that the rings maintain their greater breadth to the end. But the chief factor in the process is that the upper cambium gets the first supplies of food-substances, and in larger quantities, because lower down the diminished supplies have to spread over a larger area.

In the case of trees exposed freely to the light and air, the sun's rays warm the thinly covered soil (and its water) around the roots, and so the cambium is enabled to recommence its annual work pretty nearly at the same time over the whole stem: in this case thicker rings in the upper parts of the stem must be due to the nutrition being more abundant. All this still fails to explain the *sudden* stimulus to the annual rings in the lowermost parts of suddenly isolated trees, and Hartig suggests that the probable cause is an increased supply of potassium salts and phosphates, rendered available at the roots. This of course implies the further assumption that such minerals are employed directly, and however probable this may be, it is by no means proved.

The removal of branches from the tree leads to the same results as crowding, *i.e.* the rings formed below are thinner, because the supplies are not sufficient to feed the sheet of cambium equally from above downwards. Moreover, the complementary case may occur: a tree in the open may have *too many leaves*, as is proved by the fact that it may be pruned without any loss of increment. The leaf-area of a tree is by no means always proportional to the supplies of food-materials from the soil: it may be too large or too small to be working economically, or so large that each leaf is sluggish—lazy, so to speak, and not doing anything like the amount of work it is capable of. Not only is this idea interesting and suggestive in itself, but it has important bearings on the question of the thinning and treatment of forests generally.

We must leave this topic, however, and pass to one of a different nature, but no less scientifically important. This is the weight of the wood. Although certain practical ends can be roughly attained by merely weighing equal-sized blocks of any particular kind of timber, at any time or in any state, it is, nevertheless, easy to see that such

weighings are of little or no scientific value: only the weight of the fresh timber immediately it is felled, and the absolute dry weight (after exposure to 105° C. long enough to drive off all moisture) yield results of really scientific value.

If we regard 1 cubic metre as the unit of volume, we may obtain some useful factors by ascertaining the weight of dry woody substance in such a volume, from different parts of the tree, and from trees grown under different conditions, &c. The amount of water driven off, *i.e.* the difference between the fresh weight and the absolute dry weight, is found to vary much, and Hartig some time ago obtained most valuable results, bearing on the difficult question of the ascent of water in tall trees, by comparisons of this kind. Moreover, the real test of quality of wood—its value as fuel, and other technical properties—is given in the absolute dry weight.

Passing over the methods, and other details, it may next be pointed out that the weight of a given volume of wood depends chiefly on the sizes and distribution of the histological elements—vessels, tracheides, fibres, &c.—and in the case of beech-wood, it is especially the sizes and numbers of the vessels that have to be taken into account, and as these stand in direct relation with the magnitude of transpiration, it is clear that the quality of the timber as estimated by its weight depends on the quantity of leaves.

Neglecting the roots, we may regard the tree as consisting of three parts: the stock, the shaft, and the crown. Now, the root-stock and the crown contain wood of the best quality, and some curious results come out on examining why this is.

As is well known, the base of the tree widens at the origins of the main roots, and here the annual rings are broadest: if we bear in mind that the number of vessels in each annual ring remains constant, it is easy to understand why the wood is better—it is simply that the vessels are dispersed over a larger sectional area, and are separated by more numerous fibres, the elements which give solidity to the wood.

We have seen that in the trunk of a tree with a large crown of leaves, the mass increment increases from above downwards: this means that the same number of vessels (per annual ring) are distributed over a smaller sectional area above. In a given case, on 1 square millimetre of area, there were 115 vessels at a height of 1·3 metre, but at 10·7 metres height there were 175 vessels on the same area; hence the latter was lighter and worse wood.

By thus counting the number of vessels per square millimetre, and taking the average size of the main vessels, it was possible to get an expression of the relative area occupied by the lumina, and that of the rest of the annual ring; of course this is only approximate.

It comes out that, in trees with large crowns, while the number of vessels is the same at all heights in the stem, the number of vessels *per square millimetre* is much fewer below than above.

In the crown of the tree, however, things are very different; the number of vessels in each annual ring rapidly diminishes, because at each branching a number are given off. Thus, where 200,000 vessels were found in an annual ring in the stem, the same in the crown gave only 57,750. This alone would explain the better quality of the wood, but the number of vessels per square millimetre is also found to *increase* in the crown, and this means corresponding depreciation. But the most important factor in explaining the superior hardness, &c., of the wood in the branches is that the average size of the vessels is less, and therefore the area of lumina in the cross-section is reduced.

Physiologically, the reduction in the lumina of the vessels is in relation with the decrease in the volume of water-current as we ascend, and several facts point to the constancy of this relation. It is well known that, if the soil around a tree is suddenly deprived of much of its water, the tips of the tree die off first: "stag-headed" trees are often produced by over-exposure. This is because the average size of the vessels has been adapted for a richer supply of water than comes to them under the new conditions. Hartig says that the average size of the vessels throughout is reduced if the land is deprived of cover, and the tree exposed too much.

As has been seen, the wood of trees below 60 years of age contains fewer vessels, and these with smaller lumina, than afterwards. It is also known that the ascending water-current is confined to the younger outer wood, or alburnum; and if we neglect younger trees, it seems that in the beech it is only the 20 or 30 outer annual rings which conduct the water.

Now the authors of the book referred to find an unexpected relation between the amount of wood produced annually, and the current of water passing up the stem. By an ingenious series of measurements and calculations, it results that much more room is provided for the water-flow in early years than in old age. Thus, a given amount of water, which has for its passage in a tree 30 years old an area of wood expressed by the number 4·04, has only an area equal to 1·64 at 140 years of age. Hence, in order to conduct the larger quantities of water which must pass to the larger crown, the smaller area of wood, in the older tree, has to *increase the number and size of its vessels*, and so the wood is lighter and poorer.

It is impossible here to enter into the bearing of these matters on questions of forest management; it is only a particular case of the dependence of technical forestry throughout on the teachings of science, the principles of which it applies.

An interesting experiment may be quoted. Two beeches 150 years old were felled and examined; they had been completely freed from neighbouring trees 7 years previously. The effect of the sudden exposure to free light, air, &c., was that the mass increment rose to 2·4 times greater than previously, and the weight of the wood formed during the 7 years of exposure was 700 kilogrammes per unit volume, as against 600 kilogrammes previously, *i.e.* 16·7 per cent. more wood-substance was formed. On going into details, it was found that *five times as much wood-substance* was formed each year, and *twice as many vessels* were developed in each annual ring. But since these twice as many vessels were distributed over five times the quantity of wood, the wood was still heavier than that of 7 years previously. On the square millimetre there were 63 vessels, as contrasted with 140.

The reason that letting in the light and air around the tree has such enormous effects is obvious enough to the physiological botanist, but it should also be clear that the knowledge thus obtained is the best guide to such forest practices as thinning and freeing timber: into these matters, however, we do not propose to enter further here, but must pass to other matters. In the section on the course of growth of the beech, an interesting discussion on the limits of height of trees occurs: Hartig regards the chief limiting cause to be the gradual disappearance of the difference in tension between the air-bubbles in contiguous elements: the osmotic forces remain constant throughout, but the lifting power diminishes with age and height, until it ceases to suffice for movement. The influences of etiolation, and judicious crowding, and other devices for timber-growing, are then discussed in the light of what has been already said, and with the aid of numerous tables of close-set and well-classified figures, sufficient illustrations are given to satisfy the most stiff-necked critic of the value of these results.

The chemist's results, however dry they may appear from the tables and curve-diagrams, allow of summary in a way that endows them with an interest to the general reader, no less real than that which attaches to other parts of the work. Methods may be passed over here.

The cortex of course contains most ash, and the quantity of total ash increases with age and with height: these facts have been shown for other trees also.

In the wood proper, the quantity of ash *as a whole* increases from the periphery to the centre, but as we shall see that the distribution of the various constituents is very different in different parts, this generalization will have to be cut up into a series of less general statements. In the same period of growth the total ash increases with the height.

It is somewhat striking that the inner zones of the inner alburnum yield most ash, and thus the central part of the highest transverse section of the stem will contain most ash.

As regards the changes due to age, the ash per cent. decreases till the tree is about 60 years old, and then it increases rapidly for twenty years or so, gradually diminishing again with increasing age. These periods show such close relation to certain facts in the culture of the trees, that they are evidently explained somewhat as follows. During the first 60 years in the plantation, the young beeches crowd one another more and more, and the competing roots restrict one another, and the percentage amount of salts absorbed diminishes year by year: at or about the age of 60 years the trees are thinned by systematic felling, and so more space is given to those which remain, as well as more soil and ingredients from the decomposition of the roots, &c., of the felled trees. The consequence is the increase of ash to a first maximum. At the period about 80 to 90 years the beech has attained the seed-bearing age, and the probability that the diminution of ash henceforth is due to the drain to supply the seeds is too great to be overlooked.

It is interesting to note that shaded beeches, at all periods and in all parts, show a higher percentage of total ash than fully exposed trees, and the same is true of the silver fir (another tree which bears much shading): the trees store up mineral substan es, which must be an advantage to them under the circumstances of growth.

If, instead of regarding the total ash, we now look at the constituents, it results that the enormous excess of ash in the cortex consists chiefly of calcium carbonate, from the calcium oxalate (which may form 70 to 90 per cent. of the whole). Much potash, magnesia, and phosphoric acid also occur.

In the wood, the quantity of potassium salts increases from the periphery to the centre; whereas the reverse is the case with the phosphoric acid, sulphur, and magnesia, a fact the more remarkable because the potash usually accompanies the phosphoric acid in other parts of plants —*e.g.* in leaves, &c. It is no accident, however, and the fact comes out that the beech forms large reserve stores of potash (this being the chief cause of the large increase of total ash in the interior of the stem), whereas the phosphoric acid and sulphur travel outwards with the proteids, being repeatedly used in metabolism in the cambium, &c.

We must pass over a number of other peculiarities of the distribution of the ash-constituents, to notice the effect of the age of the tree on the chief salts. The distribution of the potash, lime, and magnesia is little influenced by age, but an extraordinary effect comes out in the case of the phosphoric acid. The young tree starts with a relatively large quantity of this constituent, but the amount sinks year by year till the fiftieth or sixtieth year, and then rises again to about the ninetieth year, to fall afterwards: in fact, the behaviour is similar to what occurs with the total ash, and is doubtless to be referred to the same causes.

Another curious result comes out in studying shaded trees: whereas they take up as much potash and lime as exposed trees, their magnesia and phosphoric acid fall far below those of exposed trees. But the most astonishing discovery is that shaded trees *take up four times as much sulphur* as exposed ones. The analyst himself notes how astounding this is, but he insists that a second series of analyses gave confirmatory results.

Another queer fact is that the kind of soil exerts little influence on the analyses; though a similar conclusion has been come to with other plants.

The study of the absolute quantities of individual ash-constituents in 1000 parts of the dry substance brings out some interesting and important generalizations, which are expressed in the form of curves, and fully bear out in detail what has already been stated.

The quantity of ash and of each ash-constitutent in 1 cubic metre of beech-wood at various ages, as compared with the wood of other trees, is next investigated. The results show that the beech takes more potash than most trees except the *Robinia*—for instance, at 40 years it contains more than four times as much as the spruce fir.

As regards phosphoric acid, the beech and oak need more than other trees, beech-wood at 40 years old having seven times as much as spruce at the same age. With lime the facts are similar: beech needs much more than conifers.

From the whole of the preceding, it is possible to put together some ideas on the quantity of ash-constituents per acre needed for beech forests, and some interesting tables and curves are given in this connection; the return of minerals to the soil in the leaf-fall, &c., is also considered. Perhaps the most important conclusion come to here is that the increment in dry weight of the tree is nearly proportional to the up-take of potash, whereas the up-take of lime is the same—gradually increasing to old age—whether the wood is good or bad, and whatever the nature of the soil. The nitrogenous substance in beech-wood behaves very like the phosphoric acid, in that it diminishes from the tenth to the sixtieth year, and then ascends to a second maximum as the tree reaches 80 years old; and again, the cause is to be found in the influence of the thinning, and in the demands on the reserves when the tree begins to bear seed.

As in all trees, there is of course most nitrogen in the twigs and buds, and in the finer roots. Beech and oak need more nitrogen than other trees, and (so far as the wood goes) the conifers need much less. The total quantity of nitrogen taken up by the beech at 6 years old, in fully stocked plantations, is calculated to be 39·43 kilogrammes per hectare, and this rises to 389·63 at 60 years, and 896·50 at 130 years.

Calculations as to the quantity of nitrogen needed annually per hectare to produce the known yield of wood are then given, and again we meet with the rapid loss after about 90 years, due to seed-production. To these are added estimates of the nitrogen removed in the thinnings, and of that restored in the fallen leaves. All things considered, the quantity of nitrogen concerned annually varies with the age, but at the critical period of 50 to 100 years it amounts to something like 60 kilogrammes per hectare *per annum*.

It is unnecessary to point out further the extreme importance of such investigations as these: it is only in proportion as a nation is armed with statistics based on careful researches like these that it can form any conclusions worth having as to the future value of its forests and the technical merits of those administering them. As to their "practical" bearings, the results speak for themselves: if this is not allowed to be practical science, we may indeed ignore the cry.

H. MARSHALL WARD.

SPECTROSCOPIC RESEARCHES AT THE NORWEGIAN POLAR STATION.

PART II. of the Report on the results obtained at the Norwegian Polar Station at Bossekop in Alten (in connection with the International Polar Investigation,

1882–83) was recently issued at Christiania; and we have already said something as to the contents (NATURE, December 13, 1888, p. 155). The following is a translation of a statement, by Herr Cand. C. Krafft, in this Report:—

"For spectroscopic researches the Expedition took with them a Wrede spectroscope. Unfortunately the obligatory observations did not render it possible to devote adequate attention to spectroscopic researches. The writer may also specially note that the use of powerful magnifiers made measurements with the above-mentioned apparatus extremely fatiguing, and often quite impossible. It seemed to me all the more permissible to omit these measurements because the situation of the usual aurora-line is often very distinctly defined. Other lines besides these were only sometimes observed. Weak, indeterminable bands I observed on November 12, 4h. 18m. If I remember rightly, I saw similar indeterminable bands on another occasion, but I cannot find any notice of it in the observations. The red line was sometimes remarked, but it showed itself very conspicously, and flashed up only some moments (November 2, 9h. 15m.; November 17, 4h.). The general rule is that only the aurora-line was to be seen even in strong auroræ; as, for example, on November 2, 8h. 55m., during a crown-formation, and on November 5, 8h.–9h. on a bow with the intensity 2–3.

"In order to find the value of the scale-division of the spectroscope expressed in wave-lengths, I made, on October 30, 1882, the following determination of the most important Fraunhofer lines:—

B ... 25·04 (λ = 6867) *a* ... 23·27 (λ = 6276)
C ... 24·16 (λ = 6562) D (Mean) 21·78 (λ = 5892)
E ... 18·51 (λ = 5269)
b (Mean) 17·84 (λ = 5174)

"With the help of these determinations I constructed a curve, and obtained from it the following wave-lengths of the auroral lines:—

November 2, 8h. 55m. aurora-line (mean) 20·37 ... λ = 5595
November 11, 10h. 15m. aurora-line 20·26 ... ... λ = 5586
[D (NaCl flame) 21·71.]

"November 17, 4h. 20m.; Herr Schroeter found the following values:—

$$\text{Aurora-line} \quad \left.\begin{array}{l}20{\cdot}37\\ 31\\ 34\\ 34\end{array}\right\}\text{Mean } 20{\cdot}34 \;\ldots\; \lambda = 5587$$

Red line 23·00 ... λ = 6205.

"On account of the rapid flashing-up and disappearance of the red line only this one measurement could be obtained.

"The spectroscope was used chiefly to decide occasionally, in doubtful cases, whether and how far the aurora was present—a matter which, as is well known, it is very often impossible to decide in any other way. Fine cirrostratus clouds may so closely resemble the aurora as to be taken for it, especially if they are lighted by the moon or by twilight. In the latter case one may recognize the aurora-line apart from the continuous spectrum (January 15, 12h.; March 29, 14h.). Meanwhile I do not think I can decide whether the aurora line is to be regarded as absolute criterion for the aurora; I have had an opportunity of observing pulsating masses of light (December 18, 9h.), and also otherwise inexplicable phenomena of light, as well with the usual aurora colour (January 13, 10h.) as with red (November 17, 6h. 15m.), without being able to discover the aurora-line. On a red mass of light it might appear very weakly, even if the light-mass shone powerfully (November 17, 16h.). Beside, the aurora-line was very often to be recognized everywhere. This sometimes made me think that the whole firmament was covered with aurora material, although the explanation may be that the line everywhere visible springs from an aurora, only slightly extended, reflected from fine clouds, &c., floating in the air. This reflected light showed the aurora-line even on objects on the earth (snow on a field, a wall), and even when the sky was pretty well covered (November 11, 10h.; November 12, 5h.; November 14, 8h.; December 15, 15h. 25m.; December 16, 9h.)."

NOTES.

THE Croonian Lecture of the Royal Society, which, as we have already announced, is to be delivered this year by M. Roux, the "Chef de Service" of the Pasteur Laboratory, has now been fixed for Thursday, May 23, at 4.30 p.m., in the Royal Society's apartments at Burlington House.

A GOOD many arrangements for the Newcastle meeting of the British Association, over which Prof. Flower will preside, have now been made. Among the Vice-Presidents are the Duke of Northumberland, the Earl of Durham, the Bishop of Newcastle, Lord Armstrong, the Mayors of Newcastle and of Gateshead, and Mr. John Morley. The following are the Presidents of the various Sections:—A—Mathematical and Physical Science, Captain W. de W. Abney, F.R.S. B—Chemical Science, Sir I. Lowthian Bell, F.R.S. C—Geology, Prof. James Geikie, F.R.S. D—Biology, Prof. J. S. Burdon Sanderson, F.R.S. E—Geography, Colonel Sir Francis de Winton. F—Economic Science and Statistics, Prof. F. Y. Edgeworth. G—Mechanical Science, Mr. William Anderson. H—Anthropology, Prof. Sir W. Turner, F.R.S. The first general meeting will be held on Wednesday, September 11, at 8 p.m. On Thursday evening, September 12, there will be a *soirée;* on Friday evening, September 13, a discourse on "The Hardening and Tempering of Steel," by Prof. Roberts-Austen, F.R.S.; on Monday evening, September 16, another discourse; and on Tuesday evening, September 17, a *soirée*. Excursions to places of interest in the neighbourhood of Newcastle-on-Tyne are being arranged for Saturday, September 14, and Thursday, September 19.

AT a recent meeting of the Executive Council of the British Section of the Paris Exhibition, the cordial thanks of the Council were given to Sir Frederick Leighton, P.R.A., and the Fine Arts Committee, for their exertions to insure that the Fine Arts Department at the Exhibition should be a credit to the British Section and the country. The result of the exertions of the Committee will be that British art will be represented in Paris by works of many of our foremost artists. Why is not like energy being displayed by English men of science? There ought to be a Science as well as a Fine Arts Committee, and the necessary arrangements might easily be made, as there are several members of the French Institute in England.

THE Directors of the Ben Nevis Observatory have applied to the Association of the Glasgow International Exhibition of 1888 for a grant from the surplus fund of the Exhibition. In the memorial setting forth the claims of the Observatory on the support of the public and of public bodies, reference is made to the immediate and important advantages that will result from the work of the High and Low Level Observatories of Ben Nevis towards the further development of the meteorology of the Clyde, in which Glasgow has taken so prominent a part, and by the results of which the shipping and commercial interests will to a certainty be largely benefited; and it is urged that, in carrying out these national objects, the Directors must look to the liberality of the public and of public bodies, for the assistance required to supplement the aid offered by the Government towards the completion and maintenance of this double Observatory.

THE Botanical Society of France has issued a circular signed by its President, M. de Vilmorin, and Secretaries, inviting foreign

botanists to attend a Botanical Congress to be held in Paris during the second half of August in the present year, and to present treatises on botanical subjects, pure or applied, that may be most familiar to them, with the view of promoting discussion on them. The following subjects are especially proposed for consideration:—(1) The usefulness of establishing joint action between the different Botanical Societies and Museums for the purpose of preparing accurate maps of the distribution of species and genera of plants over the globe, a work similar to that undertaken by International Geological Congresses. An Exhibition of maps, books, *brochures*, photographs, &c., relating to botanical geography will be held, during the Congress, at its place of meeting. (2) Characters for classification furnished by anatomy. Botanists intending to be present at the Congress should send in their names, before June 1, to M. P. Maury, the Secretary of the Organizing Committee, 84 Rue de Grenelle, Paris, when they will receive special invitations, and information as to the day and place of meeting. The titles of papers proposed to be read, or of verbal communications, should be forwarded as early as possible.

We reprint from the *Times* of March 26 the following obituary notice:—"We have to record the death, at a ripe age, of a man whose name is well known and honoured wherever the science of naval architecture is studied. Joseph Woolley, M.A., LL.D., F.R.A.S., formerly Principal of the School of Mathematics and Naval Construction at Portsmouth, and subsequently, for many years, the Admiralty Director of Education, died at Sevenoaks on Sunday, after a few days' illness. Trained at Cambridge, where he was a Fellow of St. John's College, he was selected in 1848 as the head of the Technical School founded in that year by the Admiralty. He continued to hold that office until the school was discontinued in 1853. When a School of Naval Architecture was again founded, on other lines, at South Kensington, in 1864, Dr. Woolley was appointed Inspector-General by the Committee of Council on Education, and he continued to superintend the school until his retirement from active life. In 1850 he published a "Treatise on Descriptive Geometry," which is widely known as a text-book. In 1860, when the Institution of Naval Architects was founded, very largely by his own personal influence, Dr. Woolley opened its proceedings by an address on "The present state of the mathematical theory of naval architecture." He enriched the Transactions of the Institution with frequent contributions on all current questions presenting any peculiar difficulty. He was a member of the Committee on Designs in 1871, and of other important Naval Committees. He was for many years in holy orders, but he relinquished them later in life, although he continued to be to the last a devout worshipper in the Church of England. He was a man who was much loved by all who were privileged to work with him."

We regret to learn that Prof. Donders died at Utrecht on Sunday.

Two physiologists of note have recently died: Prof. Krukenberg, of Jena, well known for his researches in invertebrate physiology, and R. Gscheideln, of Breslau, the author of a "Physiologische Methodik," which unfortunately remains unfinished.

We learn from *Science* that Captain John Ericsson, whose death, at New York, we lately recorded, continued to labour at his sun-motor until within two weeks of his death. "As he saw his end approaching, he expressed regret only because he could not live to give this invention to the world in completed form. It occupied his thoughts up to his last hour. While he could hardly speak above a whisper, he drew his chief engineer's face close to his own, gave him final instructions for continuing the work on the machine, and exacted a promise that the work should go on." *Science* says that the respect shown at Captain Ericsson's funeral was such as is seldom seen at that of a private citizen. "The streets in the neighbourhood of his late residence were crowded from the early morning hours with thousands, who for four hours passed through the house to pay homage to the departed genius. New York is a place full of human beings,—so full that each pays little or no heed to his neighbour; yet the great respect for this man of science and of action was shown in the number and character of those who followed his remains to their resting-place, in the uncovered heads as they were borne along the busy streets, and in the impossibility of admitting to Trinity all that wished. Ericsson was a man who could have endeared many to him, but he had a strong sense of duty to his work, which induced him to make few friends. This final homage of the unmindful crowds of the great city was to his genius well applied."

A gift of some scientific as well as artistic interest has just been made to the Royal Hibernian Academy, Dublin, by Miss Mary Anne Nicholl. She has presented to the Academy fifty-six studies in water-colours of the palms and foilage plants and fruits of Ceylon, painted by her late father, Andrew Nicholl, R.H.A., who held the first appointment of Master of Landscape Painting, Engineering, Drawing, and Design, in the Colombo Academy. The studies are accompanied with a list of the names of the flora.

At the distribution of prizes to students of the Polytechnic Institute, Regent Street, on Monday, Mr. W. T. Paton was able to give a good account of the past year's work. The number over and above the usual attendance had been, he said considerably more than 1000, and they now had 7000 members, who were attending classes there. The Lord Mayor, who gave away the prizes, spoke of the advance which had lately been made in technical education, and of the good influence exerted by the Polytechnic Institute. The Committee wished him to say that they had now annexed the West End School of Art to the Institute. Further funds would be required to carry on the work. The Charity Commissioners would provide £31,000 for endowment if another £4000 were forthcoming.

The Geographical Society of Bremen has commissioned Dr. Kückenthal, of Jena, to undertake another journey to the Arctic regions, in order to make zoological researches. He will start for Spitzbergen at the end of April, and is expected to return in October next.

Prof. Franz Exner, of Vienna, who has spent some months in Ceylon studying atmospheric electricity there, is now on his way back to Europe. A grant was made by the Vienna Academy of Sciences in aid of his scientific work.

The new Natural History Museum at Vienna will be opened to the public in the summer, and it is expected that the rich collections will attract large numbers of visitors.

Dr. J. Hann, Director of the Austrian Meteorological Service, has laid before the Vienna Academy of Sciences an exhaustive investigation of the diurnal range of the barometer over the globe. He has calculated the harmonic coefficients for each month, and for the year, for a large number of places, and has investigated the variation both of the phases and of the amplitudes of the single and double daily oscillations. The latter show a remarkable independence of geographical and seasonal influence (as before pointed out by Lamont and others), and appear to be connected with a cosmical origin. The investigation also shows that the amplitudes of the semi-diurnal oscillation decrease with height in exact proportion to the pressure, and have a marked dependence upon latitude. The yearly range exhibits two maxima at the periods of the equinoxes, and also a third maximum which falls in January in both hemispheres, while

in July the amplitude of the double daily oscillation is smallest over the whole globe. The author also investigates the single daily oscillation, in connection with the influence exerted by the position of the station, as well as the range of the barometer at sea, and arrives at some interesting results.

SIX shocks of earthquake were noticed at Aquila on March 11. Two were very severe, but had no injurious results. Shocks were felt at Idstein, Auroff, and Görsrod, near Wiesbaden, on March 12, at 2.29 a.m. The direction of the shocks was from west to east.

TELEGRAMS received at Madrid report that a strong shock of earthquake was felt on March 25 at Alhama in the province of Granada. No damage was done, but great alarm prevailed among the inhabitants.

M. B. HASSELBERG, of Pulkowa, has been studying the absorption spectrum of iodine, which he has succeeded in resolving into widely separated lines with a Rowland grating, and in photographing. The wave-lengths of about 3000 lines have been determined by a dividing-engine.

ANOTHER interesting pair of geometrical isomers have been discovered by Dr. Auwers and Prof. Victor Meyer. They are the monoxims of benzil, both possessing the constitution $C_6H_5 . C(NOH) . CO . C_6H_5$. Benzil, $C_6H_5 . CO . CO . C_6H_5$, is the typical di-ketone of the benzene series, and reacts, like other ketones, with hydroxylamine. As there are two CO groups present, there are two possible oxims, a monoxim and a dioxim. A few months ago, Dr. Auwers and Prof. Meyer showed that there were really two dioxims, both of the constitution $C_6H_5 . C(NOH) . C(NOH)\ C_6H_5$, but differing in the arrangement of the various groups in space. One of these isomers was shown to probably possess the configuration $\begin{array}{l} C_6H_5-C=NOH \\ \qquad\quad | \\ C_6H_5-C=NOH \end{array}$, while the other, which melts 30° C. lower than the first, may be formulated $\begin{array}{l} \quad C_6H_5-C=NOH \\ \qquad\qquad\ | \\ NOH=C-C_6H_5 \end{array}$. It is now found that there are also two corresponding monoxims, one of which, termed the α-monoxim, may be represented as $\begin{array}{l} C_6H_5-C=NOH \\ \qquad\quad | \\ C_6H_5-C=O \end{array}$, while the β-monoxim has its groups probably disposed in the manner $\begin{array}{l} C_6H_5-C=NOH \\ \qquad\ | \\ \ O=C-C_6H_5 \end{array}$. Both these monoxims are obtained when benzil and hydroxylamine, NH_2OH, are allowed to react upon each other at the ordinary temperature. If the hydroxylamine be used in the form of its hydrochloride dissolved in a little water and added to an alcoholic solution of the benzil, a preponderating quantity of the β-compound is formed, and in greater quantity the higher the temperature. At −15° C., the product consists largely of the α-monoxim; at 0°, about equal quantities of the two are formed; at the ordinary temperature of a room the product is almost exclusively of the β-compound; and finally, when the operation is conducted upon a water-bath, a quantitative yield of the β-monoxim is obtained. The α compound is best prepared by dissolving 10 parts of benzil in 30 parts ordinary alcohol, and adding a mixture of 3½ parts of hydrochloride of hydroxylamine and 4 parts of soda dissolved in a little water. After standing a few hours the mixture is poured into water, and the turbid liquid thus formed filtered. On acidifying the filtrate an oil separates out which rapidly crystallizes. On recrystallizing the mixed crystals from dilute alcohol, a large yield of the pure α-compound is obtained, owing to its much more sparing solubility, in microscopic tabular four-sided crystals possessing a mother-of-pearl lustre. It may also be recrystallized from benzol, from which it is obtained in the same form. The crystals melt sharply at 137°. On the other hand, the β-monoxim crystallizes from alcohol in thick prisms, melting at 113°, 24° lower than the α-compound. Another striking point of dissimilarity is that from benzol the β-compound crystallizes with half a molecule of benzol of crystallization. Each reacts with a further equivalent of hydroxylamine to form the corresponding dioxim; and each also forms a characteristic ethereal salt with acetic acid, the α-acetic ether forming broad flat prisms melting at 61°, and the β-acetate crystallizing in needles of melting-point 78°. Hence the oxims of benzil form a most beautiful and indubitable case of true geometrical isomerism, and a valuable further justification of the modified Van t' Hoff-Wislicenus hypothesis. It is to be hoped that, by further investigations of similar cases, we may indeed eventually be enabled to form some idea of the actual orientation of the atoms in our chemical molecules.

THE American Society of Naturalists recently appointed a Committee to report on the teaching of science in schools. The Report, which has been adopted and approved of by the Society, contains, amongst others, the following suggestions as to the mode in which, in the opinion of the Committee, science can best be taught in the schools:—Instruction in natural science should begin in the lowest grades, where it should be conducted chiefly by means of object-lessons. More systematic instruction should be given in the high schools during the four years' course preparatory to College. An elementary knowledge of some one or more branches of natural science should be required of every candidate for matriculation at College. There are some differences of opinion as to the details in carrying out this plan, but the Committee recommends that scientific study should begin with the study of plants and animals, the botanical instruction beginning with drawing the outlines of the leaves of plants, and the zoological with descriptions of the more familiar animals, special prominence being given to the study of those plants and animals which are useful to man. The simple geological phenomena of the district in which the school is situated should be taught. Children should be encouraged to collect specimens of all kinds of natural objects, and these specimens could be made the subject of the object-lessons. An attempt should be made to teach the rudiments of human physiology and hygiene. The Committee recommends the introduction into the highest grades of the grammar school of very rudimentary lessons in physics and chemistry, which would pave the way for further study in the high schools and Colleges.

WE have received several interesting papers, by Prince Roland Bonaparte, on subjects relating to geography and anthropology. One of them (in French) is an account of the early voyages of Dutchmen to the East India Archipelago. In another series of French papers the Prince deals with geographical discoveries in New Guinea. He has also reprinted from the Journal of the Anthropological Institute a note (in English) on the Lapps of Finmark. In this paper he presents various anthropological data collected during a tour of three months in Scandinavia. A more elaborate paper (in French), by F. Escard, which is printed in the same form as these essays, gives a full account of the experiences of Prince Roland and his companions in the country of the Lapps.

THE "Record of the Excursions of the Geologists' Association, 1860–84," which has been prepared by Mr. T. V. Holmes, is now ready for the press, but it will not be printed until the names of a sufficient number of subscribers have been received. The work will consist of over 500 pages, and contain accounts of all the sections and districts visited by the Association down to the end of 1884, with the illustrations (sections, &c.), which have from time to time appeared in the circulars and Proceedings.

A FRENCH translation of Prof. Romanes's "Mental Evolution in Man" is in course of preparation in Paris.

THE *Oesterreichische Botanische Zeitschrift*, now in its thirty-ninth year, is edited, from the commencement of the present year, by Dr. Richard R. von Wettstein.

AT the annual meeting of the Governors of Aberdare Hall, University College, Cardiff, which took place this month, the Executive Committee were able to submit a most satisfactory report. Several students had distinguished themselves by gaining scholarships both at the College and Hall; two had taken the B.A. degree (London); others had passed the Intermediate in Arts and matriculation examinations, among whom was Miss Moss, who took the twelfth place in Honours division, matriculation examination.

THE additions to the Zoological Society's Gardens during the past week include two Chinese Mynahs (*Acridotheres cristatellus*) from China, presented by Mrs. Rigby; a Rose-crested Cockatoo (*Cacatua moluccensis*) from Moluccas, presented by Miss Liming; a Long-tailed Copsychus (*Copsychus macrurus*, ♂) from India, two Silky Bower-Birds (*Ptilonorhynchus violaceus*, ♂ ♀) from Australia, a Blue and Yellow Macaw (*Ara ararauna*) from South America, deposited; two Squirrel Monkeys (*Chrysothrix sciurea*) from Guiana, a Four-horned Antelope (*Tetraceros quadricornis*, ♂) from India, a South American Flamingo (*Phœnicopterus ignipalliatus*) from South America, purchased; a Gayal (*Bibos frontalis*, ♀), a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBSERVATIONS OF JUPITER.—An excellent series of eighty-four drawings of the planet Jupiter at different periods during the years 1881–86, made with the reflector of 3 feet aperture at Birr Castle Observatory by Dr. Boeddicker, has just been published in the form of a communication to the Royal Dublin Society (vol. iv. series 2, March 1889). Twenty-two of the drawings were made during the opposition of 1881–82, thirty-one during 1882–83, twenty-one during 1883–84, eight during 1884–85, and two during 1885–86. The drawings made at the telescope have been exactly reproduced by a photo-mechanical process in order to avoid the errors which might have arisen by the employment of the ordinary lithographic process. Throughout the descriptive notes a very convenient notation has been employed for reference to the various features. Dr. Boeddicker draws attention to the three observations of March 16, 1883, showing remarkable changes in the appearance of one of the belts during the course of an hour. The first drawing shows two detached patches, which, in the succeeding drawings, become the shadows of large cumulus-like clouds lying across the Jovian surface. It is suggested that these apparent changes may be simply due to the combination of the more obvious details with the finer ones after prolonged examination, and that the discrepancies between drawings made at the same time by different observers may thus be accounted for. Photography may be expected in the near future to overcome this difficulty.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *MARCH* 31—*APRIL* 6.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on March 31

Sun rises, 5h. 38m.; souths, 12h. 4m. 8·2s.; sets, 18h. 30m.: right asc. on meridian, 0h. 40·3m.; decl. 4° 21′ N. Sidereal Time at Sunset, 7h. 7m.

Moon (New on March 31, 12h.) rises, 6h. 12m.; souths, 12h. 13m.; sets, 18h. 28m.: right asc. on meridian, 0h. 49·2m.; decl. 0° 7′ S.

Planet.	Rises. h. m.		Souths. h. m.		Sets. h. m.		Right asc. and declination on meridian. h. m.		° ′
Mercury..	5 17	...	10 47	...	16 17	...	23 23·3	...	6 35 S.
Venus.....	6 4	...	14 16	...	22 28	...	2 52·9	...	22 52 N.
Mars......	6 16	...	13 22	...	20 28	...	1 58·3	...	12 1 N.
Jupiter....	2 1	...	5 57	...	9 53	...	18 31·9	...	22 57 S.
Saturn....	12 49	...	20 28	...	4 7*	...	9 5·7	...	17 53 N.
Uranus...	19 16*	...	0 42	...	6 8	...	13 16·3	...	7 21 S.
Neptune..	7 33	...	15 17	..	23 1	...	3 53·7	...	18 37 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

April.		h.		
2	...	2	...	Mars in conjunction with and 5° 7′ north of the Moon.
3	...	5	...	Venus in conjunction with and 11° 7′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.		Decl. ° ′				h. m.	
U Cephei	0 52·5	...	81 17 N.	...	Apr.	1,	4 15	*m*
					,,	6,	3 55	*m*
Algol	3 1·0	...	40 32 N.	...	Mar.	31,	19 50	*m*
ζ Geminorum ...	6 57·5	...	20 44 N.	...	Apr.	4,	1 0	*M*
R Canis Minoris ...	7 2·6	...	10 12 N.	...	,,	5,		*m*
R Canis Majoris ...	7 14·5	...	16 11 S.	...	,,	5,	19 47	*m*
			and at intervals of				27 16	
U Monocerotis ...	7 25·5	...	9 33 S.	...	Apr.	6,		*M*
U Geminorum ...	7 48·5	...	22 18 N.	...	,,	6,		*M*
W Virginis	13 20·3	...	2 48 S.	...	,,	2,	1 0	*m*
X Boötis	14 18·9	...	16 50 N.	...	,,	1,		*M*
δ Libræ	14 55·1	...	8 5 S.	...	,,	2,	2 0	*m*
U Coronæ	15 13·7	...	32 3 N.	...	,,	4,	4 30	*m*
β Lyræ...	18 46·0	...	33 14 N.	...	,,	3,	0 0	m_2
R Lyræ	18 52·0	...	43 48 N.	...	,,	5,		*m*
δ Cephei	22 25·0	...	57 51 N.	...	,,	2,	23 0	*M*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A. °		Decl. °		
Near γ Libræ	233	...	15 S.	...	Swift; long paths.
From Delphinus	305	...	12 N.	...	Slow; bright.

GEOGRAPHICAL NOTES.

AT the meeting of the Geographical Society on Monday two papers were read, both dealing with the Caucasus, midway between Kazbek and Elburz. Here the chain towers up in two great parallel crests, containing within a few square miles at least half a dozen peaks over 16,000 feet in height, an elevation probably reached nowhere else by the summits of the crystalline crest. Two of these peaks are recognized as the second and third summits of the Caucasus—Koshtantau, 17,091 feet; and Dychtau, 16,924 feet. One of the papers, by Mr. A. F. Mummery, described his ascent last summer of Koshtantau, while Mr. H. W. Holder dealt with the peaks of the neighbouring Bezingi Glacier. From Mujal, on the south-west of the Zanner Glacier, Mr. Mummery and his companion made their way round by the Thuber and Gvalda passes to Bezingi in order to make the ascent from that side. The arrangement of this part of the chain, Mr. Mummery states, is, from an Alpine point of view, very curious. There is a lofty ridge with occasional *aiguilles*, from the southern slopes of which stretch the great ice-fields of the Thuber, and there is a second and rather less lofty ridge to the north and parallel to it, from the northern flank of which flow the Basil-su and its various affluents. In the narrow trough between these two ridges lies the head of the Gvalda Glacier. Though seldom so clearly marked as in this instance, the same system of short parallel ridges may be traced throughout the whole central group, with the result that the upper and middle basins of the great glaciers are nearly always parallel to the main ridges, and it is only when the drainage from these catchment basins reaches the head of the lateral valleys that the ice sweeps round and flows away at right angles from the watershed. The Gvalda Glacier is probably the most important on the south side of the Caucasus, and far exceeds in size any on the south slope of the Alps. Its basin probably exceeds in extent that of the Glacier du Géant, to which it is not without a resemblance. The Caucasian glaciers in this part of the chain are

much less crevassed than the Alpine, apparently due to the lesser inclination of the great glaciers, and possibly to the greater thickness of their ice. With reference to Caucasian forests, Mr. Mummery has some interesting observations. The upper valley of the Basil-su can still boast a fairly extensive forest; but partly by the axe, and mainly by the agency of the sheep and goats, the forests are fast shrinking. Below a certain point in the Basil-su Valley, not a tree, not a bush is to be seen; the country has been denuded by the flocks of the natives. Mr. Mummery is inclined to attribute the extraordinary contrast between the treelessness of the northern valleys and the dense forests of the southern less to climatic differences than to the form in which the wealth of their respective inhabitants exists: in the one case, oxen, horses, sheep, and goats; in the other, well-tilled and neatly fenced fields and orchards. Though at first sight it appears difficult to believe that sheep and goats can destroy the forest over great stretches of country, a careful examination of the Upper Basil-su shows that the cause is sufficient to produce a continuous contraction of the forest area, and leaves it a mere question of time as to when the last tree in that valley shall be cut down and burnt. After overcoming many difficulties, Mr. Mummery reached the summit of Koshtantau, the first time the mountain had been scaled.

Mr. Holder and his friends also succeeded in climbing Koshtantau and several other peaks. He gave in his paper an instructive account of the striking difference in the character of the mountains which form the two great chains of the Caucasus and the Alps. Mr. Holder was much impressed with the wildness, the majesty, the awfulness, of the Caucasus. Whilst the main glacier streams, *e.g.* the Bezingi, the Mishirgi, and the Dych-su, have but a slight fall, and are but little crevassed, the upper parts of the glaciers, those which come down from the mountains to form the great streams, have so steep a fall that they may be compared rather to cascades than streams of ice, and are cut into *seracs* of the most fantastic character. Comparatively little snow lies on the steep southern faces of the mountains, and the rocks which face the south are so broken and loose that the danger of falling stones in ascending and descending is extreme. No single bit of rock can be trusted, and the rope ought never for a moment to be discarded. On the northern faces much more snow lies, and the rocks were firmer and more reliable. The climate of the Caucasus is healthy and invigorating, yet distinctly more humid than in the Alps. It may perhaps be sufficiently interesting to note that none of the party experienced the slightest inconvenience on account of the rarity of the atmosphere at the highest altitude reached, over 17,000 feet; but that above about 15,000 feet the snow was always of the light and powdery character so tantalizing and fatiguing to mountaineers.

The March number of *Petermann's Mitteilungen* contains a long paper by Spiridion Gopčević on the ethnographical conditions of Macedonia and Old Servia. Herr Otto F. Ehlers contributes a lively account of his ascent of Kilimanjaro last summer. He does not add much to our knowledge of the mountain, nor did he reach the actual summit. He went round its north face, and endeavoured from the north-east side to find out the character of the summit. He found the same wall of ice which was seen by Dr. Meyer. He states that so far as he could observe he could see no trace of a crater, while the masses of ice and snow lay in quiet wave-like lines, with much fresh snow. The height he estimates at over 19,690 feet. He makes the extraordinary statement that traces of elephants, buffaloes, and antelopes were met with at a height of about 16,000 feet, where also he found the last traces of vegetation.

M. Jules Borelli has just returned from an exploration extending over nearly two years in the country of Shoa and in Galla Land, undertaken under the patronage of the French Government. M. Borelli has added much to our knowledge of Shoa and its people, and among other things has discovered the source of the Hawash. His most important work has, however, been done in the region to the south of Shoa, in the country peopled mainly by the Gallas. He traced the Omo River, to about 6° 20′ N. lat. His map throws quite a new light on the hydrography of the region. Hitherto, the Omo has been conjectured to be the upper course of the Jub, which falls into the Indian Ocean at the equator. But the data collected by M. Borelli, and which appear to be confirmed by the recent discoveries of Count Teleki, open the field to new hypotheses. It would appear that the Omo, instead of flowing towards the east, takes a westerly and then a southerly direction, when, at about 2° N. lat., with a breadth of over 1500 feet, it expands into the great lake Samburu. It remains now to discover whether this lake is an African Caspian, or whether it has an outflow towards Lake Victoria Nyanza. In that case the Omo becomes a remote feeder of the Nile.

ELECTRICAL NOTES.

Nagaoka (*Phil. Mag.*, February 1889) of the Imperial University, Japan, has investigated the effects of torsion and longitudinal stress on the magnetization of nickel. Stress increases the magnetization of iron, but diminishes that of nickel, and the effect of torsion is also reversed in the two metals. Twisting nickel wire increases magnetization, while it diminishes that of iron. Nagaoka finds that this is true for weak stresses only. Beyond a critical value of the stress in a constant field, one end of the nickel wire acquires the two opposite kinds of magnetism during torsion and detorsion. The nickel wire used was unfortunately impure, for it contained 1·7 per cent. of iron, but the fact was clear that during untwisting the polarity of a nickel wire changed sign.

Dr. John Hopkinson, F.R.S., has given the Royal Society (March 7) some interesting facts relating to the magnetization of iron at high temperatures. At 737° C. all traces of magnetism disappear, but before this point is reached, viz. 727°, its permeability increases with great rapidity to a very high figure, when it suddenly drops to unity. In a subsequent paper, read March 21, he showed that the resistance also makes an abrupt change at the same temperature, which is that of recalescence, as discovered by Barrett.

Shelford Bidwell (R.S., March 14) showed a very pretty experiment by which the effect of radiations on the magnetization of iron were made evident. An iron bar is carefully annealed, cooled, magnetized, and then demagnetized by currents without any mechanical motion. The condensed beam from an oxyhydrogen lamp is thrown upon its pole, when magnetism at once appears. There is an instantaneous magnetic change, which is purely an effect of radiation.

A. Bernstein (*Centralblatt für Electrotechnik*, i. p. 165, 1889) has proved that the formula $C = a\sqrt{d^3}$ is not true for the fusing currents of wires of a diameter smaller than 0·25 mm. It is known that such fine wires absorb more energy than thicker ones to acquire the same temperature in air, and that the formula is $C = ad$. Bernstein has experimented with carbons of different diameters in the moderate vacuum of a glow lamp, and has obtained the following results:—

Diameter in mm.	Energy per sq. mm. of surface.
0·9	0·51
0·56	0·54
0·295	0·56
0·185	0·85
0·15	0.95

His conclusion is that lamps with thick carbons absorb less energy relatively than lamps with fine filaments, and are therefore more economical.

C. S. Bowie (*Electric World*, February 23, 1889) has found that the static electricity generated by calender rolls in a paper mill acts very injuriously on the life of glow lamps. They are now effectually protected with wire guards.

Among the numerous practical purposes to which electricity can be applied *tanning* must be added. Leather is said to be produced from the raw hide in four days.

If Prof. Oliver Lodge has failed to secure general faith in his lightning conductor theories, he at least has succeeded in directing scientific fashion to experimenting with Leyden jars. Righi (*Bull. Acad. dei Lincei*, xii. 16, 1888) has constructed a battery of 108 condensers, having a capacity of 18,810 electrostatic units. With it he has produced sparks 5 metres long over strips of glass coated with zinc filings, and 1 metre long over water. Platinum, iron, brass, gold wires, very fine and of 3½ metres length, are instantly vaporized into beautiful coloured coronæ of the same shape as that impressed on the wire. The wire becomes vapour at a high temperature, and forms as it were a vacuum tube, the sides of which are cold air.

ON THE CONFLUENCES AND BIFURCATIONS OF CERTAIN THEORIES.[1]

AXIOMS, says Proclus,[2] are common to all sciences, though each employs them in its peculiar subject-matter. A little further on[3] he cites Aristotle[4] as saying that one science is more certain than another, viz. that which emanates from more simple suppositions than that which uses more various principles; and that which tells the why, than that which tells only the simple existence of a thing; and that which is conversant about intelligibles, than that which touches and is employed about sensibles.

Proclus adds that, according to these definitions of certainty, arithmetic is more certain than geometry, since its principles excel by their simplicity. For the conception of unity has no reference to position in space, while that of a point involves such reference. In short, we may say that to count a number of objects is a simpler operation than to measure the distances between them.

All this, and much more, shows how early the notion of what is sometimes called a hierarchy of the sciences arose. Proclus's order of precedence would seem to be this, viz. logic,[5] arithmetic, geometry, mechanics, optics, dioptrics,[6] and so on; the progression being from the more to the less abstract, or from the abstract to the concrete.

Francis Bacon, mindful perhaps of Proclus,[7] and duly appreciating the power of mathematics as an instrument[8] and its value as a discipline,[9] expressly takes the degree of abstractness of a science as the mark for its classification. He places mathematics, as the most abstract of sciences,[10] at one end of the scale and "policy" at the other. He does not graduate the scale minutely, but it may be that, as in the case of the categories,[11] he attached no great value to such details. Distinguishing philosophy from theology, logic, and mathematics,[12] he assigns to it the axioms which are common to several sciences and the inquiry into essences, as quantity, similitude, diversity, possibility, and the rest. Science he divides between metaphysics, the science of the abstract and permanent, and physics, that of matter and its changes.[13] Bacon, in one place, names the one universal science by the name of philosophy, while in another he treats philosophy and metaphysics as two distinct things.[14] He uses the word metaphysics in a sense different from that in which it was then[15] received. Mathematics he places as a branch of metaphysics, and as having determined or determinate quantity for its subject. To the pure mathematics, he says, belong geometry and arithmetic; the one handling continuous, and the other discrete quantity.[16] If he means continuous quantity so far as it is immovable, he agrees with the Pythagoreans.[17]

Quantity, time, and space are placed by Aristotle among his categories, or are implied in them. With regard to space, he does not seem to have reached the Kantian view in any way, nor to be very clear in his meaning, though he apparently feels that to realize space we must have motion. His conception of time as one of the elements required for measuring motion, and his starting the problem as to whether we could have time without a mind to conceive, seem a more distinct approximation, though only an approximation, to Kant's view of time as merely a subjective condition of perception.[18]

Newton, in the Scholium to his definitions, distinguishes between absolute and relative time, the latter being time conceived in its relation to phenomena. Of absolute time (otherwise called duration) which has no relation to anything external, he says that it flows equably, and that its rate of flow and the order of its parts are immutable. In his "Fluxions" he uses the word time in a somewhat different sense, viz. as meaning the independent variable, characterized by an equable increase, fluxion, or flow.[1] Sir W. Rowan Hamilton treated algebra as the science of pure time, but his doctrine is not entirely[2] assented to by De Morgan, nor by Prof. Cayley, who indeed, in his Southport Address (p. 19), intimates dissent from it. Proclus does not connect arithmetic with time, and Prof. Cayley suggests (*ib.* p. 18) that, in any case, the notion of number or plurality is not more dependent on time than on space. By the logicians, time seems to be regarded as the more abstract of the pure intuitions. In fact, time is implied in memory and in thought itself, and Prof. Francis W. Newman observes that no man could get through a syllogism if he forgot the first premiss while dwelling on the second.[3] Moreover, he has recourse to the idea of time when he comes to discuss propositions,[4] and Boole investigates the nature of the connection of his own secondary propositions with the idea of time.[5] The ancient Indians had their cyclical periods, but not therefore necessarily any notion of a uniform curvature (so to say) of time.

Absolute space, says Newton, perpetually remains similar to itself and immovable; and, further on in the Scholium, he adds that the order of its parts is immutable. In the preface to the "Principia" he had observed that the description of straight lines and circles, on which geometry is founded, belongs to mechanics, and he follows up this train of thought. But, whether he means to detach himself from Plato, I must leave others to say. It is said to be certain that he was familiar with Bacon's works; that he uses the word axiom, not in Euclid's sense, but in Bacon's, thus giving the name of axioms to the laws of motion, which, of course are ascertained by the scrutiny of nature, and to those general experimental truths which form the groundwork of optics.[6] Now Bacon says that, in his judgment, the senses are sufficient to certify and report truth, either immediately or by way of comparison.[7] Moreover, he suggests that the rule *Quæ in eodem tertio conveniunt, et inter se conveniunt*, a rule so potent in logic as that all syllogisms are built upon it, is taken from the mathematics.[8] In seeking an origin for the more abstract in the less abstract, Bacon is not solitary. Thomas Stephens Davies suggested[9] that the argument from superposition had its origin in mechanical considerations, and from the fitting together of material figures. Moreover, it is conceivable that some observant person among the ancient Egyptians, whose custom it was to stamp their bricks, noticing the resemblances of the marks and the correspondence of the impressions with the impressing tool, may have been led to a recognition of the rule quoted by Bacon. The doctrine that there enters into geometry an element derived from the senses has, indeed, appeared in books designed for ordinary readers. Thus, Prof. Newman, writing in 1836–38, although in one part

[1] Presidential Address delivered by Sir James Cockle, F.R.S., to the London Mathematical Society, on November 8, 1888.

[2] Proclus, "Commentaries on the First Book of Euclid's Elements" (Taylor's Translation, London, 1792), p. 92.

[3] Proclus, *op. cit.*, p. 93.

[4] Taylor (*ib.* p. 93) supplies the reference to the first Analytics, t. 42.

[5] Proclus, *op. cit.*, p. 79. Hume ("Treatise," vol. i., London, 1739, Book i. Part 3, p. 129. *et vid.* p. 128) says that geometry falls short of that perfect precision and certainty which are peculiar to arithmetic and algebra.

[6] Proclus, *op. cit.*, p. 93; *et vid.* pp. 78, 79.

[7] Bacon, "The Proficience and Advancement of Learning" (Oxford, 1633), pp. 49, 50.

[8] Bacon, *op. cit.*, pp. 151, 152; *et vid.* pp. 119, 120.

[9] Bacon, *op. cit.*, pp. 152, 205, and 231.

[10] Bacon, *op. cit.*, p. 218; *et vid.* pp. 150, 151.

[11] Bacon, *op. cit.*, pp. 130, 131, 140, and 201; *et vid.* p. 161.

[12] Bacon, *op. cit.*, pp. 49, 50; *et vid.* pp. 130, 131, and 140.

[13] Bacon, *op. cit.*, p. 141.

[14] Bacon, *op. cit.*, pp. 130, 140.

[15] Bacon, *op. cit.*, p. 138; *conf.* pp. 146, 147.

[16] Bacon, *op. cit.*, pp. 150, 151.

[17] Proclus (Taylor's Translation), p. 74.

[18] For this summary of Aristotle's views I am indebted to Mr. Reginald H. Roe, who referred me to Ueberweg's "Hist. of Phil.," p. 164, for a more general statement, and to p. 165 for a list of the best books for its fuller elucidation, adding that in Ritte and Preller's extracts, pp. 288 and 289, will be found all the important passages from Aristotle bearing on the question. As to the views of Boole, see his "Laws of Thought" (London, 1854), pp. 162 *et seq.*; see also p. 419. Boole treats of space at pp. 163, 175, and 418; and at p. 175 he quotes Aristotle's statements respecting the existence of space in three dimensions.

[1] Newton, "Fluxions," pp. 26 and 38 of the small edition (London, 1737). This is a genuine work of Newton's. As to its bibliography, see *Notes and Queries*, 2nd S., vol. x. pp. 163, 232, 233; 3rd S., vol. xi. pp. 514, 515; 4th S., vol. ii. p. 316; 5th S., vol. iv. p. 401; 6th S., vol. iv. pp. 129, 130; vol. v. pp. 263, 264, 304, 305, and 426. This octavo edition is very scarce. Indeed, I only know of two copies, viz. my own copy and one in the library of the Royal Astronomical Society.

[2] De Morgan, "On the Foundation of Algebra," Cambridge Transactions, vol. vii. pp. 173–87; see pp. 175, 176. The remarks of Prof. Cayley on Whewell, at p. 18 of his Southport Address, are applicable to Rowan Hamilton.

[3] Newman, "Lectures on Logic, or on the Science of Evidence," &c. (Oxford, 1838), p. 15.

[4] Newman, *op. cit.*, pp. 32–34.

[5] Boole, "An Investigation of the Laws of Thought" (London, 1854), pp. 162 *et seq.*

[6] See the account of the "Novum Organon" in the "Library of Useful Knowledge," p. 10.

[7] Bacon, "Advancement of Learning" (cited *supra*), p. 193.

[8] Bacon, *op. cit.*, p. 132.

[9] T. S. Davies, Geometrical Notes, *Mechanics' Magazine*, vol. liii. (1850), pp. 150, 169, 262, 291, 442. Davies points out "the connection between parallels and similar triangles." He thinks that Aristotle's secession from the school of Plato arose from his enforcement of his own logical doctrines. Davies rejects the notion of a geometry built upon definitions alone without the assistance of axioms.

of his "Logic" (p. 25), he says that in geometry no results are admitted by help of observation and testimony, but only by reasoning from the definition, yet he afterwards (p. 55) states that, as space and its properties appear undeniably to be learned by sense, the argument seems to him to preponderate for naming geometry a mixed science, and believing that its propositions are real and not verbal truths. And Potts[1] says that geometry seems to rest on the simplest inductions from experience and observation, and that its principles are founded on facts cognizable by the senses.

But it is to Reid[2] that the idea of a more precise mathematical treatment of the subject is due, and his name ought to head the roll on which will be inscribed the names of Lobatschewsky, Riemann, and other investigators. Kant, indeed, disposes of such questions summarily, by saying that it follows from his premisses that the propositions of geometry are not the determinations of a mere creature of our feigning fancy, but that they necessarily hold of space, and consequently of all that may be met within it, because space is nothing else than the form of all the external phenomena, in which alone objects of sense can be given ("Prolegomena,"[3] p. 51). He adds (pp. 51 and 53) that external phenomena must necessarily and precisely agree with the propositions of the geometer. Whether Kant's allusion to "superficial metaphysicians" points to the Pyrrhonists and Epicureans[4] or to others, and, possibly, even to Reid, whom he had mentioned before (Preface, p. viii.), does not appear. Whatever opinions be formed of Kant's theory, or of the nature of space, his view is impressive. Confine that view to two dimensions, and suppose the surface of a sphere to be inhabited by a being destitute of any conception of a third dimension, and whose senses are unaffected by any point not situated or any motion not taking place on that surface. He could only estimate direction and position by the tangent to the path of the visual ray at the point where that path meets his visual organ, and would think that all objects were situate in one plane. His geometry would be Euclidian; for, if he could form a notion of the actual paths of rays, he would have a conception of the third dimension in space.[5] Here Kant and Riemann would apparently be at issue; for, if a more general conception of space is to be rendered special by actual measurements on the sphere, then, after an enlarged experience, the Euclidian conception would have to be expelled and replaced by some other. And all this would have to be done without praying in aid the excluded third dimension.

Aristotle[6] notices that the nature of everything is best seen in its smallest portions, and Kant[7] remarks that there was a time when mathematicians, who were philosophers too, began to doubt, not the truth of their geometrical propositions as far as they regard space, but the objective validity and applicability of the conception itself, and of all its determinations, to nature; as they were apprehensive that a line in nature might consist of physical points, and that consequently true space in the object might consist of simple parts, though space as conceived by the geometer cannot so consist. Clifford[8] would have given due weight to the doubts of the philosophical mathematicians. He even suggests that the properties of space may change with time. Now, a number may be a function of an angle; the very angle itself determines those numbers (ratios of lines) which we call sines and cosines. But, says De Morgan,[9] in every case but this it is impossible to conceive number a function of magnitude. It seems almost equally difficult to entertain Clifford's conjecture, which, nevertheless, measurements might verify. The sentence, *Nam tempora et spatia sunt sui ipsorum et rerum omnium quasi Loca*, in Newton's Scholium, though it may suggest that omnipresence does not involve extension in space, implies no functional relation between space and time. The words "then and there," accompanying every material allegation in indictments, would suffice to show that the opinions of the world at large on certain characteristics of time and space[1] were in accord with that of the philosophers. Indeed, their isolation, as forms of intuition, may no more be a peculiarity of Kant's system than is his distinction between analytical and synthetical judgments. This distinction was present to the mind of Bacon,[2] as well as to that of Locke, whom Kant cites ("Prolegomena," p. 25), and who, elsewhere than in the place cited, adverts to the distinction. That which Locke had styled a trifling proposition, Kant called an analytical judgment; and that which Locke ("Essay concerning Human Understanding," book iv., chap. viii., Sect. 8) styled a real truth, Kant would would have called a synthetical judgment. With Hume, too, Kant is in some respects in close relation. Hume ("Treatise," vol. i., book i., part 2, pp. 53–124) treats specially of the ideas of space and time. Hume, again ("Inquiry," p. 17; Essay iv., p. 50), distinguishes between results attained by reasonings *a priori* and results arising entirely from experience ("Inquiry," p. 17; Essay, p. 49). He seems to allow conception a sufficiently wide range, for he urges ("Inquiry," p. 13; Essay ii., pp. 26, 27) that, in one exceptional instance, there may be an idea not arising from a corresponding impression; viz. in the case when from the impressions of two distinct shades of a particular colour, a conception is formed of an intermediate shade of the same colour. He asserts ("Inquiry," p. 118) that the only objects of the abstract sciences or of demonstration are quantity and number.

If, as Clifford[3] seems to think, there are no sufficient grounds for maintaining that, if our space has curvature, it must be contained in a space of more dimensions and no curvature, one difficulty is apparently removed. The one-dimensioned time is something very different to space, from which the higher-dimensioned entity might differ still more; and if a solid be treated as the shadow or projection in Euclid's space of, say, a four-dimensioned body, that part of the body which lies outside the shadow seems to have no quality analogous to impenetrability or inertia, nor indeed any quality which affects the senses or deranges the results of calculation. Prof. Cayley says (Southport Address, p. 11) that Riemann's idea seems to be that of modifying the notion of distance, not that of treating it as a locus in four-dimensional space. The suggestion (Cayley, *ib.* p. 10) of a rule changing its length by an alteration of temperature facilitates apprehension. Prof. von Helmholtz has considered the effect of the changes in sensible phenomena which a transition to a spherical or pseudo-spherical world, if such things be, would produce; and he has taken an independent view of the subject in other respects.[4]

De Morgan[5] professed to have been puzzled to know on which side the meeting of parallels took place, or whether on both. He concludes that they never meet. This, however, does not shake, nor is it to be supposed that he wished[6] it to shake, the belief in modern methods, for he apparently admits that interpretation of forms may demand conclusions which can be reached by reasoning on infinity, if increase without limit show approach. He observes that it is clearly conceived by the logicians that all division is reducible to simple dichotomy and its repetitions, and that when the logician has once shown division, difference, he does not trouble himself with the difficulty of repetitions. De

[1] Potts (Robert), "Euclid's Elements of Geometry," &c. (Cambridge and London, 1845); Notes to Book i., p. 41.

[2] Thomas Reid, "An Inquiry into the Human Mind on the Principles of Common Sense" (1764). My pagings refer to the Calcutta Reprint of 1869. Chapter vi. treats (pp. 94–277) of Seeing; its Section vii. (pp. 120–24), of Visible Figure and Extension; and its Section ix. (pp. 132–45), of the Geometry of Visibles. In Section viii. (pp. 125–32), we have Some Queries concerning Visible Figure answered.

[3] I cite from Richardson's Translation (London, 1819); and cannot now give the corresponding paging in that of Prof. Mahaffy.

[4] Montucla, "Histoire" (2de édition, An. vii.), p. 21.

[5] See Cayley, Southport Address, pp. 11, 12.

[6] See Bacon, "Advancement of Learning," p. 108.

[7] Kant, "Prolegomena," p. 52.

[8] William Kingdon Clifford, "Mathematical Papers" (London, 1882). See pp. xl. and xli. of the Introduction, by H. J. S. Smith.

[9] De Morgan, "On the General Principles of which the Composition or Aggregation of Forces is a Consequence" (Camb. Trans., vol. x., part 2, pp. 294, 295, footnote).

[1] I should have been glad to have given Locke's and Kant's descriptions of space and time, and to have compared them with Newton's. But I cannot omit to refer to a Smith's Prize paper, by Mr. Robert Franklin Muirhead, printed in the *Philosophical Magazine* for June 1887, S. 5, vol. xxiii. pp. 473–89.

[2] Bacon, "Advancement of Learning," p. 47.

[3] Clifford, "The Universal Statements of Arithmetic" *Nineteenth Century* (1879, vol. v., pp. 513–22; *vide* p. 522).

[4] A paper in *Mind*, by Prof. von Helmholtz, elicited a criticism from Prof. Land, which produced a reply; and with a brief note appended to a paper on another subject, by Prof. Land, the discussion closed. See *Mind*, vol. i. pp. 301–21; vol ii. pp. 38–46; vol. iii. pp. 212–25 and 551–55; and vol. iv. pp. 591–96.

[5] De Morgan, "On Infinity," &c. (Camb. Trans., vol. xi. Part 1, 1865, pp. 145–89; *vid.* pp. 173, 176, 180, 147). In connection with this paper, the comments of Mr. W. S. B. Woolhouse in the *Educational Times* (Reprint, vol. vi. pp. 49–52) should be considered. And in connection with a paragraph at pp. 161, 162, of De Morgan's paper, the leading paragraph of p. 424 of a previous paper of his, "On the Theory of Errors of Observation" (C. T., vol. x. Pt. 2, 1862), should be read. In the last-mentioned passage he distinguishes between the zero and the indivisible of probability. Hamilton, of Edinburgh, following earlier authorities, expressly restricts the application of logic to finite things. But it does not therefore follow that logicians in general turn a deaf ear to all reasoning upon infinities and infinitesimals, and that they reject results stamped with authority and universally accepted.

[6] This sufficiently appears from a statement at p. 15 of his paper, "On the Root," &c. (Camb. Trans., vol. xi. Pt. 2).

Morgan's remark is easily verified by turning to Potts's Note on Euc. i. 10 (p. 49). Turning again to Boole ("L. of T.," p. 91), it would seem that the logician does not completely detach himself from the notion of infinity: he has to interpret 1 : 0 as well as 0 : 0.[1]

Bacon differs from Plato, who considered forms as absolutely abstracted from matter, and not as confined and determined by it, and agrees with Aristotle in saying that words are the images of thoughts;[2] so that the agreement of the views of Bacon with those of Prof. Max Müller would seem to be tolerably close. It is easy to find cases in which a doubtful meaning of a word may give rise to disagreement on matters of substance. Boole ("L. of T.," pp. 407, 408) observes that the term "necessary" may be applied either to the observed constancy of nature or to the logical connection of propositions. He expresses no decided preference for either meaning. The meanings should be kept carefully apart. If an axiom be a necessary truth, in the strictest sense, then Newton's laws of motion are laws *a priori*, viz. giving Kant's meaning to the term ("Prol.," p. 103); they are known independently of all experience. But Laplace ("Méc. Cél.," pp. 14–18[3]) treats them as results of experience. Moreover, he treats (pp. 65–69) the laws of motion under all the relations mathematically possible between force and velocity. Newton, in fact, usually speaks of "law," and gives the term "axiom" Bacon's meaning.

Boole's chapter xx. ("L. of T.," pp. 320–75) relates to problems on causes, but his use of the word "cause" has given rise to much discussion. He proposed a question on causes in 1851, which was answered by Prof. Cayley in 1853. The solution was criticized by Boole in 1854, who arrived at a different result, and in 1854, Mr. H. Wilbraham examined both solutions. Prof. Cayley returned to the subject in 1862, and Boole thereupon admitted that it would have been better, in stating his problem, not to have employed the word "cause" at all.[4] One mode of stating the nature of the relation between "cause" and "effect" may be this, viz. when a certain (antecedent) change is immediately and invariably followed by a certain other (subsequent) change, then the relation in which the antecedent stands to the subsequent (which may now be called the consequent) change is that of cause and effect. This is, in substance, if not in form, a view common to Algazel, Glanvil,[5] Hume,[6] Brown,[7] Kant, and, as I believe, Reid; for the question seems to be one about words. It differs but slightly from the view (C. T., vol. x., part 2, p. 300) of De Morgan. Perhaps "unvarying" might be a better word than "invariable," for one instant of time is the immediate and invariable antecedent of its consecutive instant; but the idea of "cause" does not seem to arise. When "cause" is used in the above sense, the solutions of Boole and Prof. Cayley agree. Boole's question has been dealt with in our Proceedings (vol. xi. p. 118) by Mr. McColl.

The import of the word "principle" is not the same when we speak of the principle of contradiction or of excluded middle, as when we speak of the principle of the permanence of equivalent forms, or of the sufficient reason, or of continuity. That of sufficient reason has been assailed by Brown ("C. and E.," sect. iv. pp. 222, misnumbered 322, to 306), and by De Morgan (C. T., x., part 2, pp. 290–304). Clifford (*op. cit.*, p. xl.) was prepared to sacrifice the principle of continuity, even in the case of space, and the author of anonymous "Strictures" on Peacock's "Algebra" (Camb., 1837), who was (so at least I was told many years ago by Davies) Hind, concludes (p. 21) that number is perfectly abstract, that it is the only thing which is so, that it is not rightly denominated a species of quantity, being equally connected with every species. An instance of a striking failure of the principle of the permanence of equivalent forms is given by Dr. J. W. L. Glaisher in the *Messenger of Mathematics*, N. S., vol. ii. (1872) p. 95. Again, take another word —viz "disparity." Supposing it to be said that there are two persons in a room, whose united ages are twenty-one years, and between whose ages there is the greatest disparity possible. This is intelligible if one be a new-born or nascent infant, and the other a person aged twenty-one. But suppose the same statement made of three persons; the proficient in language might have to inquire of the mathematician what meaning, if any, the statement bears. Or, again, the mathematician might be asked what, or whether any, numerically definite meaning can be attached to the words, "triangle of maximum scalenity."

Prof. Newman ("Logic," 1838, p. 52) says that the truths of arithmetic are verbal. Perhaps this, and the corresponding statements of Dugald Stewart, would not now be insisted on. They are opposed to the views of Kant, Clifford, and De Morgan (C. T., xi., part 1, p. 160). The identities $3^2 + 4^2 = 5^2$, and $3^3 + 4^3 + 5^3 = 6^3$, seem to be something very different from definitions of words. Kant considers $7 + 5 = 12$ to be a synthetical judgment ("Proleg.," pp. 22, 23).

Metaphysics and mathematics are consorts in the East as well as in the West. Bhascara says that the analytical art is merely sagacity exercised, and is independent of symbols, which do not constitute the art.[1] If De Morgan[2] be right in placing Diophantus as late as the beginning of the seventh century, Aryabhatta was earlier, by two centuries, than Diophantus. The name certainly seems to have been a very common one. Josephus[3] relates that Alexander (a son of Herod the Great) said that Diophantus the scribe had imitated his hand. But Mr. Heath's work[4] renders it scarcely possible to sustain De Morgan's contention.

[1] See the last footnote but one.

[2] Bacon, "Advancement of Learning," p. 143; *conf.* pp. 130, 140. See also pp. 192, 209.

[3] My pagings refer to the 2nd ed. of the "Mécanique Céleste," vol. i. (Paris, 1829).

[4] Boole, *C. and D. M. J.*, vol. vi. p. 286; "L. of T.," pp 321–26; *Phil. Mag.*, S. 4, vol. vii. pp. 29–32; vol. xxiii., pp. 361–63; Wilbraham, *Phil. Mag.*, S. 4. vol. vii. pp. 465–76; Cayley, *Phil. Mag*, S. 4, vol. vi. p. 259; S. 4, vol. xxiii. pp. 352–65, and 470. A short letter by Boole (*Phil. Mag.*, S. 4, vol xxiv. (1862), p. 80, concludes the discussion.

[5] Glanvil (Joseph), "Scepsis Scientifica," &c. (Lond., 1665, 4to): Lond., 1885, 8vo. On Causation. I have only mentioned comparatively recent authors. But, going further back, we find Thales (with his elemental analysis), Xenophanes (with his one cosmic substance), and Pythagoras (with his arithmetical and geometrical combinations), all recognizing invariable sequences in nature; and Socrates admitted a class of phenomena wherein the connection of antecedent and consequent was invariable and ascertainable by human study (Grote, "History of Greece," vol. i., 1846, pp. 495–98). Socrates applied similar scientific reasonings to moral and social phenomena (*ib.*, p. 504).

[6] David Hume, "A Treatise of Human Nature," &c. (Lond., vols. i. and ii., 1739; vol. iii., 1740: his name does not appear on the title-pages). "Philosophical Essays concerning Human Understanding" (2nd ed., Lond., 1750). "An Inquiry concerning Human Understanding" (Lond., 1861) marks the issue to which I refer.

[7] Thomas Brown, "Inquiry into the Relation of Cause and Effect" (3rd ed., Edinburgh, 1818). Draper does not admit the construction put upon Algazel's words by Whewell ("Hist. Ind. Sc.," Lond., 1837, i. p. 251). A facsimile reprint of Glanvil has been published within the last few years. Buckle pronounced Brown's to be one of the best books ever written.

[1] Colebroke, "Algebra," &c. (London, 1817), p. xix.

[2] De Morgan, "Arithmetical Books" (London, 1847), p. 47.

[3] Josephus, "Antiquities of the Jews" (Burder's Translation, vol. i. pp. 616, 617). Burder's preface is dated London, October 1, 1811.

[4] T. L. Heath, "Diophantos of Alexandria: a Study in the History of Greek Algebra" (Cambridge University Press, 1885).

EXHIBITION OF METEOROLOGICAL INSTRUMENTS.

THE Royal Meteorological Society's tenth annual Exhibition of Instruments was held in the rooms of the Institution of Civil Engineers, 25 Great George Street, Westminster, from the 19th to the 22nd instant. This Society's Exhibitions are always interesting and instructive, as each one is devoted to some special class of instruments: this year the instruments consisted principally of actinometers and solar radiation apparatus. Specimens of most of the various forms of these instruments were exhibited; but when it was not possible to obtain an instrument itself, a photograph or drawing of it was shown, so that the visitors to the Exhibition could readily see what instruments have actually been made.

Several specimens were exhibited of Sir John Herschel's actinometer, for ascertaining the absolute heating effect of the solar rays, in which time is considered one of the elements of observation. This consists of a large cylindrical thermometer bulb, with a special open scale, so that minute changes may be easily seen. The bulb is of transparent glass filled with a deep blue liquid, which is expanded when the rays of the sun fall on the bulb. When taking an observation, the actinometer is shaded for one minute and read off; it is then exposed for one minute to sunshine, and its indication recorded; it is finally shaded again, and its reading again noted. The mean of the two readings in the shade, subtracted from that in the sun, indicates the expansion of the liquid produced by the sun's rays in one minute of time.

The Kew Committee exhibited Hodgkinson's actinometer, the principle of which is the same as that of Sir J. Herschel's,

and also Pouillet's direct pyrheliometer, which consists of a cylindrical box of steel filled with mercury, into which the bulb of a thermometer is introduced, the stem being protected by a piece of brass tubing. As the surface on which the sun's rays fall and the quantity of mercury in the cylinder are both known, the effect of the sun's heat upon a given area can be expressed by stating that it is competent in five minutes to raise so much mercury so many degrees in temperature. The Rev. F. W. Stow showed an improved form of Pouillet's pyrheliometer, in which the instrument is placed in a silvered tube to shield it from wind and from all solar rays, except when the tube is turned directly towards the sun. Mr. Casella exhibited Secchi's solar intensity apparatus, in which two thermometers are kept immersed in a liquid at any convenient temperature, and a third, of which the stem passes through the same liquid and the bulb is outside it, is exposed to the rays of the sun shining down the hollow cylinder. The increase of temperature thus obtained is found to be the same, independent of the temperature of the liquid which surrounds the thermometer.

The British Association Solar Radiation Committee showed Prof. Balfour Stewart's actinometer; and Dr. Ångström, of Stockholm, sent one of his pyrheliometers and a photograph of another pattern.

Luvini's dietheroscope for observing the changes of atmospheric refraction optically, and Bellani's lucimeter, as arranged by Prof. G. Cantoni for use at the Italian meteorological stations, were exhibited by the Meteorological Council; and Mr. Hicks showed some of Crookes's radiometers.

Dr. A. Downes illustrated his method of slow actinometry by oxalic acid, in which a definite quantity of a standard solution of oxalic acid is exposed to the action of light for a definite period; subsequently it is used to bleach a standard solution of permanganate of potash. The quantity of oxidized oxalic acid solution, compared with the quantity originally required to produce the same effect, is a measure of the intensity of the light.

Engravings illustrating Violle's, Crova's, and Frölich's actinometers were also exhibited.

The solar radiation thermometer consists of an ordinary maximum thermometer, with the bulb and about one inch of the stem coated with lamp-black, inclosed in a glass shield exhausted of air. Various specimens of this instrument were exhibited, with arrangements for testing the degree of exhaustion. Hicks's black bulb maximum thermometer *in vacuo* is supplied with platinum wires and a battery for testing the vacuum, while Negretti and Zambra's has a mercurial test gauge. Mr. Hicks also showed one of these instruments which had at the end of the outer jacket a second chamber in which is mounted one of Crookes's radiometers for testing the vacuum.

The Royal Meteorological Society showed a pair of black-bulb and bright-bulb maximum thermometers *in vacuo* as recommended for use at the Society's stations; while Messrs. Negretti and Zambra exhibited a similar pair of thermometers mounted in an upright position with the bulbs uppermost, as used at the Montsouris Observatory, Paris.

Mr. Casella showed Southall's helio-pyrometer for testing the accumulated heat of the sun in a confined blackened space, under glass. A black-bulb maximum thermometer is fixed on a cushion at the bottom of a box, the sides of which are also cushioned, and a thick piece of plate-glass is laid upon the top to prevent currents of air carrying off the heat. The box is placed in such a position that the sun's rays may strike as nearly as possible perpendicularly on the glass, when water contained in a small vessel will boil violently in the box.

The practical working of sunshine recorders may be said to date from 1854, when Mr. J. F. Campbell mounted a hollow glass sphere filled with acidulated water in the centre of a bowl of mahogany so arranged that the sun's rays were focussed on the interior of the bowl and burned it. The lines of burning therefore indicated the existence of sunshine. Solid glass spheres were substituted for the hollow ones in 1857, and in 1875 cards in metal frames were substituted for the wood. The Meteorological Council exhibited a number of wooden bowls showing the effect of sunshine by burning in the years 1855–56, 1883–84, and 1887–88; and the Astronomer-Royal sent the sunshine recorder with a hemispherical metal bowl which was in use at the Royal Observatory, Greenwich, from 1876–86. Specimens of the Campbell sunshine recorder with the improved Stokes's zodiacal frame for a fixed latitude, were shown by the Meteorological Council; a recorder with adjustments for use in any latitude, by Messrs. Negretti and Zambra; and the Whipple-Casella sunshine recorder, by Mr. Casella.

Mr. Jordan exhibited an experimental instrument for recording the intensity of daylight, the results being obtained by revolving a disk of sensitized paper behind a screen with a rectangular aperture. Messrs. Negretti and Zambra showed the various patterns of Jordan's photographic sunshine recorder, which consists of a cylindrical box, on the inside of which is placed a slip of cyanotype paper. Sunlight being admitted into this chamber by three small apertures, is received on the paper, and travelling over it by reason of the earth's rotation, leaves a distinct trace of chemical action. In the second pattern of this instrument two apertures are used instead of three; while in the new pattern two semi-cylindrical boxes are employed, one to contain the morning and the other the afternoon record. Prof. McLeod's photographic sunshine recorder was exhibited by Mr. Hicks. This consists of a glass sphere silvered inside and placed before the lens of a camera, the axis of the instrument being placed parallel to the polar axis of the earth. The light from the sun is reflected from the sphere, and some of it passing through the lens forms an image on a piece of sensitized paper within the camera.

Mr. A. S. Marriott showed two patterns of his instrument for comparing the active value of light at different stations; and the Kew Committee sent the chemical photometer devised by Sir Henry Roscoe.

Among the new instruments exhibited were Fineman's and Galton's nephoscopes for observing the direction of motion of clouds; Davis's improved air meters; Negretti and Zambra's recording hygrometer; Casella's Boylean-Mariotte barometer; and de Normanville's self-compensating sympiesometer. Mr. Murday showed in action his apparatus for obtaining readings of an aneroid placed at a distance by means of electric currents. An instrument, called the stephanome, which is used at the Ben Nevis Observatory for measuring the angular size of halos, fog-bows, glories, &c., was also exhibited.

Mr. Clayden showed a very ingenious and instructive working model illustrating the generation of ocean currents, which was a great attraction to all the visitors at the Exhibition. This model shows how the prevalent winds over the Atlantic are the chief cause of the circulation of the waters. A number of tubes are so arranged that when an attached blower is worked the circulation of air produced resembles that of the atmosphere; the imitation winds thus set up react upon the surface of the water, creating a system of currents which reproduces the main features observed in the Atlantic. Special attention was drawn to the Gulf Stream issuing from the Gulf of Mexico, and to the return current flowing eastwards between the equatorial currents. Mr. Clayden also showed some lantern slides illustrating the spiral circulation of the wind in both a cyclone and an anticyclone.

One of the chief features of last year's Exhibition was the large collection of photographs of flashes of lightning which had been gathered together by the Royal Meteorological Society from all parts of the world; this year the Society exhibited a number of similar photographs which have been received since May 1888. Near to these were placed a number of photographs of the electric spark taken by Mr. Wimshurst when the sensitive plate was rotating 2500 times per minute. These flashes are quite sharp and distinct, and show no sign of the movement of the plate.

A very interesting and valuable collection of sixteen photographs taken on the summit of Ben Nevis during the last eleven months were exhibited by the Directors of the Observatory, of which the following were of special interest: (1) cirrus cloud at the northern horizon, taken at midnight at the time of the summer solstice when the clouds are seen brightly illuminated; (2) St. Elmo's fire, at 11 p.m., on the top of the stove-pipe; and (3) views of the Observatory after continued fog and strong wind, but no fall of snow, when everything is covered with long crystals of ice formed out of the fog.

Mr. Bromhead exhibited two large photographs showing the thick rime on trees at Lincoln on January 7 last; and Mr. H. P. Curtis showed a photograph taken by moonlight.

Photographs of clouds were exhibited by Captain Wilson-Barker, Mr. Shepherd, and Captain Maclear.

The Exhibition also included a number of photographs and drawings of instruments, &c., as well as some models of hailstones, 7 inches in circumference, which fell near Montereau, France, on August 15, 1888.

WILLIAM MARRIOTT.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The recent discussion on the proposed new buildings for anatomy and physiology disclosed that the Financial Board do not consider that the whole plan can be proceeded with at present. They hold that only £10,000 is available; but this is because it is proposed to diminish the annual contributions from the Colleges for some years. It appeared to be agreed that the lecture-room or middle block could be best dispensed with if absolutely necessary, the blocks of physiological and anatomical class-rooms and dissecting-rooms being most essential. Prof. Foster mentioned that the labour of conducting the practical classes in physiology was so great as to leave no time for research, and to strain the health of himself and his demonstrators almost to the point of breaking down. The present buildings not only limited but spoiled their work.

The adjudicators of the Hopkins Prize in connection with the Philosophical Society have recommended that it be awarded to Sir William Thomson for his mathematical researches upon the theory of the tides, and other important investigations in mathematical physics.

The General Board of Studies has issued a report deprecating the proposed diminution of College contributions, and showing that that proposal will destroy any chance of appointing new teachers, or increasing the small stipends now paid to University lecturers and readers, or of making any payments in aid of research. A great number of detailed needs for all the departments are specified, the scientific Boards being well represented. The Reader in Botany and the Lecturer in Animal Morphology and others are strongly recommended for immediate increase, and a capital expenditure of £30,000 is needed for museums, laboratories, lecture-rooms, &c.

The following is the subject for the Adams Prize to be adjudged in 1891:—The motion of a satellite about a spheroidal planet, and the reaction on the planet. The ordinary approximation is supposed to be inadequate, either because the ellipticity of the planet is too great, and the distance of the satellite too small, or because the obliquity of the orbit is too great. It is also desired that the influence of a distant disturbing body (such as the sun) may be taken into account in so far as is found practicable. The successful candidate will receive about £170, but is required to print the essay at his own expense.

The report on the local examinations of last December states that in chemistry the answers were on the whole satisfactory, but chemical calculations were in general inaccurately performed. In heat, the juniors answered badly, the senior boys better, but many of the senior girls were quite ignorant of the subject. In statics, dynamics, and hydrostatics, the juniors had not grasped the elementary ideas, while the seniors did better, except in the arithmetic of calculations. The answers seem to show that these physical subjects are not suitable for juniors. In electricity and magnetism, taken only by seniors, the boys did well. The botanical answers varied greatly at different centres, and questions on flowering plants were much better answered than those on cryptogams. In zoology the elements were known, but many answers were very wordy and irrelevant. Several seniors described the structure of a Vorticella rather well, but also named and described the mouth-appendages of a crayfish (the specimen being before them) likewise as a Vorticella.

SCIENTIFIC SERIALS.

American Journal of Science, March.—Some determinations of the energy of the light from incandescent lamps, by Ernest Merritt. Two series of experiments are described, which have been carried out for the purpose of determining what portion of the energy supplied to a lamp is given off as light, and what proportion is wasted practically as dark heat. In the first, based on Melloni's calorimetric method, the light is separated from the dark heat by passing the radiations to be measured through a thin layer of water, or, better still, through a solution of alum in water. The energy of the dark heat, which is almost entirely absorbed, is then measured by the rise in temperature of the water, and that of the light by a thermopile. In the second process the calorimeter was abandoned, and a cell, 1 decimetre thick, containing a strong solution of alum, was used for absorbing the dark heat. The light, after passing through this cell, was allowed to fall on a thermopile, and the deflection was observed. Then the alum cell was removed, and the deflection corresponding to total radiation was observed, the ratio of the two deflections giving the ratio of the light energy to the total energy. This being determined by electrical measurements, the energy of the light could be calculated.—On the ophiolite of Thurman, Warren County, New York, with remarks on the *Eozoon canadense*, by George P. Merrill. This ophiolite, a kind of verdantique marble, is found to be an alteration, or metasomatic product after a mineral of the pyroxene group. Its constitution promises to throw some light on the Eozoon problem.—On the origin of the deep troughs of the oceanic depression; are any of volcanic origin?, by James D. Dana. A general survey of the oceanic regions leads to the inference that volcanic action can only have had a very subordinate part in determining the origin and position of the great marine depressions. Their source must be sought still less in superficial causes, such as erosion, but rather in the interior agencies of primordial development. The paper is accompanied by a bathymetric map of the Pacific and Atlantic, based on the recent charts of the British and United States Hydrographic Departments.—Description of a problematical organism from the Devonian, at the Falls of the Ohio, by F. H. Knowlton. These puzzling organisms, here provisionally named *Calcisphæra lemoni*, from the collector, have been submitted to various American and European palæontologists, and the evidence both for and against the view that they are a fruit of Chara, is given in detail.—Papers are contributed by George H. Williams, on the geology of the Island of Fernando de Noronha (part 2, petrography); by S. L. Penfield, on some curiously developed pyrite crystals from French Creek, Delaware County, Pennsylvania, and on some crystallized bertrandites from Maine and Colorado; and by J. S. Diller and J. E. Whitfield, on dumortierite from New York and Arizona, peridotite from Kentucky, and gehlenite occurring in furnace slag in Pennsylvania.

THE *Memoirs of the Novorossian* (Odessa) *Society of Naturalists*, vol. xiii. fasc. 1, contain a series of papers on the late L. Cienkowski, by P. Boutchinsky, W. Zalensky, L. Richavi, G. Sadkowsky, and S. Karwatzky, being full reviews of the late Professor's extensive scientific work, and giving a full bibliography of his contributions to science.—The next papers of importance are: on the rainfalls in South-Western Russia, by A. Klossovsky; on the copulation of the nuclei of cells during the sexual processes of Fungi, and on the absorption of water by the overground parts of plants, by W. Chmielevsky; on the Jurassic beds of Orenburg and Samara, by I. Sintsoff, being revised lists of fossils found in various parts of these provinces; on the action of methylene-iodide upon the ether of malonic acid, by S. Tanatar; and on the influence of the medium, and especially of temperature, upon *Planorbis vertia*, by Mary Balashova.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 7.—"On the Cranial Nerves of Elasmobranch Fishes. Preliminary Communication." By J. C. Ewart, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Prof. Burdon Sanderson, F.R.S.

This paper contains a short account of the cranial nerves of *Læmargus microcephalus* and of *Raia batis*, and it is especially shown that in connection with the roots of the trigeminal and facial nerves there are altogether five large ganglia—one of them apparently representing two ganglia—and that in connection with the vagus there are three separate ganglia in Læmargus and six in Raia. It is further pointed out that the nerve to the lateral line arises by a special root quite distinct from the rest of the vagus complex, and that it is provided with a separate ganglion, and also that the mucous canals of the head and trunk, together with the numerous ampullæ of the sensory tubes, are either supplied by nerves belonging to what is termed the facial complex or the lateralis division of the vagus complex.

Attention is especially directed in Læmargus to the following facts: (1) that the ganglion of the ophthalmicus profundus lies only very slightly in front of the ganglion (Gasserian) of the trigeminal; (2) that there is no connection between the oculo-motor nerve and the ophthalmicus profundus ganglion; (3) that the ciliary nerves spring from the trunk of the ophthalmicus profundus some distance in front of its ganglion; (4) that neither in

any part of the trunk or main branches of the oculo-motor nerve nor in connection with the branches which pass from the oculo-motor to join the ciliary branches of the ophthalmicus profundus are there any ganglionic cells; (5) that the so-called facial consists of four separate nerves, the ophthalmicus superficialis, buccal, palatine, and hyomandibular—the last receiving a special bundle of fibres while still in the cranial cavity from the ophthalmicus superficialis; (6) that there are numerous ganglionic cells at the base of the palatine nerve—the nerve which is said to correspond to the great petrosal nerve of higher Vertebrates; and (7) that the lateralis nerve supplies the aural mucous canal as well as the canal of the lateral line.

In Raia the following points are brought out, viz., (1) that the ganglion of the ophthalmicus profundus, which is some distance in front of the Gasserian ganglion, lies over the deep branch of the oculo-motor nerve, from which minute branches pass *under* the ophthalmicus profundus ganglion to join two or more of the ciliary branches of the profundus; (2) that the ciliary branches usually arise from the under surface or outer edge of the ganglion of the ophthalmicus profundus, none of them except on rare occasions springing as in Læmargus from the trunk in front of the ganglion; (3) that were the root and trunk of the ophthalmicus profundus greatly reduced in size an arrangement similar to that which exists in the higher Vertebrates would be produced, and the ganglion of the ophthalmicus profundus would appear to especially belong, as has often been taken for granted, to the oculo-motor nerve; (4) that the nerve, generally stated to correspond to the chorda tympani of higher Vertebrates, consists chiefly of fibres which spring from the large ganglion at the base of the hyomandibular nerve; (5) that the ganglion of the lateralis lies several inches (three or four) from the origin of the nerve, and that in addition to supplying the canal of the lateral line the lateralis supplies the dorsal pleural mucous canal, the aural canal, and part of the occipital; (6) that the five additional ganglia of the vagus complex are disposed as follows, one for each of the four branchial nerves, and one for the intestinal nerve.

In this preliminary communication the segmental value of the various cranial nerves is not considered, but it is pointed out that further investigations may show that the ganglia in connection with the superficial ophthalmic buccal palatine and hyomandibular nerves are related to the geniculate, otic, spheno-palatine, and sub-maxillary ganglia of the higher Vertebrata.

Physical Society, March 9.—Prof. Reinold, President, in the chair.—Prof. O. J. Lodge read a paper on magneto-optic rotation by transient currents, with reference to the time required for the production of the effect. If a piece of heavy glass, or tube of carbon bisulphide, be placed between two crossed Nicols, and surrounded by a solenoid, light passes through when a Leyden jar is discharged through the wire. That the discharge is oscillatory, may be proved by turning the analyzer slightly to one side or the other, this having no effect on the result; or the beam may be examined by a revolving mirror, in which case a beaded band is seen when the discharge takes place. When the spark itself is analyzed in the same way, a serrated band results. The frequency of the oscillations being given by the formula—

$$n = \frac{1}{2\pi\sqrt{\frac{LS}{\mu K}}},$$

it is evident that n will be decreased by increasing the capacity S and self-induction L, and this fact was demonstrated by connecting two condensers, first in series and then in parallel, and placing coils of wire in the circuit. The pitch of the sound emitted by the spark was by these means brought within the musical scale. Contrary to expectation, the insertion of a coil with an iron core produced little or no change in the pitch, the reason given being that the induced currents in the skin of the iron wire due to such rapid oscillations of current prevent the interior being magnetized. From the mathematical theory of the brightening of the dark field, it appears that the relative brightness, B, when compared with the light field obtained from the uncrossed Nicols, is given by—

$$B = \frac{1}{\tau}\int_0^\tau \sin^2 \theta dt,$$

where τ is the time during which an impression can be accumulated on the retina, and θ the angle through which the polarized beam is rotated. When θ is considered small,

$$B = 16\pi^2 k^2 n^2 \frac{\frac{1}{2}SV_0^2}{R\tau},$$

where k = Verdet's constant, n = number of convolutions on the solenoid, R = resistance of circuit, and $\frac{1}{2}SV_0^2$ = the initial energy of the static charge. The general solution is given as—

$$B = \frac{1}{2m\tau}\int_0^A \frac{1 - J_0(x)}{x} dx,$$

where $m = \frac{R}{2L}$, $A = 8\pi k n \mu V_0 \sqrt{\frac{S}{L}}$, and $J_0(x)$ Bessel function.

Taking the approximate solution, the question as to what is the best size of wire wherewith to wind the solenoid is considered, and as the insulation is very important, it is concluded that the secondary of a Ruhmkorff coil is very suitable. The main interest of the experiment is said to lie in the evidence afforded of the practical instantaneity of the development of the rotary property in the substance under examination, for Villari (from experiments made on a glass drum revolving in a magnetic field) inferred that a distinct time, between 1/800 and 1/400 of a second, was necessary, whereas Profs. Bichat and Blondlot, of Nancy, have concluded that the time required is less than 1/30000 of a second. The author finds that carbon bisulphide is able to show the effect when the rate of alternation is 70,000 per second, and has no reason to believe that glass is in any way inferior. As a possible explanation of Villari's results, he suggests that the strain due to centrifugal force would modify the components of the polarized beam, and produce elliptic polarization. Mr. Ward mentioned that experiments similar to Villari's were now being carried out at the Cavendish Laboratory, a disk of glass being rotated about two hundred times per second by means of a turbine. The results so far obtained do not confirm Villari's, but, owing to difficulties in keeping the speed constant, it is difficult to make exact measurements. It has, however, been found that the strain due to centrifugal force rotates the plane of polarization, and elliptically polarizes the beam; and that passing an alternate current round a stationary glass bar produces a distinct rotation of polarized light passing through it. Referring to the oscillatory discharge of a jar, Prof. Rücker directed attention to Dr. E. Cook's experiments, described before the Society in June 1888, when photographs showing the dust-figures produced by sparks were exhibited, and pointed out that the frequency required to produce air-waves of the length there indicated was of the same order as the rate of oscillatory discharge—viz. about one million per second. Prof. Rücker also wished to know whether glass behaved precisely like CS_2. Dr. Lodge said his experiments were not exact enough to decide the latter question, and mentioned that Mr. Chattock had, some time ago, produced dust-figures in tubes by jar-discharges, and shown that the wave-length depended on the capacity and self-induction. Prof. Ayrton suggested the use of a phonograph as a means of recording and reproducing the oscillations, in the same way as himself and Prof. Perry have analyzed the current-curves of alternating dynamos. The discharge could be passed through a small coil fixed to a diaphragm, and placed near a coil through which a steady current was passing, the attractions and repulsions serrating the surface of the rapidly revolving cylinder. By means of a mirror attached to a delicate magnifying spring, the section of the surface may be determined. He also inquired whether the experiments shown do prove that the effect is instantaneous. Dr. Thompson remarked that it was satisfactory to learn that Villari's results admit of an interpretation other than by time effect, and thought it advisable to vary the experiment by rotating a bar of glass; but Mr. Ward said he attempted that experiment four years ago, and abandoned it on account of the enormous speed required.—Dr. Lodge showed some experiments allied to those of Hertz, and pointed out that all the effects were due to resonance. The plates of an air-condenser were connected by a wire loop, and placed near a Holtz machine in action. On adjusting the distance between the plates to a particular value, sparks were observed to pass between them; but, on increasing or decreasing that distance, the sparks ceased. It was also shown that the sparking was interrupted if the connecting loop was replaced by a coil, though the coil was effective when connected to a condenser of smaller size, thus demonstrating that the time-constant of the condenser circuit was all-important. Another important condition to be observed in such experiments is, that the receiving circuit must be closed, except at the sparking place, so as to permit the surgings of the electricity to take place freely. Other experiments were shown, in which two spheres provided with rods terminating in knobs were used as a Hertz's oscillator, and sparks could be obtained from straight pieces of wire of

suitable length held at some distance from the spheres. Dr. Lodge remarked that sparks could be obtained from almost any scrap of wire or metal in close proximity to an induction-coil and oscillator; but, to produce the effects at considerable distances, careful timing of the receiving circuit was necessary. The author also mentioned the results of some experiments on the velocity with which electric waves travel along wires, and concluded (contrary to Hertz) that this was not greatly different from the velocity of light.—Owing to the late hour, the other papers announced to be read were postponed.

Entomological Society, March 6.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—Mr. F. P. Pascoe exhibited several specimens of the Saüba Ant (*Œcodoma cephalotes*) from Parà, carrying portions of dried leaves. It seemed questionable whether the leaves were collected by the ants for the purpose of making their nests, or for the sake of some Fungus which might be growing on them.—Mr. Jenner-Weir exhibited, and read notes on, specimens of a Butterfly (*Tirumala petiverana*) from Mombaza, Eastern Africa.—Mr. J. H. Durrant exhibited a living larva of *Cossus ligniperda*, which had entirely lost its ordinary colour, and had become first pink and then white. He attributed the change, and subsequent loss, of colour to the fact that it had been deprived of its natural food, and fed for eighteen months on pink paper, with which the box in which it was kept was lined, and subsequently on white cardboard. Mr. McLachlan remarked that the most extraordinary peculiarity about this larva, in addition to the loss of colour, was the absence of the usual odour of *Cossus*. Lord Walsingham observed that it was questionable whether the colours of larvæ were dependent on the colours of their surroundings, or whether they were affected by the contents of the intestinal canal. Prof. Meldola, F.R.S., said that the caterpillar exhibited, having eaten the pink paper, had most probably become dyed by the colouring matter, and he did not think the observation had much bearing on the question of the protective colouring of caterpillars. It was well known to physiologists that certain dye-stuffs could be introduced into the tissues of animals by mixing the colouring-matters with the food, and paper was frequently stained with coal-tar dyes, such as eosin, magenta, &c., so that it was simply a case of direct dyeing of the larva.—Mr. B. A. Bower exhibited a specimen of *Parasia neuropterella*, bred from heads of *Centaurea scabiosa*, and said he believed the species had not been previously bred. He also exhibited series of *Coleophora olivaceella*, *C. solitariella*, and *Laverna subbistrigella*.—Mr. White exhibited a series of male and female specimens of *Orgyia thyalina*, obtained by the late Mr. H. J. Pryer in Japan. Some of the females had their wings fully developed, and some of them were semi-apterous, as is usual with the females of this genus. Mr. White remarked that he knew of no other species of the genus in which the females had fully-developed wings. Lord Walsingham, Prof. Meldola, and Mr. R. South took part in the discussion which ensued.—Lord Walsingham exhibited specimens of preserved larvæ of *Eupithecia extensaria*, from King's Lynn; also a preserved larva of *Smerinthus ocellatus* and one of *Sphinx ligustri*. The larva of the last-named species was a variety, and the President remarked that it was the only one of this species he had ever seen.—The Secretary read a communication from the Rev. Dr. Walker, announcing his intention of making an expedition to Iceland this year, from June 23 to July 29, and asking that any entomologists who might wish to accompany him would send him their names.—Mr. Distant suggested that the meeting should pass a resolution expressing regret at the death of the Rev. J. G. Wood.—Mr. Gervase F. Mathew, R.N., communicated a paper entitled "Descriptions and Life-histories of New Species of Rhopalocera from the Western Pacific."

Zoological Society, March 5.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February 1889, and called attention to four Marbled Polecats (*Putorius sarmaticus*), presented by Colonel Sir Oliver B. C. St. John, new to the collection; and to a fine specimen of Owen's Apteryx (*Apteryx oweni*) from the South Island of New Zealand, presented by Prof. T. Jeffrey Parker.—Mr. A. Thomson exhibited a series of insects reared in the Insect House in the Society's Gardens during the past year, and read a report on the subject.—Prof. G. B. Howes exhibited and made remarks on some specimens of the embryo of *Myrmecobius fasciatus*.—Mr. O. Thomas exhibited a specimen of a new Muntjac from Tenasserim, lately discovered by M. Fea, and proposed to be called *Cervulus feæ*.—A communication was read from Mr. Joseph S. Baly, containing descriptions of some new South American Coleoptera of the genus *Diabrotica*.—A communication was read from the Rev. H. S. Gorham, containing descriptions of some new species and a new genus of the Coleopterous family Telephoridæ, from Eastern Asia. Thirty-nine new species and one new genus (for which the name *Lycocerus* was proposed) were described. Of these new forms, the greater part were from India and China.—Colonel R. H. Beddome read a paper on new land-shells from the Island of Koror (Pelew Group), based on specimens collected for Dr. Hungerford by a resident in that island. The series comprised examples of eight new species of the genus *Diplommatina*, of two new and very curious species of *Endodonta* (a section of *Helix*), and of a remarkable new genus, allied to *Diplommatina*, proposed to be called *Hungerfordia*.—Mr. W. E. Hoyle read a paper on the anatomy of a rare Cephalopod (*Gonatus fabricii*), originally discovered by Fabricius in the last century, but little known in recent times. The author gave a general description of the anatomy of the species, and recorded the existence of several tracts of cartilage hitherto unobserved in the Cephalopoda. Some details were given regarding the structure of the pen-sac and the development of the pen, as well as some new facts reregarding the structure of the funnel-organ, and a suggestion regarding its function. The genus was regarded as being somewhat more nearly related to *Onychoteuthis* than to *Enoploteuthis*, but as much further removed from them both than they are from each other. The creation of the sub-family Gonatidæ was thus held to be justified.

Mathematical Society, March 14.—Mr. J. J. Walker, F.R.S., President, in the chair.—The following papers were read:—Notes on plane curves: iv., involution-condition of a cubic and its hessian; v., figure of a certain cubic and its hessian, by the President (Mr. E. B. Elliott in the chair).—The problem of duration of play, by Major MacMahon, R.A.—Some results in the elementary theory of numbers, by Mr. C. Leudesdorf.—The characteristics of an asymmetric optical instrument, by Dr. J. Larmor.—A new angular and trigonometircal notation, with applications, by Mr. H. MacColl.

EDINBURGH.

Royal Society, March 4.—Sir W. Thomson, President, in the chair.—A paper, by Dr. J. Oliver, on deductive evidence of a uterine nerve-centre and of its location in the medulla oblongata, was communicated.—The President exhibited a gyrostatic model of a medium capable of transmitting waves of transverse vibration. The model was two-dimensional, but a three-dimensional model could readily be constructed on the same principle.—Dr. Thomas Muir read a paper on the relation between the mutual distances of five points in space. He has reduced Cayley's determinant to one of the fourth order.—Dr. Muir also communicated a note, by Prof. Tait, on the relation among four vectors. In this note Prof. Tait gave an investigation of the same problem by means of quaternions. His result can be interpreted in two ways; one interpretation leads to Dr. Muir's result, while the other gives the well-known relation among the sides and diagonals of a spherical quadrilateral.—Dr. Muir exhibited a diagram illustrating the history of determinants.—Dr. Noel Paton and Dr. Ralph Stockman communicated a paper on the metabolism of man during starvation.

March 18.—Dr. John Murray, Vice-President, in the chair.—Prof. Haycraft read a contribution, written by Dr. Harold Scofield and himself, to the chromatology of the bile.—A paper by Prof. Tait on a relation between two groups of four vectors was read. When the two groups are identical, the result reduces to that obtained by him in his paper read at last meeting. When one spherical quadrilateral is the polar of the other, the relation reduces to $\cos Ab \cos Bc \cos Cd \cos Da = \cos Ac \cos Bd \cos Ca \cos Db$. Cayley's determinant can at once be obtained from the identity $\Sigma x(\alpha - \theta)^2 = \Sigma x\alpha^2 - 2S\theta\Sigma(x\alpha) + \theta^2\Sigma(x)$, where there are five vectors $\alpha_1 \ldots \alpha_5$, and $\Sigma(x) = 0$, $\Sigma(x\alpha) = 0$, by replacing θ by the various vectors α in turn and eliminating the x's from the resulting equations by aid of the equation $\Sigma(x) = 0$.—A paper by Mr. John Aitken, describing a portable apparatus for counting the dust particles in the atmosphere, was read. This apparatus is constructed on the same principle as his former one, but various improvements have been made. The paper also included an account of some of Mr. Aitken's observations with the large apparatus. It is pointed out that when much dust is pre-

sent in the atmosphere the heat of the sun is greatly absorbed. Hence it seems probable that dust particles may aid in the formation of fogs in another way than by acting as nuclei. Their great radiating power will cause rapid cooling of the air, and so produce saturation.—Dr. Sims Woodhead communicated a paper, by Mr. R. W. Gray and himself, on the stomach of the narwhal.—Dr. Crum-Brown read the third part of a paper, by Dr. A. B. Griffiths, on micro-organisms.—A second note by Prof. Tait, on the virial equation, was read.

PARIS.

Academy of Sciences, March 18.—M. Des Cloizeaux, President, in the chair.—On the fixation of nitrogen during the process of slow oxidation, by M. Berthelot. The object of these researches is to determine the fixation of nitrogen during the slow oxidation especially of those principles that give rise to certain intermediate oxides endowed with mixed oxidizing and oxidizable properties which temporarily fix free oxygen, afterwards transforming it to an almost indefinite extent to other bodies capable of definite oxidation. Such are ordinary ether, the essence of terebenthine, various aromatic hydrocarbons, and other substances capable of producing those effects which their discoverer, Schoenbein, attributed to ozone.—On the heat of formation of antimoniuretted hydrogen, by MM. Berthelot and P. Petit. The formation of this extremely unstable compound is described, and its heat of formation determined by six experiments at – 84·5 calories.—On the essays that have been made to explain the fundamental principles of thermodynamics by mechanical laws, by M. H. Poincaré. The paper deals mainly with the views developed by Helmholtz in his memoirs on the statics of the monocyclic systems, and on the principle of least resistance (*Crelle's Journal*, vols. xcvii. and c.). M. Poincaré accepts the mechanical explanation as satisfactory for the reversible phenomena, but shows that it is not applicable to those of the irreversible order.—On certain fourfold periodical expressions depending on two variables, by M. E. Picard. In this note the author indicates certain series depending on two independent complex variables, and possessing in relation to them four couples of conjugated periods.—On the movement of a material point on a sphere, by M. Gustave Kobb. In his treatise on some applications of the elliptical functions, M. Hermite has reduced the integration of the equations in the movement of the conic pendulum to the integration of Lamé's differential equation. Here M. Kobb shows that there also exists another kind of movement of a material point on a sphere which leads to a similar application of Lamé's equation.—On the elastic equilibrium of arches forming arcs of circles, by M. Ribière. Two typical cases are worked out mathematically, which offer a complete solution of the problem of the elastic equilibrium of circular vaults.—On the solubility of salts, by M. H. Le Chatelier. The author replies to some critiques on his own researches (*Comptes rendus*, vol. c. p. 50), made by M. Bakhuis Roozeboom, in the remarkable work recently published by him on the solubility of salts.—On the chloride and bromide of copper, by M. Denigès. The author describes a simple process for preparing these substances by means of the haloid salts of the alkalies and the sulphate of copper. The same chemist indicates a new and characteristic reaction of the salts of copper, the principle of which rests on the easy transformation of these salts into cupric bromide under the influence of potassium bromide, and on the dehydratation of the resulting salt by means of sulphuric acid.—Researches on the saccharine substances contained in certain species of mushroom, by M. Em. Bourquelot. These researches have been made on eight species belonging to the genus *Lactarius* of Fries, and to Sowerby's *Boletus aurantiacus*. The proportion of mannite was found to vary from 1·90 to 15 per cent., according to the different species, and sometimes in the same species from season to season. From *Lactarius piperatus* a substance was obtained identical with M. Berthelot's trehalose, the presence of which in mushrooms had already been indicated by M. Müntz.—On the physiological and therapeutic action of orthomethylacetanilide, by MM. Dujardin-Beaumetz and G. Bardet. This substance, which has recently been prepared by M. Brigonnet under the name of exalgine, with formula $C_9H_{11}NO$, is toxic, therapeutic, and anæsthetic, according to the dose administered. In these respects it greatly resembles antipyrine, but appears to be superior as a cure for all forms of neuralgia.—Thermic classification of fresh-water lakes, by M. F. A. Forel. Lacustrine basins are here grouped as tropical, temperate, and polar, according as the surface waters are always above, about, or under 4° C. respectively. But with this grouping is combined the variation of temperature due to depth, this variation increasing with the shallowness of the lake. Further modifications are caused by special climatic conditions, such as altitude, latitude, aspect, volume, so that from the thermic stand-point every fresh-water basin has its special features. All are comprised in six broad classes, based, however, mainly on the two more important elements of surface temperature and depth.

BERLIN.

Physical Society, February 22.—Prof. von Helmholtz, President, in the chair.—Prof. Neesen demonstrated several pieces of mechanical apparatus which he is in the habit of using in his lectures to illustrate and explain the parallelogram of forces, the laws of inertia, and the action of friction with special reference to the slipping of locomotives. He further described several arrangements connected with mercurial air-pumps by which some of their defects and inconveniences may be avoided.—Dr. Wolff gave an account of the results of a long series of measurements which he had made on galvanic cells, consisting of zinc and zinc sulphate or chloride and a second metal, either copper, silver, or iron. By determining the electromotive force of each cell and the simultaneous heat-production (by means of an ether calorimeter), he endeavoured to prove that the source of the current-energy in each case is due to the combining of oxygen with the several metals, copper, silver, or iron. He hence considered himself justified in giving the name "oxygen-elements" to the above class of galvanic cells.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Chemical Lecture Notes: P. T. Austen (Wiley, New York).—The Scientific Transactions of the Royal Dublin Society, vol. iv. (series 2), ii. A Monograph of the Marine and Fresh-water Ostracoda of the North Atlantic and of North-Western Europe; Section 1., Podocopa: G. A. Brady and Rev. A. M. Norman; iii. Observations of the Planet Jupiter: O. Boeddicker; iv. A New Determination of the Latitude of Dunsink Observatory: A. A. Rambaut (Williams and Norgate).—The Patents, Designs, and Trade Marks Acts, 1883–88, consolidated, with an Index: L. Edmunds (Stevens).—Longmans' New Atlas: edited by G. G. Chisholm (Longmans).—British Dogs, Nos. 27, 28, 29: H. Dalziel (U. Gill).—Travel-Tide: W. St. Clair Baddeley (Low).—Naturalistic Photography: P. H. Emerson (Low).—Stellar Evolution and its Relations to Geological Time: J. Croll (Stanford).—Naturalist's Voyage round the World: C. Darwin (Murray, 3*s.* 6*d.*).—Northern Afghanistan: Major C. E. Yate (Blackwood).—Modern Cremation: Sir Henry Thompson (K. Paul).—Cosmic Evolution: E. A. Ridsdale (Lewis).—Physical Geography and the Climate of New England: W. M. Davis (Camb., Mass.).—Proceedings of the Academy of Natural Sciences of Philadelphia, October–December 1888 (Philadelphia).

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THURSDAY, APRIL 4, 1889.

A "PRACTICAL MAN" ON ELECTRICAL UNITS.

AT the last meeting of the British Association an energetic attempt was made to prove that the progress of the human race has been chiefly due to the "practical man," and this teaching was quickly caught up and explained to mean that the triumphs of industry have been achieved without the help of workers in the field of pure science. We have before us a periodical which is instructive reading when viewed in the light of the discussion on this subject. It is a recently issued number of the Transactions of an Institute connected with one of the most important of our national industries. Among other provincial organizations it holds high rank. Meetings are held at frequent intervals, papers are read, and not only are they printed at length, but the discussions by which they are followed are also given in full. Two papers were recently communicated to this Society on the application of electricity to the industry with which its members are chiefly connected. One of them was by a gentleman who, according to the Chairman, represented an important firm "who have done more of this class of work in this country than anyone else," and he added that on this account "anything that he may say will carry great weight and give information to us unattainable otherwise, and I am sure we shall therefore appreciate the more, the trouble he has been at in coming here." The whole of the last number of the Transactions of the Institute—forty-one pages in all—is filled with the discussion that followed the speech in which this passage occurred.

We give these details because we wish to make it clear from the outset that the thing which we are about to discuss was not done in a corner. No circumstance which could add to the formality and importance of the occasion was wanting. A full account of the whole is published, and is matter for public comment.

Let us now see how the gentleman thus introduced and who calls himself a practical man utilized this opportunity. In the course of his remarks he discussed the theory of the electric motor, and then proceeded to say that he was frequently asked

"why we measure electrical quantities in volts, amperes, and watts, rather than in foot-pounds. Well, the main reason is that the practical units, the *volt*, *ampere*, and *ohm*, are so easy to measure and so simply connected by the equation

$$C = \frac{E}{R},$$

in which C = the ampere, the unit of current;
E = the volt, „ „ pressure;
R = the ohm, „ „ resistance.

Now, the product of one *ampere* and one *volt* = one *watt*, and 746 *watts* = one horse-power.

"In the mechanical units 33,000 foot-pounds = one horse-power per minute; and if we are doing electrical work at the rate of 746 watts per minute, we are doing 33,000 foot-pounds per minute. The electrical unit of work is then related to the mechanical unit by the ratio $\frac{746}{33,000}$, or one *watt* is equal to 4·4 foot-pounds.

"We could thus measure all electrical quantities in foot-pounds if it were desirable, but it is far more convenient to measure the volts and amperes, and then estimate the horse-power. If anyone, however, wishes to express electrical quantities in foot-pounds, he will now be able to do so; but bear in mind that the electrical horse-power is equal to the mechanical brake horse-power."

It is hardly necessary to point out the blunders with which every sentence of this passage teems, but it is necessary that they should be made evident to "practical men." To talk of measuring a volt or an ampere in foot-pounds is as ridiculous as to propose to measure miles or gallons in seconds; and yet, when the inquirer asks why it is not done, he is told that "all electrical quantities could be measured in foot-pounds if it were desirable."

In the equation C = E/R, the symbols, when applied to the practical system of units, do not represent the ampere, volt, or ohm, but certain numbers of amperes, volts, and ohms.

The "horse-power per minute" is an old friend; it has as much meaning as the statement that a man has walked four miles per hour per second. Of course, the equally absurd phrase "watts per minute" follows.

Next, we find the "electrical unit of *work*" confounded with the unit of power, and the statement that their ratio is 746/33000; the truth being that the ratio between the watt and the horse-power is 1 : 746. As the first of these fractions is nearly seventeen times greater than the second, this sentence, in so far as it means anything, makes the watt seventeen times too great.

Lastly, like a cockney, who, having put in unnecessary *h*'s, proceeds to redress the balance by leaving them out where ordinary mortals insert them, the speaker, having liberally distributed "per minute" where it makes nonsense, proceeds to leave it out where it is absolutely necessary. The statement that "1 *watt* is equal to 4·4 foot-pounds" is wrong, (1) because the ratio of 33,000 to 746 is 44, and not 4·4; (2) because it is necessary to add "per minute" after "foot-pounds." The "practical man" of this type apparently thinks that it is quite unimportant whether a machine does 44 foot-pounds of work in a second, or in a minute, or in all eternity.

In drawing attention to this extraordinary series of statements, it is necessary to point out that nobody desires to interfere with such engineers provided they confine themselves to fulfilling the useful function of putting together machines they do not understand, and learning by bitter experience, but, be it well understood, at other people's expense, practical wrinkles which may no doubt often be of great service.

When, however, they pose as electricians, go down to the provinces as instructors of the ignorant, and print their opinions on "Sir William Thompson's" (*sic*) address to the Institute of Electrical Engineers, the matter becomes serious.

The speech from which we have quoted was characterized as "clear and explicit." A "junior member of the Institute enjoyed this lecture more than any we have had since I have joined," and thought that "we have had a very instructive meeting."

Thus these guileless representatives of a great British

industry sit agape while enjoying phrases which have as much meaning to them, and to the speaker, as "that blessed word Mesopotamia" to a village crone. But, surely, there must be a limit to their endurance. Even from the point of view of the most practical of "practical men" it must be a serious thing to find, in one line containing eleven words and ten figures, the ratio of the watt to the horse-power made seventeen times larger than it really is, a decimal point misplaced so that the watt is made ten times less than it really is, the vulgar fraction from which the ratio is deduced in decimals inverted, and the essential statement as to the time in which the work is done omitted in a calculation of power.

If men are to be "practical" and nothing else, they must at all events be accurate. If they are to use formulæ which they do not understand, they must at all events know how to use them correctly. If the representative of a great firm—in explaining the answer to a question, which is not sprung upon him unawares, but which he himself puts forward as one which he has been "frequently" asked, and to which, therefore, he volunteers a reply—can in addition to employing language which makes all his statements nonsense, turn a fraction upside down, misplace a decimal, and, finally, pass the report of his speech for press with these blunders uncorrected, how can outsiders avoid suspecting that similar mistakes may be not infrequent in calculations upon which specifications and contracts are based, and on which very "practical" questions of success or failure, and of pounds, shillings, and pence, depend?

And now for the application of all this. The speaker gave as his reason for using volts and ohms that they are "so easy to measure, and so simply connected." Do the supporters of the "practical man" think that this easy measurement, this simple connection, came by accident? Do they think that this system which they find so useful could have been elaborated by men who, when it has been before the world for years, cannot open their mouths or put pen to paper, without showing in every sentence that they are absolutely ignorant of the fundamental conceptions on which the whole system is based, and equally incapable of using it correctly?

In the course of the evening the same speaker claimed for himself and for practical men that "we don't want to know what [electricity] is, but what it will do." He, and such as he, have yet to learn that what electricity has done is mainly the outcome of the work of men who did want to know what it is, being certain that if they knew that they could make it do more than under any other conditions.

They elaborated a system of units which our authority finds *easy* and *simple*, by the aid of investigations which even now require a good knowledge of mathematics and physics on the part of those who would really understand them, and which at the date of their original performance were masterpieces which only intellects of a very high order and knowledge of a very wide grasp could have achieved. Among them were numbered some engineers, but these ranked among them not because they were practical men who did not "want to know what electricity is," but because they had risen above such wretched cant, and had become not only "practical" but scientific.

The mischief done by the Bath meeting is not yet ended. It may or may not be a good joke to discuss whether Sir William Thomson is or is not an engineer. But the views then expounded are, all over the country, leading so-called "electrical engineers," who are ignorant of all that concerns what is, by their own confession, the easy part of their subject, to fling their cheap sneers at men who do "want to know what electricity is," who have made it possible to use and measure electrical quantities, and who have directly or indirectly created the very trades by which their detractors earn their daily bread.

"We don't want to know" will be the ruin of British industry, unless its leaders use their influence to crush the spirit indicated by this expression. In the March number of the *Fortnightly Review*, Lord Carnarvon relates that chairs, of which the various parts are fastened by glue, as is the custom in this country, will not hold together in the warmer climate of Australia. English makers did not know, perhaps did not "want to know, this. "The Austrian manufacturers, on the other hand, had discovered the cause of the defect, and, by a very simple alteration in the fastening, had practically driven out of a large part of the country our home-made furniture." "Wherever I went," says Lord Carnarvon, "I observed that, as a matter of fact, German, and not English, furniture was in use." And so, while the columns of every newspaper are full of the unity of the Empire, and of the unemployed, another tie between mother-country and colony is broken, another outlet for British industry is closed, because our manufacturers do not know what the Austrian discovers for himself.

It is all of a piece with this that in England, in the year of grace 1889, an electrical engineer, who is, as we gather from the Chairman's statement, no tyro or underling, but who was welcomed at an important meeting as a worthy exponent of the views of a well-known firm, was not ashamed to tell his hearers that he does not "want to know what electricity is," and that he could "measure all electrical quantities in foot-pounds."

THE CEPHALOPODA.

Catalogue of the Fossil Cephalopoda in the British Museum (Natural History), Cromwell Road, S.W. Part I., containing part of the Sub-order Nautiloidea, consisting of the Families Orthoceratitidæ, Endoceratidæ, Actinoceratidæ, Gomphoceratidæ, Ascoceratidæ, Poterioceratidæ, Cyrtoceratidæ, and Supplement. By Arthur H. Foord, F.G.S. Pp. xxxii. and 344, and Fifty-one Woodcuts. (London: Printed by Order of the Trustees, 1888.)

JUST as heraldry in the Middle Ages formed a necessary part of the education of every knight and noble, without which it would have been impossible to trace the connection of the great families whose genealogy was symbolized on banner, shield, and crest, so palæontology is essential to the biologist, if he would successfully trace the connection of the living forms around him with their remoter progenitors whose records must be sought for in rocks of Palæozoic age.

Of such high lineage are the Cephalopoda, whose ancient life-history Mr. A. H. Foord has essayed to write in the carefully-prepared volume before us. There is evidently a fascination about the nautilus and cuttle-fish family, which seems specially to attract the attention

of naturalists. The living animals of the cuttles and squids are remarkably vivacious, as well as cosmopolitan, whilst the Octopus, or "devil-fish," has been invested with quite supernatural powers and intelligence. As to the gigantic calamaries of the North Atlantic, they almost realize in size De Montfort's fancy sketch of the "colossal poulpe" seizing a three-masted ship in its arms; or the fabled "Kraken," described by Dr. Paullinus and the Bishop of Bergen in the last century as a beast so huge that a regiment of soldiers could conveniently manœuvre on its back!

Striking and varied as are the animals of living Cephalopods, their shells, both recent and fossil, are of immense interest, indeed they are unsurpassed for elegance and variety of form by any of the Molluscan sub-kingdom; and as we have seldom the other parts left to us—especially in the older rocks—save the shell alone, it becomes necessary to study these structures with increased attention, and strive to elicit from them all that is possible of the past life-history of their inhabitants, and thus, by the light which they afford us, to trace the origin of the allied living forms.

The labours of naturalists during the past fifty years have tended to eliminate certain groups formerly classed with the Mollusca, and thus to define more clearly the characters of this great phylum of the Cœlomata as now recognized. The first group to be removed from the Mollusca was that of the Cirripedia by the labours of J. V. Thompson in 1830. In 1866, Kowalewsky showed that the Tunicata had affinities with the Vertebrata, and that their agreement with the Mollusca was only superficial. In 1844, H. Milne-Edwards had placed the Polyzoa with the Brachiopoda and Tunicata in a large group, the "Molluscoidea"; but the investigations of Cardwell, in 1882, showed that the Polyzoa and Brachiopoda had only a delusive agreement with the Mollusca, and must be removed from that phylum also.

There now remain, according to Prof. Lankester, only two great branches of the Molluscan phylum; namely, (1) the GLOSSOPHORA (characterized by possessing an odontophore), embracing the Gasteropoda, the Scaphopoda, and the Cephalopoda; and (2) the LIPOCEPHALA (= *Acephala*, Cuvier), including all the Lamellibranchiata (mussels, oysters, cockles, clams, &c.), without any definite head.

The question that interests us most to-day is, To which of the Mollusca belongs the honour of representing the primitive type from which all the varied forms we now recognize have arisen? Leaving out of consideration the earlier Brachiopoda, as having been already excluded from the Mollusca, we find in the older Palæozoic rocks that the Pteropoda, Heteropoda, Nautiloidea, and Lamellibranchiata (or Lipocephala) appear almost contemporaneously. But the Pteropoda (represented by *Theca* and *Conularia*), and the Cephalopoda (by *Orthoceras sericeum*, and at least three other species), begin in the Tremadoc rocks; Lamellibranchs (such as *Palæarca* and *Ctenodonta*) in the Arenig; whilst Gasteropods of several well-marked genera (*Murchisonia*, *Pleurotomaria*, *Euomphalus*, *Trochus*, &c.), with *Bellerophon* and the strange *Maclurea*, are found in the Bala series. If, as appears from the views of Prof. Lankester, we are to regard the *Lipocephala* as degenerated forms of *Glossophora*, they must have begun very much earlier indeed to have become so differentiated as we find them in the Arenig group. Nor do the Gasteropoda of the Bala series present the appearance of primitive forms (unless it be the genus *Maclurea*), for we find mollusks with turreted, turbinate, and discoidal shells, already defined as distinct generic types.

That the Pteropoda preceded the higher Cephalopoda in time seems pretty certain, and that both of these preceded the Gasteropoda seems established; but of the priority of the latter over the Lamellibranchiata there is no evidence.

As Prof. Lankester, in his recent classification of the Mollusca, places the Pteropoda with the Cephalopoda as Branch A. *Pteropoda*, Branch B. *Siphonopoda*, we must be content, for the present, to consider that the Cephalopoda represent the most ancient type of Mollusca, and that the shells of the little Pteropod, *Theca*, are the earliest representatives which we at present know.

In *Orthoceras* we become acquainted with the first and simplest form of camerated Cephalopod shell. They were straight shells, with plain suture-lines marking the septa, the siphuncle varied in position, the septa being concave towards the aperture; the initial chamber was conical, with a cicatrix, the body-chamber large, and its aperture simple (pp. 1–128). Nearly 200 species of these straight simple shells are described by Mr. Foord, ranging from the Tremadoc shales of Portmadoc to the Trias of St. Cassian. That they were external shells is proved by their surface-ornamentation, consisting of transverse and longitudinal ridges, and fine decussating striæ, with occasional colour-bands and markings rarely preserved.

In *Endoceras* (pp. 129–63) the internal structure of the shell is varied by the undulating character of the septa, which bend downwards, and overlap the neck of the preceding septum, forming a complete shelly siphuncular tube. The siphuncle, moreover, is eccentric in position, and often half the diameter of the shell. Within this wide siphuncle a series of funnel-shaped conical sheaths (endosiphons) have been observed, of the nature of which we are at present left in doubt. *Piloceras* has also an unusually large siphuncle, within which a series of invaginated sheaths, similar to *Endoceras*, occur.

The genus *Actinoceras* presents other peculiarities in the structure of the shell. Within the siphuncle, which is very large, a slender tube passes down the centre, called the endosiphon (pp. 164–99). The siphuncle expands between each septum into a broad bead-like dilatation, perforated around its periphery by a series of minute shelly radiating tubuli given off from the endosiphon. It has been suggested by Owen that these were connected with the vascular system of the animal, and were designed to convey nutrition to the lining membrane of the septal chambers. No such complex siphon and endosiphon exists in the shell of any living Cephalopod; nevertheless, it is difficult to accept for them the interpretation here suggested, unless we would invest the shells of these ancient Mollusca with a structure akin to that of the skeletons of the Vertebrata!

The huge siphuncles of *Huronia*—allied to *Actinoceras*—have been described by Stokes, Bigsby, Hall, and

Woodward (pp. 199–207). The septa and shell-wall are thin, and but rarely preserved. These weathered tubes were frequently noticed by Dr. Bigsby standing out in relief from the limestone cliffs of Drummond Island on Lake Huron, and were as large as the vertebra of a man, and not unlike them in shape, and over 6 feet in length.

In the family of the *Gomphoceratidæ* (pp. 211–45), we meet with shells ranging from nearly straight, through varying degrees of curvature, to one in which a complete whorl is attained. The aperture of the shell in this family is so contracted that it is obvious the animal could not have withdrawn its head into its body-chamber as does the living *Nautilus*. The mouth of the shell is T-shaped, and reminds one of the mouth of the shell in some land-snails, like *Helix globulosa* and *Auricula scarabæus*, which are so guarded by tooth-like projections from the margin of the aperture as seemingly to preclude the animal from ever retreating into its shell, or emerging from it if withdrawn.

The *Ascoceratidæ* (p. 246) have the test of a sac-like form, the body-chamber extending to the lower end of the dilated portion of the shell, while the septa adhere to the dorsal wall, and bend upwards with their convex side towards the mouth of the shell. The apex was unknown, the shell being always found truncated, but Barrande, first, in Bohemia, and subsequently Lindström in Sweden, have discovered the earlier apical portion of the shell of *Ascoceras*, which was nearly straight, and with the septa normal, as in *Orthoceras* (p. 335).

The *Cyrtoceratidæ* (pp. 262–318) are more regular in their growth than the preceding; the shell is more or less curved, and tapers rapidly, or more slowly, according to the species or variety examined. The siphuncle is small, and varies in position in the different species, being external, internal, or sub-central; they range from the Carboniferous to the Tremadoc series, and are well represented in the Devonian of Gerolstein, Eifel, by large and handsome forms.

In his introduction the author discusses many points of great interest relating to the class, as, for example, the classification, the structure of the shell, the range in time, and the distribution of the group.

Seventeen genera and 403 species are described, but there yet remain the *Lituitidæ*, *Trochoceratidæ*, *Nautilidæ*, and *Bactritidæ*, to complete the NAUTILOIDEA; while the AMMONOIDEA and the DIBRANCHIATA will be treated of still later on.

Mr. Foord writes:—

"The classification of the Nautiloidea adopted in this volume will be found to differ in some of its details from systems hitherto employed, the more recent writings of Noetling, Zittel, Mojsisovics, and Hyatt having furnished the basis of the changes introduced. The arrangement of the groups described in the following pages is primarily zoological, secondarily stratigraphical, each genus being dealt with separately, from its appearance to its extinction."

The author passes in review the various systems of classification of the Cephalopoda proposed by Prof. Hyatt, Dr. Paul Fischer, Barrande, and other writers on this group.

"Hyatt considers that the generic terms *Cyrtoceras*, *Gyroceras*, *Lituites*, *Nautilus* are merely 'descriptive terms for the different stages in the development of an individual, and also the different stages in the development or evolution of the adult forms in time. In other words, each of these genera, as now used, includes representatives of all the different genetic series of Tetrabranchs, which are either young shells in the corresponding stage of growth, or adult shells in the corresponding stage of evolution.' He finds 'that genetic affinities on a large scale are best exhibited by the siphuncle, particularly by the funnels of the septa, which are more invariable than any other part of the shell.'

"He next discusses the embryonic relations of the structure of the septa and of the siphuncle, and mentions the difference between the Nautiloids and the Ammonites exemplified in these structures, the one commencing with a globular initial chamber ('protoconch' of Owen), the other with a conical initial chamber and a cicatrix. He remarks that generally among the Palæozoic Cephalopod types much greater differences exist, in regard to the septa, the position of the siphuncle, and so on, than among the Mesozoic forms, thus indicating that the evolution of forms was quicker in the Palæozoic epoch than at subsequent periods, and from these circumstances he concludes that 'types are evolved more quickly, and exhibit greater structural differences between genetic groups of the same stock, while near the point of origin, than they do subsequently.' It must not be forgotten, however, that the Palæozoic epoch was of much longer duration than the Neozoic. 'In the smaller divisions (families and genera) of Hyatt's scheme of classification, an important place is assigned to the characters of the sutures for distinguishing the different groups. In some groups, however (notably the Orthoceratidæ), the less stable characters presented by the ornamentation of the shell are for a like purpose employed.'

"While there can be no question as to the value of Prof. Hyatt's work, and the thoroughness of research which he has brought to bear upon the class he has with so much boldness and originality attempted to re-classify, the extremely revolutionary nature of the changes he has proposed in the minor divisions of his system (involving the wide separation of many forms hitherto associated together) challenges the inquiry as to whether our knowledge of the developmental history of the Cephalopoda is not as yet far too imperfect to justify such a radical departure from existing systems. The suppression of the familiar names *Cyrtoceras* and *Gyroceras* seems quite unnecessary, and seeing that the names *Orthoceras* and *Nautilus* are retained, in a restricted sense, in Hyatt's scheme, there seems to be no good reason why the two former should not have been similarly used" (Introduction, p. vii.).

Mr. Foord discusses at some length the nature of the camerated structure of the Cephalopod shell, and the question as to whether the *camera* should be called "air-chambers" or "water-chambers"; he concludes to avoid the difficulty by calling them "septal chambers." Bearing in mind the fact that each sealed-up chamber of the shell is but the partitioned-off lower portion of the animal's body-chamber, it is obvious that it must, at the moment of separation, contain the same medium as that which envelopes the animal.

In the case of the living *Nautilus*, dredged by the *Challenger* off Matuku Island, in 320 fathoms,[1] it seems improbable that the "septal chambers" could have been full of gas when the animal was crawling upon the sea-bed at a depth at which the pressure would be equal to about 750 pounds on each square inch of surface, or fifty-three times greater than at the sea-level. Any such

[1] See "Notes of a Naturalist on the *Challenger*," by H. N. Moseley, M.A., F.R.S. (p. 297).

inclosed gas-filled chambers would have sufficed, by their buoyancy, to bring the shell and animal at once to the surface.

That the living Nautilus, noticed by Prof. Moseley, should have been unable to sink in the tub of sea-water in which it was placed on the deck of the *Challenger*, and that this inability was due, as he observes, "to some expansion of gas in the interior, occasioned by the diminished pressure," is equally certain; but Moseley does not state that the expanded gases were in the shell-chambers; had such been the case, the gas, in order to expand, must have ruptured the rigid shell-wall. But gases are, no doubt, evolved within the crop and alimentary canal of the animal, and these, by their expansion, on coming to the surface, would suffice to produce the effect observed by Moseley. "The living specimen," he says, "seemed crippled, and unable to dive, no doubt because it had been brought up so suddenly to the surface from the depths" (*op. cit.*, p. 298). Exactly similar effects were observed in fishes with "swim-bladders." These "come up" (says Moseley), "in the deep-sea dredge, in a horribly distorted condition, with their eyes forced out of their heads, their body tense and expanded, and often all their scales forced off" (*op. cit.*, p. 580).

Mr. Foord quotes an observation by Dr. Woodward, "that many dozens of specimens of newly-imported shells of *Nautilus*, examined by him at the Docks, were, when shaken, all found to contain fluid within their chambers, just as in the camerated shell of the Water Spondylus (*S. varians*)" (Introduction, p. xiii.). We cordially indorse Mr. Foord's remark that "it is much to be regretted that recent opportunities of setting this question (of the contents of the septal chambers) at rest should apparently have been neglected."

What we would strongly insist upon is, that, it being admitted on all hands that the Cephalopoda are, in every respect, Glossophorous Mollusks, their shells must, in a similar manner, be found to conform to the ordinary Molluscan type. The striking regularity of their septal chambers has usually hindered a comparison with those of other camerated Molluscan shells; but in *Caprinella*, the camerated interior of the "water-chambers" is quite equal in regularity and symmetry with that of the Cephalopoda, and many of the *Hippuritidæ* show not only septa, but a pseudo-siphuncle, reminding one still more of the chambered Nautilus.

Space does not permit a longer notice of Mr. Foord's excellent "Catalogue"; it is a most valuable addition to the now really fine series of descriptive Catalogues issued by the Trustees of the British Museum of Natural History. We hope soon to welcome the appearance of the second part of this useful work, when we may be tempted to reopen the question of shell-growths and shell-structures.

SANITARY SCIENCE.

Transactions of the Sanitary Institute of Great Britain. Vol. IX. (1887–88.)

THIS volume is largely composed of the papers and addresses read at the Congress of the Institute at Bolton. The authors are chiefly men well known in their different professions, whose contributions are not only of much scientific interest, but carry weight with the public. Sanitation is a science in which the "faddist" delights to dabble, but his effusions have but little representation in the volume before us. The majority of the workers appear to be content to record steady advances in knowledge, or to make practical suggestions for administrative reform, rather than to air brilliant theories, popular with the public for their novelty, but greatly wanting in substantial proof. To force premature conclusions in sanitation, as in other older sciences, is to retard true progress; and on the whole it may be said that sanitary reformers, however earnest, are content to preach the doctrine of pure air, earth, and water, which is as old as Hippocrates.

In the department of practice, improved sanitary administration is urged on all hands. Such reforms take largely the shape of what has been called "interference with the liberty of the subject"; an interference, however, which is not unnecessary or uncalled for, but is imperative in health matters where the act or default of one individual may imperil the lives of many. Those who on this plea found their objections to such measures as compulsory vaccination, compulsory notification of infectious disease, isolation of infectious disease in hospitals, better supervision of building operations, dairies, and cow-sheds, and stricter enforcement of nuisances clauses and sanitary regulations generally, appear to forget that civilized existence depends upon the observance of mutual obligations, and that society could not exist if every individual were free to exercise his liberty of action at the expense of the community. Little is thought of the restrictions that already exist, and to which all law-abiding citizens cheerfully adhere, but when, with advancing knowledge of cause and effect in disease, certain measures are pointed to as being necessary to avoid unwholesome conditions, or to prevent the dissemination of epidemics, an outcry is raised which is too often not only illogical but insincere.

In the section of chemistry, meteorology, and geology some valuable papers are contributed on the application of bacteriology as a means of obtaining evidence as to the purity of water-supplies, and of ascertaining the degree of contamination of the air of buildings and sewers. As a science, bacteriology is still in its infancy, but already its teachings are producing a most profound effect in the domains of medicine and hygiene. The recognition of the bacterial and fungoid organisms as the principal factors in the processes of fermentation and putrefaction of organic substances, and the discovery that certain specific microbes are the actual agents provocative of certain contagious diseases, have secured a basis on which can be founded rational measures for the prevention and alleviation of disease and for the control of insanitary conditions. Hygiene was a science practised with most beneficial results before the discoveries of Pasteur, Koch, and Tyndall, but many of its teachings and precepts were at that time empirical, although founded more or less on experience and observation. With the more definite knowledge of disease causation and dissemination now arrived at, it is most satisfactory to find that the measures of sanitary reform and improvement which have marked the latter half of the present century are almost entirely in accord.

At the close of the Congress addresses were given to the working classes of the town of Bolton. The Sanitary Institute appears to fully recognize the desirability of bringing home to the minds of all classes of the community the importance of healthy homes and temperate living; and the lesson it should aim at inculcating is that if the public sanitary authorities can do much for the working man, he can do for himself by his own efforts—if he only knew how—very much more to improve his own surroundings, and bring up his family in health and comfort.

The annual visits of the Institute, for the purpose of holding Congresses, to the large towns of this country, must rank as one of the most influential means of popularizing sanitary science. The proceedings of the Congress are watched with interest by persons whose attention it is not easy otherwise to engage; whilst the exhibition of sanitary appliances brings under their notice the latest improvements in domestic and municipal sanitation, and is an incentive to manufacturers to turn out none but the most approved articles.

It also appears that the Institute holds examinations and grants certificates to local surveyors and inspectors of nuisances to sanitary authorities. These examinations should prove of the greatest use to the public sanitary service, for the certificate is a guarantee that the holder possesses sound knowledge on sanitary subjects; and there can in future be no reason why Local Boards should appoint incompetent and ignorant officials, when certificated candidates for office present themselves.

GLEANINGS IN SCIENCE.

Gleanings in Science. By Gerald Molloy, D.D., D.Sc. (London: Macmillan and Co., 1888.)

THIS is a pleasantly-written book, containing ten popular lectures which have, from time to time, been delivered by the author to popular audiences. They were, with one exception, delivered under the auspices of the Royal Dublin Society. The lectures are not offered as containing anything new, but simply as an attempt to popularize some of the most important of modern developments in physical science.

The author has, in our opinion, been thoroughly successful. In a short space he has been able to put before his hearers and his readers, in a satisfactory and thoroughly intelligible manner, a great variety of most important scientific facts and principles. The subjects of the lectures are well chosen.

First, we have two lectures on "Latent Heat," in which the fundamental discoveries of Black are explained and the great results which have flowed from them are recounted. Three lectures on electrical subjects follow. Then we have two lectures on "The Sun as a Storehouse of Energy." Two lectures on the Electric Light enable the author to lay before his readers in a popular but scientific manner the principles of dynamo-machines and the modes of producing the light from the electric current. The transformations of energy which take place during production of the light are also explained. The last lecture is on the Glaciers of the Alps.

The illustrations, which take the place of the experiments of the original lectures, are for the most part suggestive and satisfactory. The lecture on glaciers contains three or four striking, if somewhat sensational, woodcuts by Mr. Whymper, whose Alpine sketches are well known.

A book of the kind we have described is scarcely likely to be absolutely free from blemishes, particularly of style; and there are two or three to which we would call the attention of the author. One of them, at any rate, can be easily amended in another edition. It is the habit of saying "indefinitely small" when he means either infinitely small or extremely small. This use of the word indefinite he shares with some, sometimes indefinite, writers on certain branches of mathematics and dynamics. But indefinite does not in pure nor in common language mean infinite; much less can "indefinitely small" be put for "extremely small," as in speaking of the length of the path of a molecule. There is nothing indefinite about the path of a molecule any more than about the path of a billiard ball. Again, there is nothing "inconceivable" about the velocity of the radiant energy of the sun (p. 191). It is measurable and well known. The pruning down of some exaggerated language would make the wonders of science all the more wonderful.

A lover of Nature is apt to get into difficulties if he invokes her aid too frequently in descriptions of the physics of our universe. When we read of Nature dealing out the sun's energy *to man* with the prodigality of a spendthrift, we are apt to think of the much more prodigal way in which the sun's energy is poured out all round. The energy which comes in our direction is a very small proportion of the whole. There are too many references to personified Nature in these lectures.

The parts which seem to us least satisfactory are the early part of the lecture on storing of electrical energy, and the classification of the forms of energy contained in the next lecture. The enumeration of various forms of energy available to man is very imperfect. For example, energy of chemical separation is omitted from the list, and yet dynamite, gunpowder, and the gas-engine, are surely worthy of mention. Dynamite is manufactured as electrical energy is; but an engine worked, say, by native petroleum, would be far more efficient than the *earth currents* which Nature provides, but which, as our author properly remarks, are more of a trouble than a pleasure. In another place "bags of oxygen and hydrogen" are referred to as examples of stored-up heat. This seems a little far-fetched, to say the least of it. Just about the same place a rod of "chalk" is used to produce the lime-light.

Perhaps, however, it may seem somewhat ungracious to prolong the list of minor corrections. The book is most readable, and is deserving of praise throughout.

OUR BOOK SHELF.

The Gamekeeper's Manual; being an Epitome of the Game Laws of England and Scotland, and of the Gun Licences and Wild Birds Acts. By Alexander Porter, Chief Constable of Roxburghshire. Second Edition. Pp. 120. (Edinburgh: Douglas, 1889.)

ASSUMING, and we are not in a position to affirm or deny the assumption, that the legal points laid down in this little book are sound, it will certainly fulfil its author's

intention, which, he tells us, is "to place within the reach of all connected with the protection of game a knowledge of the law which it is their duty to administer," and we may hope, also in his words, that it may contribute "to a more effective suppression of a form of lawlessness which leads to so many crimes of a more serious nature." Seeing that it is a second edition, that the author is the head of the constabulary of an important Border county, and that in compiling it he has had the assistance of other chief constables, Mr. Porter is probably right on all points that have been decided. Within his prescribed limits he traces his subject, as it seems to us, very well, though tersely enough, beginning with the earliest records of both Kingdoms, and ending with the silly (but well-meant) Sand Grouse Act which closed the labours of the last session of Parliament—an admirable instance of locking the stable-door after the steed was stolen. However, his treatment is so purely from an executive aspect that comments upon the book are hardly suited to these pages. Yet we may remark that the game law question, which, not so many years ago, was a party cry, has through certain modifications of opinion ceased to occupy that position. There are now few reasonable men who do not perceive that if our ever-increasing population is to continue the enjoyment of the delicacies which "game" (using the word in a wide and not a legal sense) affords, some sort of preservation of such "game" is a necessity. The practical extermination, in many large districts, of *Lepus timidus* since the passing of the Act of 1880 (43 and 44 Vict. cap. 47), leading to the almost prohibitive price of hares in our markets, has fully shown this. The way in which the principle of preservation should be applied is, of course, quite another thing. All men nowadays agree in condemning the savagery of the early laws, though many are apt to forget that it was only of a piece with the savagery of other contemporary laws; but until within the last twenty years few enactments, whether in Great Britain or Ireland, grasped the scientific truth which has been at the bottom of the most recent legislation (the Ground Game Act excepted), and should be at the bottom of all—namely, take care of the parents, and you may leave the offspring to take care of themselves.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Meteoric Theory of Nebulæ, &c.

MY last letter in NATURE of March 7 (p. 436) substantially contended that there must be a certain limit to the rate of translatory motion possible to meteoric masses, beyond which they would be rapidly resolved into vapour and consequently be unable to maintain the weight of the heaped-up material constituting the nebula (supposed to be formed of meteorites). It appears tolerably evident that there must be such a limit as to size to which the system can apply, if a period anything like "a few thousand years" mentioned[1] by Prof. G. H. Darwin is to be accorded to this stage of evolution. The doubt was expressed whether the size (or mass) of the original solar nebula was not past that limit, in view of the rate of translatory motion required, viz. $5\frac{1}{2}$ kilometres per second in the mean.

That the theory is in principle true, appears but little open to doubt. For when masses fall in a confused manner to a centre, the kinetic movement is naturally and inevitably produced, and corrected so as to be maintained symmetrically in all directions by the masses themselves. The main question seems to be, What is the limiting mass of the future sun to which such a system can apply in its completeness, so as to allow anything like permanence enough for the forcible tendency of the energy to equalize itself to extend to a notable radial distance (to settle down into a sort of temporary kinetic equilibrium, that is)? It is conceivable that in the case of an eventual very large sun, the system of meteorites may be heaped up to a certain height, and then collapse at or near the nucleus, where the density is greatest, and consequently the impacts most numerous. Such a partial collapse would give rise to violent oscillation in the nebula, and perhaps assist the throwing off of rings (query). The fact that meteorites are exceptionally the only celestial masses that we can handle and analyze in our laboratories, gives a basis of certainty to inquiries about them, which lends a special interest to theories as to the part played by them in Nature.

In my last letter I avoided the use of the term "elasticity," considering purely the physical conditions which constitute its basis. Prof. G. H. Darwin remarks on this point:—"It may, however, I think, be shown that the very greatness of the velocities will impart what virtually amounts to an elasticity of a high order of perfection" (NATURE, p. 82). "Ordinary elasticity must be nearly inoperative" (p. 107). To produce this exceptional degree of elasticity, the expansive action of the gas generated by the high velocity of impact is relied on, which is regarded to act as "a violent[1] explosive introduced between the two stones." No doubt the volatilized gases due to the collision expand and assist rebound; but in estimating its value we must consider if the chilling of the gas, in doing the work, is at all the equivalent of the heating. If the gas is left in an incandescent state (as is probable, from spectroscopic evidence also), that represents so much spent work, so much[2] imperfect elasticity. The gas is not confined as in a gun, and so cannot exert its expansive effect to full advantage.

Meteorites constituting a nebula having a mean velocity of $5\frac{1}{2}$ kilometres per second represent an energy about eighty times that of a cannonade. Taking the velocity of an ordinary projectile at 2000 feet per second, that of the meteorites ($5\frac{1}{2}$ kilometres) is 18,000 feet per second, about. Meteorites normally composed of materials imperfectly welded together, cannot stand the same knocking about as steel projectiles. If it were imagined that the latter could have a velocity of 18,000 feet per second imparted to them, they would doubtless leave a luminous track in the atmosphere from friction, and the energy of their mutual collision at this speed may be imagined. It must be remembered, however, that the collisions are generally oblique or glancing.

No doubt in a nebula a metallic rain flying in all directions like the meteorites would accompany the motion. This rain of metal we may suppose under favourable accidents to collide together to form nuclei in various parts of the nebula; and an incipient nucleus constituting a sort of shelter would collect more rain (or hot metal), and so constitute a new meteorite—much as the occasionally dissociated lumps or molecular clusters of a compound gas reunite in another part of the gas, so that the mean state of aggregation remains the same.

The mean interval between encounters depends, of course, on the mean size of the meteorites. It appears from the data afforded that thirty days or a month would be the average interval between collisions (at about one-third radius of the supposed original solar nebula), for meteorites of mean mass equal to $3\frac{1}{3}$ kilogrammes, which would possess a volume of about half a cubic decimetre.[3] So that a nucleus, which we may suppose at rest or only slowly moving—as it is, on the average, struck equally on all sides by the metallic rain—would have, as a mean, a month to collect or grow before the chance of being disturbed by a collision in a nebula under the conditions named. In this way, doubtless, nuclei could be formed, and the meteorites renewed.

The question of the applicability of the theory would seem to

[1] Abstract of Mr. G. H. Darwin's paper "On the Mechanical Conditions of a Swarm of Meteorites," appeared in NATURE, November 22 and 29, 1888 (pp. 81 and 105). The complete paper is in the Philosophical Transactions, vol. clxxx., 1889.

[1] The entire passage is: "It must necessarily be obscure as to how a small mass of solid matter *can* take up a very large amount of energy in a small fraction of a second, but spectroscopic evidence seems to show that it does so; and if so, we have virtually what is a violent explosive introduced between the two stones" (NATURE, November 22, 1888, p. 82).

[2] It will be seen afterwards that this radiation must not be considered as absolutely lost, but in great part radiated to another region of the nebula of meteorites.

[3] For meteorites of $3\frac{1}{3}$ tonnes (about half a cubic metre in volume), the interval between collisions would be 300 days, nearly a year; and the mean length of path 130,000,000 kilometres, nearly the sun's distance. Multiplying the mass by a thousand (or the diameter by 10) increases the interval between collisions and the length of path ten times. At this rate we should get to the stellar distances and stellar movements, regarding the visible universe as a nebula, if a merely speculative remark may be allowed.

be one of degree rather than of principle. For, even if its application for the full time required for that stage of evolution, in the case of so large a nebula as the original solar one, were considered doubtful, there is no objection apparently on that head to be adduced in the case of smaller nebulæ, or comets (the smallest class of nebulæ); in which latter case there is some support to the theory afforded by experiment and observation.

In regard to the question of "elasticity," the only resource, in my view, is to abandon this idea, in the ordinary sense of that term (which conveys the idea of retention of form), and suppose that there may be complete disintegration by the collisions at times,[1] welding or fusion together at others; so that the mean degree of aggregation remains constant so long as the translatory motion remains constant. This idea of the existence of a number of possible mean states of aggregation of matter between the extremes of complete integration into one mass, and complete disintegration into molecules (the states being dependent on the rate of translatory motion), was thrown out by me in NATURE (vol. xix. p. 461);[2] also, further, in the *Philosophical Magazine*, August 1879, p. 153.

According to this view, it is implied that "cohesion," as a central force, can play the same part as "chemical action" under translatory motion, and produce fluctuations about a mean state of aggregation—just as in a compound gas, for example, even at normal temperature, the small lumps of matter which move as wholes in the motion of translation are disintegrated now and then, and integrated elsewhere, the *mean* state (only) of the gas remaining unchanged.

The above, with the exception of the last two paragraphs, was written before the appearance of Prof. G. H. Darwin's reply (March 14, p. 460), to some criticism I ventured to offer (March 7, p. 436) on certain points of his theory in my first letter. I would make a few remarks in addition here.

I am not quite able to agree with the view of M. E. Minary, expressed in a paper brought before the French Academy on February 18, of which an abstract appeared in NATURE of February 28, p. 432, and is referred to in Prof. Darwin's letter. The chief passage, as given in the abstract, is as follows:—

"The gases being perfectly elastic bodies, and in the upper atmospheric regions in an extremely rarefied state, heat cannot be produced by the shock of bodies endowed with great velocity and impinging on perfectly elastic molecules capable of receiving the motion and acquiring the velocity of these bodies: in this case the movement is communicated, not dissipated or transformed into heat."

This view is opposed, I venture to think, to the deduction we may draw from such an experiment as that with the "fire syringe," where the air in a cylinder is inflamed by suddenly compressing a piston. What is the flame here observed through the tube of glass due to? It is due, of course, to the *vibrations* of the molecules of air, which break the ether up into waves, and so affect the eye. From the fact that the air molecules are "perfectly elastic," it becomes impossible for a moving body to impinge against them violently without throwing the molecules into energetic vibration, which is the physical basis of "radiant heat." The same must occur on a greater scale, as it appears, when the air is compressed by a flying meteorite, although I accept M. Cornu's suggestion (quoted in the abstract, p. 432) that the *luminosity* observed may partly be of electric origin.

In Joule's "Scientific Papers," vol. ii., experiments in association with Sir William Thomson are described, of whirling thermometers through the air (attached to a lathe). The experiments, which were numerous, gave 163·7 as the velocity in feet per second; on the average, equivalent to a rise of temperature of 1° C. (vol. ii. p. 316). Whirling a thermo-electric junction attached to a reflecting galvanometer was tried with consistently the same result as the thermometer (p. 310). These experiments were made partly[3] with the view to test the theory of the heating of meteorites. The temperature was found to be as the square of the velocity. The law of Clausius, that the translatory motion and the vibratory motion (which latter motion alone affects the eye and senses as radiant heat) of the molecules of an ordinary gas are proportional to each other, has—as Prof. Darwin allows—been experimentally verified through[1] a considerable range of temperature. To my mind it appears obvious that these two forms of motion (translatory and vibratory) must be interconvertible and mutually sustain each other. When the gas, for example, is exposed to the pulsations of ether waves (radiant heat), this vibratory motion is first taken up by the molecules, but part of it is converted into translatory motion, as proved by the rise of pressure. If, on the other hand, the translatory motion of the molecules of gas be augmented, part of this is instantly, as we know, converted into vibratory motion, the source of radiant heat. S. TOLVER PRESTON.

Paris, March.

[1] A special property of iron, which may have importance here.

[2] It would seem even curious to my mind, if there were no intermediate state between these two extremes of complete integration and complete disintegration, by a varying rate of translatory motion (or energy). It may be observed that the radiant energy set free so abundantly at a collision is not lost, but radiated in great part to another region in the nebula (and there absorbed).

[3] Also see vol. i. pp. 399, 536. The temperature was found to be independent of the form and size of the thermo-electric function, and was assumed as evidently independent of the density of the air. No doubt the more air there is the more there is to heat. But it nevertheless seems plain, I think, that the temperature of a meteorite must rise higher in dense air than in excessively rarefied air. How is this to be explained? Each individual molecule of air in striking the moving meteorite, is thrown into violent vibration, and this (temperature) is independent of the number of air molecules evidently. But in dense air, no doubt, the heat accumulates much faster than it can be radiated away, and so the temperature of the meteorite attains a final *maximum*, which is greater the denser the air is. The temperature may probably be appreciably constant within certain limits of variation of density; but it appears obvious that in excessively rare air the temperature developed must be less, owing to rapid dissipation in space by radiation.

[1] The ratio of Maxwell referred to in my last letter, and which Prof. Darwin remarks in a footnote (p. 460) was stated inaccurately by me—was not, I may explain, the one quoted by him from Maxwell's "Theory of Heat." It is contained in Maxwell's paper "On the Dynamical Evidence of the Molecular Constitution of Bodies" (NATURE, vol. xi. pp. 357 and 374). After alluding to the law of Clausius, Maxwell remarks:—"In 1860 I investigated the ratio of the two parts of the energy on the hypothesis that the molecules are elastic bodies of invariable form. I found, to my great surprise, that, whatever the shape of the molecules, provided they are not perfectly smooth and spherical, the ratio of the two parts of the energy must be always the same, the two parts being in fact equal" (p. 375).

The Molecular Formulæ of Aluminium Compounds.

IN a letter to NATURE, December 27, 1888 (p. 198), I gave a tabulated statement of the numerous vapour-density determinations of halogen, and a few other compounds of aluminium and the allied metals, and pointed out what appear to me to be the legitimate conclusions to be derived from the experiments, regarding the molecular formulæ of these compounds.

Since then two interesting articles have appeared in NATURE (pp. 447 and 495), in which accounts are given of determinations of the vapour-densities of aluminium acetyl acetonate, $Al(C_5H_7O_2)_3$, and aluminium methide, $[Al(CH_3)_3]_n$. The simple formula given for the first compound has been proved to be correct, at any rate for the conditions under which the experiments were made. The results given for aluminium methide are: calculated for $Al_2(CH_3)_6$, 4·98; calculated for $Al(CH_3)_3$, 2·49; observed (10° above the boiling-point under atmospheric pressure), 3·92.

What I ask permission to call attention to and to criticize is the conclusion drawn by the author of these articles from the experimental results. Speaking generally, the conclusion may be stated in this way: Molecules of the formula MR_3 do exist, *therefore* molecules with the double formula M_2R_6 do not. I confess that I am wholly unable to appreciate the force of the argument. Must we take the existence of the molecule NO_2 as a proof of the non-existence of N_2O_4?

That I have not stated the argument unfairly may be shown by quotations from the article (p. 495). Speaking of aluminium acetyl acetonate the author says: "It is supremely satisfactory that in this case the density, at a temperature only 45° above the boiling-point, was found to actually correspond precisely with that required by the triad formula, *precluding again the possibility of the existence of molecules of the type* Al_2R_6." And previously, after giving the results for aluminium methide, notwithstanding the fact that the observed density 3·92, obtained by Quincke, corresponds rather more closely to the higher than the lower formula, the author remarks: "*Hence it can no longer be doubted that molecules of the double formula are incapable of existence.*" The italics in both cases are mine. The conclusion I should draw from all the experiments with aluminium compounds is this: The experiments of Deville and Troost, Friedel and Crafts, and Louïse and Roux, prove conclusively that molecules of the higher formula Al_2R_6 are capable of existence; the results obtained by Nilson and Pettersson, and by Buckton and Odling, point also to the existence of molecules of the lower formula, but further proof was certainly needed, and this has now been afforded by the valuable experiments of M. Alphonse Combes with aluminium acetyl acetonate.

In conclusion, it appears to me that the close relationship of the metals aluminium, gallium, indium, iron, and chromium, makes it probable that molecules of both formulæ M_2R_6 and MR_3 may be capable of existence in the case of all these metals, even though the conditions necessary for the stability of the higher molecules have not yet been attained for indium and chromium chlorides. That the conditions have been attained in the case of the chlorides of aluminium, gallium, and iron, appears to me to admit of no doubt. SYDNEY YOUNG.

University College, Bristol, March 23.

Luminous Night-Clouds.

IN accordance with my anticipations (see *Astr. Nachr.*, 2885), luminous night-clouds have now been seen at the southern point of South America. Herr Stubenrauch, meteorological observer at Punta-Arenas, writes to me that he twice saw the phenomenon in December 1888. According to the same observer, it was noticed several years ago by a naval officer in the Beagle Channel, rather to the south of Punta-Arenas. The description given by Herr Stubenrauch leaves no room for doubt that the phenomenon is identical with that observed in Europe.

Sternwarte, Berlin, March 28. O. JESSE.

Zodiacal Light Observations.

COULD any of your readers furnish me with a list of observations of zodiacal light, or refer me to any record where such a list can be found? I applied to the Meteorological Office some years ago, and found that no such list was to be obtained. All I require is the precise time of the display, the place of observation, and any other mathematical observations concerning the angle of inclination, &c., which may be relied on.

W. DONISTHORPE.

32 Pembridge Villas, W., March 30.

Vapour, or Meteoritic Particle.

ABOUT 6.30 p.m. on Friday last, while out in the country, I observed a large meteor falling slowly and almost vertically in the north-north-east. After it had disappeared, half-way between the zenith and the horizon, I noticed, in the strong twilight, a faint phosphorescent trail, which did not disappear, but changed to a straw-colour, like a streak of vapour illuminated by the setting sun. Gradually the shaft expanded in the centre and contracted at the ends, until it assumed the shape of a balloon. Continuing to change, it flattened out horizontally like a football, and apparently about half the size; and it looked a little brighter as it became more condensed. This process occupied quite half an hour, and the object remained like a nebulous patch lit up by the sun's rays, until that orb was well below the horizon, finally disappearing about 7.15 p.m. That part of the sky was perfectly clear—there were no clouds except the usual dense bank on the horizon. F. B.

Rugby, March 27.

The Satellite of Procyon.

WITH reference to Mr. Barr's letter on p. 510, I would mention that Mr. Burnham has lately observed Procyon with the 36-inch refractor at the Lick Observatory, and finds no trace of any close companion with powers up to 3300. H. SADLER.

March 29.

RECENT RESEARCHES ON THE RARE EARTHS AS INTERPRETED BY THE SPECTROSCOPE.[1]

IF I name the spectroscope as the most important scientific invention of the latter half of this century, I shall not fear to be accused of exaggeration. Photography has rendered vast services in recording astronomical and biological phenomena, and it even supplies us with indirect means of studying ray vibrations to which the human retina does not respond. The electro-acoustic devices of Edison and his co-workers permit almost magical communication between human beings. Ruhmkorff's coil and the Geissler tube have rendered notable service in physical investigations; and the electric lamp promises to aid in exploring the internal parts of living animals as well as in studying the organic forms of the deep sea. But in the spectroscope we possess a power that enables us to peer into the very heart of Nature. In the extent of its grasp and the varied character of its applicability it surpasses the telescope, and at least rivals the microscope. It enables the astronomer to defy immeasurable distance, and to study the physical condition and the chemical composition of the sun and the stars as if they were within touch, and even to ascertain the direction of their movements.

Without attempting to discuss the import of the results thus gained—which would lead us too far—I may point out that they overthrow a dogma concerning the classification of the sciences. It has been said that the simpler and more general sciences lend both doctrines and methods to the more complex and less general sciences, and that the latter give nothing in return. But we now see chemistry endowing astronomy with an original and fruitful method of research.

Turning to the very opposite extremity of the scientific hierarchy, we find that to the biologist the spectroscope is of value in studying the relations of animal and vegetable fluids, and even of certain tissues. But this wonderful instrument is clearly destined to play its chief part in what is called terrestrial chemistry—the field where it has won the most signal triumphs.

It must be remarked, despite this vast range of applicability—a range sweeping through the whole universe and embracing all the four elements of antiquity—and despite the astonishing results already achieved and the prospect of greater revelations to come, that the spectroscope is still inadequately appreciated by professed men of science, and in consequence is to a great extent ignored by the "educated and intelligent public." In urging its more thorough recognition, I do not advocate the formation of Spectroscopic Societies for the fragmentary study of everything that can be observed with a spectroscope. But I recommend researching chemists to appeal to this instrument wherever requisite and possible.

An elaborate spectroscopic study of the basic constituents of rare minerals from different localities would be of great value, and I would suggest that on all possible occasions meteorites should be submitted to careful spectroscopic analysis.

I do not propose to discuss all the splendid achievements of the spectroscope in chemistry; nor its applications in ordinary analysis, qualitative and quantitative; nor the conduct of technical operations, such as the Bessemer process. I confine myself to the light thrown by the spectroscope upon the nature and the relations of our *elements*, real or supposed.

Though systematically employed by few experimentalists, the spectroscope has already led to the discovery of several hitherto unknown elements. In the early days of spectrum analysis, attention was mainly concentrated on the flame spectra: that is, the bodies in question were vaporized and rendered luminous by the action of a flame, such as that of the Bunsen burner or of the oxyhydrogen jet. This procedure in the hands of Bunsen and Kirchhoff gave us cæsium and rubidium; afterwards, in my own hands, thallium; and in those of Reich and Richter, indium.

Then followed the production and examination of spark spectra. The spark produced by means of the induction coil, especially when its energy is reinforced by the intercalation of a Leydén jar, volatilizes and renders luminous minute portions of matter, solid, liquid, or gaseous, which may then be examined by the spectroscope. In this

[1] Abstract of Address delivered on Thursday, March 28, at the annual general meeting of the Chemical Society, by the President, Mr. W. Crookes, F.R.S.

manner gallium was discovered, in 1875, by Lecoq de Boisbaudran. In consequence of the sharpness and the well-marked character of these spark spectra, they are relied on by chemists as certain proof of the identity of any two elements which yield identical spectra.

Next was introduced the systematic study of the absorption spectra seen when a beam of light is passed through certain transparent solids or through solutions of various substances. One of the earliest observers in this branch of spectroscopy was Dr. Gladstone, who, in 1858, read before this Society a paper on the absorption of light by various metallic salts, and gave the first description of the absorption spectrum of didymium. This branch of spectroscopy has proved not less fruitful in the recognition of new metallic elements.

In the investigation of the rare earths my principal object has been to separate the true from the undemonstrated and spurious, verifying the true, rejecting the spurious, and reducing as far as possible the number of the doubtful. In the following table I have given a list of the so-called "rare elements," with which for the last seven or eight years I have been specially occupied.

TABLE.

	Atomic Weight of Metal and Formula of Oxide.	Gives Spectrum by	Component Meta-Elements according to Crookes (1886).	Component Meta-Elements according to Nilson and Krüss (1887).
Didymium	Neodymium—140·3, Nd_2O_3 Praseodymium—143·6, Pr_2O_3 Unnamed.	Absorption.	Dα λ = 475	Diα Diβ Diγ Diδ Diε Diη Diθ Diι Diχ
Decipium				
Samarium	150·12, Sm_2O_3.	Absorption and Phosphorescence.	Sδ Gε Gγ Gθ	Smα Smβ
Lanthanum	138, La_2O_3.	Phosphorescence.		
Erbium	166, Er_2O_3.	Absorption and Phosphorescence.	λ 550 λ 493	Erα Erβ
Philippium	45—48, PpO.	Phosphorescence.		
Holmium		Absorption.		Xα Xβ Xγ Xδ
Thulium	170·7, Tm_2O_3.	Absorption.		Tmα Tmβ
Dysprosium		Absorption.	λ 457 (1888)—448	Xζ Xε Xη
Yttrium	88·9, Yt_2O_3.	Phosphorescence.	Gα Gβ Gδ Gζ Gη	Zα Tb Yt Lecoq de Boisbaudran.
Terbium	124·7, Tb_2O_3.			
Gadolinium (Yα)		Phosphorescence.	Gβ Gζ	
Ytterbium	173·01, Yb_2O_3.	Phosphorescence.		
Scandium	44·03, Sc_2O_3.			

Column 1 gives the names by which they are commonly known. Column 2 gives their atomic weights, &c. Column 3 shows in what manner they come under the domain of spectroscopy; and columns 4 and 5 notify the components or meta-elements into which some of these bodies have been decomposed in 1886 by myself, and in 1887 by Krüss and Nilson. In the first column I have exercised a judicial leniency in retaining candidates, for the sake possibly of old associations, when strict justice would have disestablished them. Thus, it may be doubted whether decipium, philippium, or gadolinium should have been retained. But since doubts have been cast on the integrity of nearly all the occupants of this column, the line should not be drawn too strictly.

At first spectroscopic examination was applied directly to substances, natural or artificial, which had not undergone any special preparation. The idea next occurred of attempting to split up substances supposed to be simple into heterogeneous constituents before appealing to the spectroscope. The refined chemical processes

used for this operation may be summarized under the name of fractionation, whether they be fractional precipitations, crystallizations, or decompositions. The essential principles of this process were so fully discussed on the last occasion when I had the honour of addressing you that I need not further allude to them.

The Didymium Group.

A combination of such delicate and prolonged chemical processes with spectroscopic examination applied to bodies showing absorption spectra soon led to important discoveries. When in that year the didymium from samarskite was examined by M. Delafontaine (*Comptes rendus*, vol. lxxxvii. p. 632; *Chemical News*, vol. xxxviii. p. 223), he found it to differ somewhat from ordinary didymium as extracted from cerite and gadolinite, and by a series of chemical fractionations he succeeded in separating from it an earth which he called decipium, giving at least three absorption bands, one having a wave-length of 416 ($1/\lambda^2$ 578); another narrower and stronger, at wave-length 478 ($1/\lambda^2$ 438), and a very faint "minimum of transmission" near the limit of the blue and green. Nine months later, M. Lecoq de Boisbaudran (*Comptes rendus*, vol. lxxxix. p. 212; *Chemical News*, vol. xl. p. 99) announced the discovery of samarium as a constituent of the didymium from samarskite.

Still didymium was not reduced to its ultimate simplicity. In 1885, Dr. Auer von Welsbach (*Monatsh. Chem.*, vi. 477), by fractionally crystallizing the mixed nitrates of ammonium, didymium, and lanthanum, showed it was thus possible to cleave didymium in a certain direction and separate it into two other bodies, one giving green salts and the other pink salts. Each of these has a characteristic absorption spectrum, the sum of the two sets of bands approximating to the old didymium spectrum. These bodies the discoverer has named respectively praseodymium and neodymium. The neodymium spectrum, according to Dr. Auer, consists of the whole of the bands in the red, with part of the large one in the yellow; it then misses all the green and blue, and takes in the second line in the violet. The spectrum of praseodymium takes the other part of the yellow band and all the green and blue, except the second blue, which belongs to neodymium. Subtracting these two spectra from the old didymium spectrum, there are still two bands left at λ 462 and 475 ($1/\lambda^2$ 465 and 443). Assuming that the argument from absorption spectra is a legitimate one—and all recent research tends to show that if not quite trustworthy it is at all events a weighty one—the inference I draw from these results is that the old didymium still contains a third body distinct from neo- and praseo-dymium, to which one or both of these extra bands is due.

I must venture to lay especial emphasis on the words *in a certain direction*. Didymium in my own laboratory has undergone other cleavages, and I have not yet decided whether we shall have to recognize further decompositions of neodymium and praseodymium, or whether the original didymium is capable of being resolved differently according to the manner in which it is treated. Keeping the band in the orange always of the same strength, in many of the fractions of didymium from different sources the other bands of neo- and praseodymium are seen to vary from very strong almost to obliteration (*Chemical News*, vol. liv. p. 27). In this way I have worked on the spectra of didymium from allanite, cerite, euxenite, fluocerite, gadolinite, hielmite, samarskite, yttrotitanite, &c., and the further I carry the examination the more the conclusion is forced upon me that didymium must not be regarded as compounded of two elements only, but rather as an aggregation of many closely allied bodies. Later researches of Messrs. Krüss and Nilson have led them to the same conclusion.

By examining the absorption spectra of solutions of rare earths obtained from widely different sources, MM. Krüss and Nilson (*Berichte der deutsch. chem. Gesellschaft*, vol. xx. Part 12, p. 2134; and *Chem. News*, vol. lvi. pp. 74, 85, 135, 145, 154, 165, 172) came to the conclusion that the elements giving absorption spectra, and known as didymium, samarium, holmium, thulium, erbium, and dysprosium, were not homogeneous, but that each one contained almost as many separate components as it produced bands of absorption.

They have discovered that in didymium obtained from some minerals one of the fainter lines of the normal didymium spectrum is strong, while others usually stronger are almost or quite absent; results to which I shall presently refer will show that this cannot be explained by dilution or concentration. In this way, by examining a great number of minerals, they found anomalies occurred in the case of almost each of the old didymium lines, and therefore decided, as above mentioned, that it is a compound body, capable of resolution into at least nine separate components.

Identical arguments are brought forward to prove that each of the other so-called elements, samarium, erbium, holmium, thulium, dysprosium, &c., are compounds of many closely allied bodies.

Messrs. Krüss and Nilson, I believe, are pushing their investigation with the object of isolating the separate components of these different earths. They, however, question the possibility of resolving the erbia and didymia earths into their several ultimate constituents by a fractionated decomposition of the nitrates. In fact, they assert that by means of the methods of separation at present known it would be almost impossible to completely isolate any single constituent of the mixed earths. They therefore propose, as I had previously done,[1] a method by which we may certainly arrive nearer to the mark and dispense with much tedious fractionation. If we examine the minerals which contain these rare earths, we find they occur in very different states of mixture or combination. Sometimes many of the constituents which we wish to separate are conjointly present, and sometimes but few. The desired differentiation, in fact, has already been commenced by Nature. Krüss and Nilson, therefore, whichever ingredient they wish to separate, propose to operate upon a mineral which contains that ingredient as far as possible in a state of isolation. In other words, they will take advantage of the work that Nature has already begun, and endeavour by refined chemical means to put the last finishing touches to her work. Thus they will be able to work with smaller quantities of primary material,—no small consideration in the case of some minerals,—and to obtain results in a shorter time. How widely the composition of one and the same mineral, as judged by our searching physical tests, may vary, will be seen from the following instances. Fergusonite from Arendal shows six of the bands of holmium, fergusonite from Ytterby four, and that from Hitterö only three. Moreover, the ingredient provisionally called X*a* is to be found in the fergusonite from Ytterby, but not in that of Arendal and Hitterö.

The foundation for thus firmly declaring what I had previously ventured to infer, is the striking differences in the spectra given by several specimens of one earth, say didymium, when obtained from different sources.

We are anxiously waiting the results of this investigation, but although the paper quoted was published in July 1887, no further communication has come from these illustrious workers.

Chemists recently have stated, as proof of the existence of new elements, the fact that certain bands of absorption as seen in various fractions "follow the same variations

[1] Address to the Chemical Section of the British Association, Birmingham Meeting, *Chem. News*, vol. liv. p. 123. "On the Fractionation of Yttria," *Chem. News*, vol. liv. p. 157. *Proc. Roy. Soc.*, vol. xl. (1886), p. 505.

of intensity." Before deciding the question whether didymium is a homogeneous whole, or whether an argument in favour of its heterogeneity can be based on the fact that the absorption spectra of didymium from different minerals differ *inter se*, it was necessary to ascertain if the absorption bands seen in its solutions, whatever the thickness of the layer, whether dilute or concentrated, followed the same variations, and also to ascertain the nature of these variations. To contribute to this inquiry I examined the absorption spectrum of a solution of neutral didymium nitrate containing 1 part by weight of metal in 10 of water, as seen through a series of cells from 1 mm. to 25 mm. in thickness. For this work I used a new form of binocular spectroscope, fitted with a mechanical tracing arrangement, so that each spectrum can be automatically mapped on paper strips. At the bottom, at 25 mm. thickness, all the known bands are visible, and they become fainter and die out in order, some of them remaining visible almost to the end. For instance, almost as long as the deep line in the blue part of the spectrum at $1/\lambda^2$ 507 can be distinguished, it is possible to see the group of three very narrow ones next to it. Two or three other less characteristic bands can be seen only when there is a very considerable depth of liquid; thus, the group in the red at about $1/\lambda^2$ 255 cannot be seen distinctly through less than 20 mm. of this strength of solution.

Having ascertained in this series how the spectra varied in appearance with different thicknesses of the same solution (strength 1 of Di in 10 of water), I repeated the experiments, keeping the thickness of layer of solution constant, and diluting the standard solution of didymium so that the rays of light passed through the same quantity of metal as in the former series. The results in each case were practically identical; the differences being too slight to be detected in my apparatus. The spectrum exhibited, for instance, by 1 mm. of the standard solution of didymium is found to be identical with the spectrum shown by the same solution diluted twenty times and viewed through a 20 mm. cell.

There are at least two points in these researches that I must touch, since they illustrate the necessity of great caution in drawing conclusions from an examination of absorption spectra. Messrs. Paul Kiesewetter and Krüss (*Berichte der deutsch. chem. Gesellschaft*, vol. xxi., 2310; *Chem. News*, vol. lviii. pp. 75, 91) have recently published a paper on this subject. They have examined gadolinite, and find that some of the constituents of didymium and samarium are absent, notably the group of lines in the green to which I have already referred. In my own laboratory I have worked for the last two years almost exclusively upon the earths from gadolinite—of which I obtained a large quantity from Fahlun—and there is not the shadow of a doubt that in my gadolinite earths the lines reported absent by Kiesewetter and Krüss are present in abundance.

Some hitherto unexplained condition doubtless rendered these lines invisible to Messrs. Kiesewetter and Krüss—perhaps the presence of some other earths, or some condition of concentration or acidity. In the light of this knowledge I do not see how we can take the results of Messrs. Krüss and Nilson or my own as final.

Owing to its complicated nature, Kiesewetter and Krüss consider gadolinite an unfavourable source of didymium for these investigations, and recommend that a large quantity of earth from keilhauite should be systematically worked up, for the reason that keilhauite didymium is more simple in constitution.

The Erbium Group.

It is known that a certain oxide, ten years ago called erbia, and regarded as belonging to a simple elementary body, has been resolved by the investigations of Delafontaine, Marignac, Soret, Nilson, Clève, Brauner, and others into at least six distinct earths—three of them, scandia, ytterbia, and terbia, giving no absorption spectra, whilst others, erbia (new), holmia, and thulia, give absorption spectra.

The first to announce that erbium was not a simple body was Delafontaine, who in 1878 (*Comptes rendus*, vol. lxxxvii. p. 556; *Chemical News*, vol. xxxviii. p. 202), published an account of philippium, a yellow oxide characterized by a strong band in the violet, λ 400 to 405, a broad black absorption band in the indigo-blue, about λ 450, two rather fine bands in the green, and one in the red.

The history of philippium is curious, and I may perhaps be allowed to give it in some detail. A year after Delafontaine's discovery, Soret (*Comptes rendus*, vol. lxxxix. p. 521; *Chemical News*, vol. xl. p. 147) published a paper in which he declared that philippia was identical with his earth X. The next month, in a note on erbia, Clève (*Comptes rendus*, vol. lxxxix. p. 708; *Chemical News*, vol. xl. p. 224) said he could not identify Soret's X with Delafontaine's philippia, as the latter was characterized by an absorption band in the blue which occupied the same place as one of the erbia bands. In February 1880 (*Comptes rendus*, vol. xc. p. 221; *Chemical News*, vol. xli. p. 72), Delafontaine returned to the subject, enumerating ten new earths in gadolinite and samarskite, viz., mosandra, philippia, ytterbia, decipia, scandia, holmia, thulia, samaria, and two others unnamed. He said that the properties of philippia were those of Soret's X and of Clève's holmia, and proposed that the name "holmia," being a duplicate name for an already known earth, should be discarded in favour of philippia. In July 1880 (*Comptes rendus*, vol. xci. p. 328; *Chemical News*, vol. xlii. p. 185), Clève repeated his former statement that philippia was not the same body as Soret's X or holmia. Delafontaine next withdrew all he had said about the absorption spectrum of philippium, and decided that it had no absorption spectrum (*Archives de Genève*, [3], 999, p. 15). Finally, Roscoe (Journ. Chem. Soc., vol. xii. p. 277), in an elaborate chemical examination of the earth-metals in samarskite, proved that philippia was a mixture of yttria and terbia. From a prolonged chemical study of these earths I have since come to a similar conclusion; but a spectroscopic examination of the earth left on igniting some specially purified crystals of "philippium formate" tested in the radiant-matter tube, has shown me that in the separation of Delafontaine's philippium the yttria undergoes a partial fractionation, and three of its components or meta-elements, $G\zeta$, $G\delta$, and $G\beta$, are present in great abundance, while others, $G\alpha$ and $G\eta$, are almost if not quite absent.

Shortly after the announcement of philippium, Soret (*Comptes rendus*, vol. lxxxvi. p. 1062) described an earth which he provisionally called X. This was soon found to be identical with an earth subsequently discovered by Clève (*Comptes rendus*, vol. lxxxix. p. 479; *Chemical News*, vol. xl. p. 125), and called by him holmia. Soret admitted the identity, and agreed to adopt Clève's name of holmia. The absorption spectrum of X consists of a very strong band in the extreme red, λ 804, two characteristic bands in the orange and green, λ 640 and 536, besides fainter lines in the more refrangible part of the spectrum.

Simultaneously with the discovery of holmia, Clève announced the existence of a second earth from erbia, which he called thulia. Its absorption spectrum consists of a very strong band in the red, λ 684, and one in the blue, λ 464·5.

In 1886 (*Comptes rendus*, cii. 1003, 1005), Lecoq de Boisbaudran showed by fractional precipitation of Soret's X, and by spectroscopic examination of the simple fractions, that this X, or holmium, consisted of at least two elements, one of which he named dysprosium, retaining the name of holmium for the residue left after deducting

dysprosium. The absorption spectrum of dysprosium contains the two bands λ 753 and λ 451·5, the residual holmium having a spectrum consisting of the remaining two bands, λ 640 and 536.

Phosphorescence Spectra.

I will now deal with phosphorescence spectra. Not a few chemists and physicists, conspicuous among whom is Ed. Becquerel, have carefully studied the phenomena of phosphorescence. Phosphorescence may be excited by elevation of temperature, by mechanical action, by electricity, and by exposure to the rays of the sun, and the light thus given off, for example in the case of fluor-spar, has been examined by means of the spectroscope. In my own spectroscopic research I have dealt with the phosphorescence occasioned by the impact of the molecules of radiant matter upon certain phosphorescent bodies, or what I have ventured to call molecular bombardment.

It is not necessary for me to describe the mode of procedure further than to say that the substance under examination is placed in a very high vacuum—a vacuum which varies in degree in the case of certain earths. In such a vacuum, when submitted to the action of the induction current, substances phosphoresce very differently from what they do when treated similarly at the ordinary pressure of the atmosphere. Under such circumstances the spectroscopic examination of matter affords what I have called the radiant matter test. The number of substances which are thus phosphorescent is very considerable. Glass of different kinds, according to its composition, phosphoresces with various colours. Phenakite (glucinium silicate) phosphoresces blue; spodumene (aluminium and lithium silicate) gives off a rich golden-yellow light; whilst the emerald phosphoresces crimson, and the diamond, being exceptionally sensitive and brilliant, throws off a bright greenish white light.

The ruby, one of the minerals I examined earliest in this manner, glows with a rich brilliant red tone, quite independent, as regards its depth and intensity, of the colour of the stone as seen by daylight; the pale, álmost colourless specimens, and the highly-prized variety of the true "pigeon's blood," all phosphoresce with substantially the same colour.

This method of observing the constitution of the rare earths, duly aided by delicate and prolonged chemical processes, has permitted us to push our investigations further than had previously seemed practicable. It enables us to determine whether we have reached the end of our investigations—a consummation which had hitherto been vainly sought. It has enabled us to prove that yttrium, samarium, &c., are not simple, homogeneous bodies. But what of the constituents into which they have been thus resolved? Suppose we refine them down until each displays merely one spectral band—what then? Is each one of such bodies, barely differentiated from its neighbours chemically or physically, entitled to rank as an element? If so, as I pointed out in the address which I had the honour to deliver before you in March last, we shall have to deal with further perplexing questions, arising in part from the relation of such elements to the periodic system. In a discussion of the elements, not as yet published, Dr. Wundt maintains that their possible number cannot exceed seventy-nine. But I myself see no definite and sufficient reason for limitation to this number. If these bodies are not elementary, possessing as they do the properties commonly regarded as characteristic of an element, we must be prepared to show why not?

Whatever rank may ultimately be assigned to these substances, they must, for convenience sake, have names as soon as our knowledge of their properties is in a sufficiently advanced state to allow of their removal from the suspense account.

The Yttrium Group.

Yttrium—the old yttrium—proves now to be not a simple element, but a highly complex substance. I have come to the conclusion that it may be split up certainly into five and probably into six constituents. If we take these constituents in the order of their approximate basicity—the chemical analogue of refrangibility—the lowest of these constituents gives a deep blue band, $G\alpha$; then follows a strong citron band, $G\delta$, which increases in sharpness until it may be called a line; then a red band, $G\zeta$: then a deeper red band, $G\eta$; and lastly a close pair of greenish blue lines, $G\beta$. Following these are frequently seen $G\epsilon$, $G\gamma$, and $G\theta$, the yellow, green, and red components of samarium.

A possible explanation of the existence and nature of the new bodies into which "old yttrium" has been split up, and of parallel cases which will doubtless be found on closer examination, is this. Our notions of a chemical element must be enlarged; hitherto the elemental molecule has been regarded as an aggregate of two or more atoms, and no account has been taken of the manner in which these atoms have been agglomerated. The structure of a chemical element is certainly more complicated than has hitherto been supposed. We may reasonably suspect that between the molecules we are accustomed to deal with in chemical reactions, and the conponent or ultimate atoms, there intervene sub-molecules, sub-aggregates of atoms, or meta-elements, differing from each other according to the position they occupy in the very complex structure known as "old yttrium."

The arguments in favour of the different theories are as yet not unequally balanced. But the assumption of compound molecules will perhaps account for the facts, and thus legitimate itself as a good working hypothesis, whilst it does not seem so bold an alternative as the assumption of eight or nine new elements.

I have just mentioned that the earth heretofore called yttria, and supposed to be simple, has been split up into a number of simpler bodies. Now these constituents of the old yttria are not *impurities* in yttria, any more than praseodymium and neodymium are impurities in didymium. They proceed from a real splitting up of the yttrium molecule into its components, and when this process is completed the "old yttria" has disappeared. If these newly-discovered components on further examination should be found worthy to take the rank of elements, I think, as first discoverer, I am entitled, by the custom prevailing among men of science, to name them. For the present, and until their investigation is more advanced, I designate them by provisional symbols. One of the most distinct characteristics of "old yttria" is its very definite spark spectrum. To which of its components this spark spectrum belongs I am not yet able to say. It is possible the particular component to which the spark spectrum is due yields no phosphorescent spectrum. It is also possible that the spark spectrum, like "old yttria," may prove to be compound, and then the well-known lines it contains will have to be shared between two or more of the newly-discovered bodies.

I wish emphatically to re-state that at present no single component of old yttria can lawfully lay claim to what may be called the paternal name; and it seems to me that in the present state of the question no one is entitled to call one of the new bodies "yttria," and to characterize the remainder as impurities.

Interference of Phosphorescence Spectra.

A recent discovery of some beautiful spectra given by the rare earths when their pure oxides are highly calcined, shows the remarkable changes produced in the spectra of these earths when two or more are observed in combination. It has likewise opened to me a wide field of in-

vestigation in the nature of the elements themselves. Alumina is especially active in inducing new spectra when mixed with rare earths. I have given more than a twelvemonth to the exclusive study of alumina phosphorescence, and still the research is incomplete. But I have obtained some remarkable results. A moderate amount of fractionation has enabled me to penetrate below the surface of the red glow common to crude alumina, and to see traces of a most complicated sharp line spectrum. By pushing one particular process of fractionation to a considerable extent I have obtained evidence of a body which is the cause of some of these lines. The spectrum, described by me in 1887 (*Chem. News*, vol. lvi. pp. 62, 72), is one of great beauty. The new body is probably one of the rare elements or meta-elements closely connected with decipia, for I have reproduced the spectrum very fairly by adding decipia to alumina. Before arriving at definite conclusions much time must be devoted to the subject. Certain it is that this new earth is not yttria, erbia, samaria, didymia, lanthana, holmia, thulia, gadolinia, or ytterbia, the spectrum of each of these when mixed with alumina being very beautiful, but differing entirely from the decipia-alumina spectrum.

M. de Boisbaudran's Reversion Spectra.

Another modification of the phosphorescence process is afforded by the "reversion spectra" of M. Lecoq de Boisbaudran.

The following is the description of this process by M. Lecoq de Boisbaudran read before the Academy of Sciences on June 8, 1885:—"When the electric spectrum of a solution *with a metallic base* is produced, it is customary to make the outside platinum wire (whence the induction spark strikes) positive, the liquid consequently forming the negative pole. If the direction of the current is reversed, the metallic rays (due to the free metal or to one of its compounds) are scarcely or not at all visible; at all events, so long as the exterior platinum wire now forming the negative pole is not coated with a deposit."

M. de Boisbaudran continues:—"Having again taken up last year my researches on the rare earths belonging to the didymium and yttrium family, I had occasion to observe with many of my preparations the formation of spectrum bands, nebulous, but sometimes tolerably brilliant, having their origin in a thin layer of a beautiful green colour, which appeared at the surface of the liquid (a solution of a chloride) when it was rendered positive."

M. de Boisbaudran further adds:—"The production of my reversion spectrum appears to be analogous physically with the formation of the phosphorescence spectra obtained by Mr. Crookes at the negative pole in his high vacuum tubes containing certain compounds of yttria. The conditions of the two experiments are, however, practically speaking, very different."

By this method M. de Boisbaudran has discovered phosphorescent spectra, which he considers due to the presence of two earths, one of which, supposed to be new, he has provisionally named $Z\alpha$, and another, also thought at first to be new, and therefore called $Z\beta$, but since admitted by him to be terbia (*Comptes rendus*, vol. cviii. p. 167, January 28, 1889). In the hands of so skilful an experimentalist as my accomplished friend, this method may give trustworthy indications, but the test is really beyond the range of practical analysis, owing to the difficulty of eliciting the phenomena. Unless the strength of the spark, the concentration and acidity of the solution, and the dispersive and magnifying power of the spectroscope bear a certain proportion to each other, the observer is likely to fail in seeing a spectrum even in solutions of earths which contain considerable quantities of $Z\alpha$ and terbia.

The Phosphorescence of Alumina.

I now wish to draw attention to some recent researches on the phosphorescence spectrum given by alumina. So far back as 1859, Becquerel examined in his phosphoroscope pure alumina carefully prepared, and described it as glowing with a splendid red colour. He rendered his specimens phosphorescent by exposure to the sun, and made no use of the induction spark. As described by Becquerel (*Annales de Chemie et de Physique*, vol. lvii., 1859, p. 50), the spectrum of the red light emitted from alumina agrees with that of the ruby when submitted to the radiant matter test. It displays one intensely red line a little below the fixed line B in the spectrum, having a wave-length of about 689·5. There is a continuous spectrum beginning at about B and a few fainter lines beyond it, but in comparison with this red line the faint ones are so dim that they may be neglected. My latest observations in the vacuum tube prove this line to be double, the distance apart of the components being about half the distance separating the D lines (Roy. Soc. Proc., vol. xlii. p. 26, December 30, 1886), their respective wave-lengths being 694·2 and 693·7 ($1/\lambda^2$ 207·5 and 207·8).

The red phosphorescence of this alumina is exceedingly characteristic. M. de Boisbaudran (*Comptes rendus*, vol. ciii. p. 1107; vol. civ. pp. 330, 478, 554, 824) contends, however, that this red phosphorescence is due, not to the alumina itself, but to an accompanying trace of chromium, 1/1100 part of chromium being sufficient to give a splendid red phosphorescence, whilst even 1 part of chromic oxide in 10,000 will produce a very distinct rose colour. In testing this view I have purified alumina most carefully, so as to secure the absence of chromium, and on examining it in the radiant-matter tube I have still obtained the characteristic phosphorescence and spectrum. I have then added to my purified alumina chromium in known varying proportions, but without finding any increase in the intensity of the phosphorescence. I fractionated my purified alumina by different methods, and found that the substance which forms the crimson line becomes concentrated towards one end of the fractionations, whilst chromium concentrates at the other end. I have suggested four possible explanations of the phenomena—

(1) The crimson line belongs to alumina, but it is liable to be masked or extinguished by some other earth, which accumulates towards one end of the fractionations.

(2) The crimson line is not due to alumina, but to the presence of an accompanying earth which accumulates towards the other end of the fractionations.

(3) The crimson line belongs to alumina, but its development requires certain precautions to be taken in the duration and intensity of the ignition, and absolute freedom from alkaline and other bodies carried down by precipitated alumina, and difficult of removal by washing.

(4) The earth alumina is a compound molecule, one only of its component sub-molecules giving the crimson one. If this hypothesis is correct, alumina must admit of being split up in a manner analogous to yttria.

Conclusions.

During the course of the investigations—whose results are briefly summarized in the foregoing pages,—I have repeatedly had recourse to the balance, to ascertain how the atomic weights of the earths under treatment were varying. An atomic weight determination is valuable in telling when a stable molecular grouping is arrived at. During a fractionation, the atomic weight of the earth slowly rises or falls until it becomes stationary, after which no further fractionation of that lot *by the same process* makes it vary. Usually a result of this kind has been relied on as proof that the elementary stage has been

reached. This constancy of atomic weight, however, only proves that the original body has been split up by the fractionating process into two molecular groupings capable of resisting further decomposition by that identical process; but these groupings are not unlikely to break up when a different fractionating process is brought to bear on them, as I found in the separation of didymium and samarium when using dilute ammonia as the fractionating precipitant. In my paper on "Radiant Matter Spectroscopy" I said ("Part II., Samarium," Phil. Trans. Roy. Soc., Part 2, p. 129, June 18, 1885):—"After a time a balance seems to be established between the affinities at work, when the earth would appear in the same proportion in the precipitate and the solution. At this stage they were thrown down by ammonia, and the precipitated earths set aside to be worked up by the fusion of their anhydrous nitrates so as to alter the ratio between them, when fractionation by ammonia could be again employed."

It is obvious that when the balance of affinities here spoken of was reached, the atomic weight of the mixture under treatment would have become constant, and no further fractionation would have caused the atomic weight to alter.

Atomic weight determinations are valuable in telling when the fractionating operation in use has effected all the separation it can: at this point it becomes constant. The true inference is, not that a new earth has been obtained, but simply that the fractionating operation requires changing for another, which will cleave the group of meta-elements in a different direction.

Meantime, I have kept strictly in view the question, What is an element, and how shall it be recognized when met?

On this subject I beg to submit the following considerations, which, primarily referring to didymium, may at any moment apply to other cases:—

Neodymium and praseodymium are simply the products into which didymium is split up by one particular method of attack.

It must be remembered that a single operation, be it crystallization, precipitation, fusion, partial solution, &c., can only separate a mixture of several bodies into two parts, just as the addition of a reagent only divides a mixture into two portions, a precipitate and a solution, and these divisions will be effected on different lines according to the reagent employed. We add, *e.g.*, ammonia to a mixture, and at once get a separation into two parts. Or we add, say, oxalic acid to the same original solution, and we then split up the mixture into two other parts differently arranged.

Thus by crystallizing didymium nitrate (in Auer's way) we divide the components into two parts. By fusing didymium nitrate we divide its components in a different way; but so long as different methods of attack split up a body differently, it is evident that we have not yet got down to "bed rock."

Further, a compound molecule may easily act as an element. Take the case of didymium, which is certainly a compound, whether the products of Auer's operation be final or not. Didymium has a definite atomic weight; it has well-defined salts, and has been subjected to the closest scrutiny by some of the ablest chemists in the world. I refer particularly to Clève's classical memoir. Still the compound molecule known as didymium was too firmly held together to act otherwise than as an element, and as a seeming element it emerged from every trial. The simple operations to which it had been submitted in the preparation of its salts, and in its purification from other compound molecules, such as samarium and lanthanum, were not sufficient to split it up further. But subjected to a new method of attack it decomposes at once.

We have, in fact, a certain number of reagents, operations, processes, &c., in use. If a body resists all these, and behaves otherwise as a simple substance, we are apt to take it at its own valuation and to call it an element. But for all that it may, as we see, be compound, and as soon as a new and appropriate method of attack is devised, we find it can be split up with comparative ease. Still, we must never forget that, however complex, it can hardly be resolved into more than two parts at one operation.

From considerations above laid down I do not feel in a position to recognize neodymium and praseodymium as elements. We need some criterion for an element which shall appeal to our reason more clearly than the old untrustworthy characteristic of having not as yet been decomposed; and to this point I must beg to call the special attention of my colleagues. It may be that whatever body gives only one absorption band is an element, but we cannot conversely say that an element may be known by its giving only one absorption band, since most of our elements give no bands at all!

Until these important and difficult questions can be decided, I have preferred to open what may be figuratively called a suspense account, wherein, as I have previously suggested, we may provisionally enter all these doubtful bodies as "meta-elements."

But these meta-elements may have more than a mere provisional value. Besides compounds, we have hitherto recognized merely ultimate atoms or the aggregations of such atoms into simple molecules. But it becomes more and more probable that between the atom and the compound we have a gradation of molecules of different ranks, which, as we have seen, may pass for simple elementary bodies. It might be the easier plan, so soon as a constituent of these earths can be found to be chemically and spectroscopically distinguishable from its next of kin, to give it a name and to claim for it elemental rank; but it seems to me the duty of a man of science to treat every subject, not in the manner which may earn for him the greatest temporary κῦδος, but in that which will be of most service to science.

If the study of the rare earths leads us to clearer views on the nature of the elements, neither my colleagues nor myself will, I am sure, regret the months spend in tedious and apparently wearisome fractionations. No one can be more conscious than myself how much ground is yet uncovered, and how many radical questions have received but very inadequate answers. But we can only work on, "unresting, unhasting," trusting that in the end our work will throw some white light upon this deeply interesting department of chemical physics.

NOTES.

A MOST important and profoundly interesting letter from Mr. Stanley to the Chairman of the Emin Pasha Relief Committee has been published this week. It is dated Bungangeta Island, Ituri River, or Aruwimi River, August 28, 1888, and records the adventures of the Expedition from June 28, 1887, until the time when the letter was written. There is no more stirring tale even in the long and romantic history of African exploration. On April 29, 1888, Mr. Stanley met Emin Pasha on the shores of the Albert Nyanza Lake, and it would be impossible to over-rate the courage, energy, and resource manifested by the great traveller in grappling with the terrible difficulties which had stood in his way. Having spent some time with Emin, Mr. Stanley returned to the Aruwimi River, and reorganized what remained of Major Barttelot's force. When the letter was despatched he was on the point of starting again for the Albert Nyanza, and we may have to wait some time for further intelligence. Next week we hope to give an account of the geographical results of the Expedition, so far as they are now known.

THE Executive Committee of the International Exhibition of Geographical, Commercial, and Industrial Botany, to be held at Antwerp in 1890, has decided to celebrate on this occasion the three hundredth anniversary of the invention of the microscope. It proposes to organize what it calls a retrospective exhibition of the microscope, and an exhibition of instruments produced by living makers. Conferences relating to all important questions connected with the microscope will also be held. The Exhibition ought to be remarkably interesting, and will no doubt be a great success.

WE are glad to hear that the Congress of the United States has recently provided for the establishment of a Zoological Park in the City of Washington, and has appropriated money for the purchase of a tract of land of not less than one hundred acres in extent, immediately adjacent to that city. The proposed site for the park is the valley of Rock Creek, a small river emptying into the Potomac at Washington. This is said to be one of the most picturesque sites ever devoted to such a purpose, having several rocky cliffs of considerable extent, groves of pine, oak, beech, and other trees, and several little streams running down the steep sides of the valley into the river. Part of the land is under cultivation, but much of it is virgin forest, so that its natural advantages have been preserved by singular good fortune in spite of the neighbourhood of the growing city. Amongst the animals to be kept here will, no doubt, be a herd of the buffalo (*Bison americanus*), now nearly extinct in the Western prairies.

THE half-yearly general meeting of the Scottish Meteorological Society was held in the hall of the Royal Scottish Society of Arts, Edinburgh, on Monday, April 1. Next week we shall give some account of the proceedings. We are glad to learn from the Report of the Council that the application of the Society for a grant from the surplus fund of the Association of the Edinburgh International Exhibition of 1886 has been acceded to, the Association having granted the handsome donation of £1000 towards the completion of the Ben Nevis Observatory by the establishment of the Low Level Observatory at Fort William.

A FRIGHTFUL hurricane, which raged for nearly two days, broke over the Samoan Islands on the night of March 16. Of seven foreign war-vessels caught by the hurricane at Apia, only H.M.S. *Calliope* contrived to make the open sea. The German and American squadrons were destroyed, and many lives lost.

THE meeting of the French Meteorological Society on the 5th of March was chiefly occupied by an analysis of the report of the Krakatão Committee, by M. L. Teisserenc de Bort. The Abbé Maze presented an apparatus for rotating a thermometer fixed in a framework, and intended to take the place of the sling-thermometer in cases in which the latter was inconvenient. The Society has received a circular from the Minister of Public Instruction, asking for a list of old manuscript observations, with a view to the preparation of a catalogue for publication.

VAPOUR-DENSITY determinations of bismuth, arsenic, and thallium have been successfully carried out at extraordinarily high temperatures in the laboratory of the University of Göttingen by Dr. Biltz and Prof. Victor Meyer. The highest temperature hitherto attained in former experiments by Dr. Mensching and Prof. Meyer lay somewhere between 1400° and 1500° C. Now, thanks to a suggestion of Prof. Nilson, of Stockholm, means have been found of raising the temperature of the Perrot gas furnace, in which the well-known Victor Meyer porcelain density apparatus was heated, to a white heat of 1650°–1750°. Hence there are now from 200 to 300 more degrees of temperature at which density determinations are possible, and it may naturally be expected that substances which were only partially vaporized at 1450° may yield definite results at 1750°. The temperatures were determined by means of a glazed porcelain air-thermometer, which was decisively proved to be impermeable to the furnace gases. In order to further strengthen that portion of the porcelain apparatus placed in the furnace, it was surrounded by an outer casing of platinum. The volatilizations were effected in atmospheres of nitrogen, all traces of oxygen being rigorously excluded. Bismuth was found in the former experiment to be only partially vaporized at 1450°. At the higher temperatures now available it has been found to be rapidly and completely volatilized, and density determinations have been readily carried out. The values obtained in two experiments at 1600°–1700° are 11·98 and 10·12 (air = 1). If the bismuth molecule in the gaseous state consists of the normal two atoms, Bi_2, its vapour-density should be 14·4; if it contains only one atom, Bi_1, the density becomes 7·2. The values obtained, which are considerably less than that required for the normal molecular condition, show that this condition is impossible, and bismuth therefore resembles mercury, cadmium, and zinc, in containing only one atom to the molecule. In the case of arsenic the results agree very well with the assumption of a two-atom molecule, As_2. At 1714° the density found was 5·45; and at 1736°, 5·37. As_2 requires 5·20. Hence the four-atom molecule of arsenic at lower temperatures becomes dissociated about 1750° into the normal molecule consisting of two atoms. Thallium appears to be at once normal. At 1636° the value obtained was 16·11; and at 1728°, 14·25. The ordinary molecule Tl_2 corresponds to 14·17. The metal, however, is still difficultly vaporizable even at this tremendous temperature. Another very interesting result was obtained in case of cuprous chloride, which even at 1700° gave densities almost exactly corresponding to the formula Cu_2Cl_2. Sulphur, iodine, and mercury also gave results confirming the stability of molecules consisting of two atoms of sulphur and single-atom molecules of iodine and mercury.

MR. C. G. HALL, of Dover, sends to the new number of the *Entomologist's Monthly Magazine* the following note, written by the late moth-collector, Mr. H. J. Harding:—"On a beautiful evening at the end of June 1852, in the locality of Darenth Wood, I had just pinned my first insects taken at sugar, when I heard a strange sound behind me, and, on looking round, observed what I thought was a beetle flying round a sallow bush; when in my net, it again repeated the sound, but what was my surprise upon finding it a Lepidopterous insect. I had now got it between my thumb and finger to give it an entomological pinch, when it again produced the sound; the deadly pin was now presented, and, with the aid of my lantern, I found it was a common *Halias prasinana*. But it was a fact new to me: I I had never, during thirty years entomologizing, heard of such a thing before. The sound was as if you passed a pin sharply along three or four teeth of a comb. I suppose it was a love song to charm his lady."

RECENTLY there have been some valuable "finds" of antiquities belonging to the Iron Age in Norway. At Nötterö, on the Christiania Fjord, there were found in a mound some bones, an iron pot with handles, a sword 2 feet 6 inches long, the handle having knobs of a yellow metal, an anvil, and a pair of smith's tongs. The mound was no doubt at one time situated close to the sea; it is now some 300 yards inland. At Laurvig a large number of similar articles were discovered in two mounds.

IN a Report, just received, Mr. R. L. Jack, Government Geologist, Queensland, gives a valuable account of the geology of the Russell River, which he lately visited with Mr. Christie Palmerston. They were accompanied by eight "aboriginal boys," and Mr. Jack incidentally presents a vivid description

of these attendants, who (with one exception) had been, eighteen months before, "absolute savages." "I observed with some interest," says Mr. Jack, "Mr. Palmerston's method of 'working' his boys. Its essential elements seemed to be giving them time, feeding them well, and keeping them in good humour by allowing for their propensities to hunt or play, and by making fun with them. It is fortunately unnecessary to be a Joe Miller to keep the boys amused, as anything which would amuse children will serve the purpose as well as the most delicate jokes. Mr. Palmerston's method is evidently successful, as I never saw more contented, willing, useful, and well-bred young men of any nationality."

DR. FRANZ BOAS contributes to the Proceedings of the U.S. National Museum an interesting paper on the houses of the Kwakiutl Indians, British Columbia. In these houses, the uprights are always carved according to the crest of the gens of the house-owner. The Indians of the present time make various combinations of the emblems of the gentes of both parents of the house-owner, and this is the reason for the great variety of forms. Besides this, legends referring to certain ancestors are illustrated in the emblems, and thus it happens that seemingly the ancient styles are not strictly adhered to.

AT a recent meeting of the Swedish Anthropological Society, Prof. G. Storm read a paper on his researches relating to the Lapps. The speaker held that this race had settled in Northern Scandinavia as far back as the Stone Age, and had not begun to move southwards until the Middle Ages. These southward movements had occurred periodically. At the end of the fifteenth century the Lapps had reached the sixty-fourth degree of latitude, but were now found much further south. The subject was of interest, because of the general belief that the Scandinavians had driven the Lapps northwards. In common with others, Prof. Storm was of opinion that the Lapps belonged to the Finnish-Ugrian race.

THE Dundee and District Association for the Promotion of Technical and Commercial Education have issued an elaborate Report on education in Dundee and the neighbourhood. They bring together a mass of facts which afford "abundant and gratifying evidence of a growing appreciation of science studies." The Committee, however, point out that the increase of attendance at science classes is chiefly in the elementary stages of the different subjects. More advanced classes are small, or do not exist at all. Classes in the higher branches of mathematics, physics, chemistry, and engineering have been most abundantly provided for at the Dundee University College, and the Committee hope that a very large increase in the numbers attending these may in the immediate future be the result of the elementary teaching which is now so widely appreciated.

THE Royal University of Ireland has issued its Calendar for the year 1889. The papers set at the examinations in 1888 have already been published in a separate volume, and form a supplement to the Calendar.

PROF. ANGELO HEILPRIN has contributed to the Proceedings of the Academy of Natural Sciences of Philadelphia some valuable notes on the zoology of the Bermuda Islands. These notes are based on personal observations, and on collections made during a recent brief sojourn on the islands in company with a class of students from the Academy of Natural Sciences. Prof. Heilprin devoted much time to the study of the geological features of the Bermudas. The results of his work in this department he will embody in a future paper.

AT the annual meeting of the Asiatic Society of Bengal, on February 6, an address was delivered by the President, Colonel J. Waterhouse. This address has now been printed. It contains an interesting review of the progress of science and Oriental literature in India and its nearer border-lands during the year 1888.

WE have received Parts IV. and V. of "A Catalogue of the Moths of India," compiled by Mr. E. C. Cotes and Colonel C. Swinhoe. The subjects dealt with are Geometrites and Pyrales.

TWO works on palæontology are now being issued, in parts, in Germany: "Handbuch der Palæontologie," by Dr. A. Schenk and Prof. K. A. Zittel; and "Elemente der Palæontologie," by Dr. G. Steinmann and Dr. L. Döderlein. The former work is published by R. Oldenbourg, Munich and Leipzig; the latter by W. Engelmann, Leipzig.

ONE of the Johns Hopkins University Circulars, for March, contains the following morphological notes from the biological laboratory of the University: a preliminary abstract of researches by W. K. Brooks and F. H. Herrick on the life history of Stenopus, by W. K. Brooks; list of Actiniaria found at New Providence, Bahama Islands, by J. Playfair McMurrich; on the occurrence of an Edwardsia stage in the free-swimming embryos of a Hexactinian, by J. Playfair McMurrich; notes on the fate of the amphibian blastopore, by T. H. Morgan; on the anatomy and histology of *Cymbuliopsis calcœla*, by J. I. Peck; on a new phenomenon of cleavage in the ovum of the Cephalopod, by S. Watase; on the structure and development of the eyes of the Limulus, by S. Watase; notes on the embryology of *Mülleria agassizii*, Tel., a Holothurian common at Green Turtle Cay, Bahamas, by Charles L. Edwards; on the occasional presence of a mouth and anus in the Actinozoa, by Henry V. Wilson; on the breeding-seasons of marine animals in the Bahamas, by Henry V. Wilson; the multiplication of Bryophyllum, by B. W. Barton; notice of Dr. H. V. Wilson's paper on the development of *Manicina areolata*, by T. H. Morgan; report of Dr. Henry V. Wilson as Bruce Fellow of the Johns Hopkins University.

THE Royal Society of Victoria prints an alphabetical list of the genera and species of Sponges described by Mr. H. J. Carter, F.R.S., together with a number of his more important references to those of other authors, with an introductory notice, by Mr. Arthur Dendy, Demonstrator and Assistant Lecturer in Biology in the University of Melbourne.

ON Tuesday evening Prof. Raphael Meldola delivered an interesting lecture at the Royal Victoria Hall on "Insects in Disguise, and on Mimicry," to an audience of about 500 persons, chiefly working men. Many illustrations, lent by Mr. Poulton, were thrown upon the screen, and were much appreciated. The following lectures will also be delivered at the Royal Victoria Hall:—Tuesday, April 9, "Polarized Light," by Prof. Silvanus Thompson; Tuesday, April 16, "Electric Tram-cars," by Dr. Heming.

THE additions to the Zoological Society's Gardens during the past week include a Spanish Terrapin (*Clemmys leprosa*), South European, presented by Mr. F. T. Mason; two Tuatera Lizards (*Sphenodon punctatus*) from New Zealand, a Long-billed Butcher Bird (*Barita destructor*) from New Holland, deposited; a Common Otter (*Lutra vulgari* ♂), British, two Black-necked Storks (*Xenorhynchus australis* ♂ ♀), from Malacca, a Teguexin Lizard (*Teius teguexin*) from South America, purchased; an Alleghany Snake (*Coluber alleghaniensis*) from North America, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE ASTRONOMICAL SOCIETY OF THE PACIFIC.—The increased interest in astronomy which has been felt in California in consequence of the erection of the Lick Observatory, and perhaps even more widely from the recent solar eclipse, which was so well and widely observed in the State, has led to the formation of an Astronomical Society under the above title. The Society was organized at a meeting held on February 7, 1889, and Prof. Holden was appointed interim President,

Messrs. Schæberle and Burckhalter Secretaries, and Mr. Molira Treasurer, until March 30, when a general meeting was to be held for the election of officers and Council, and other necessary business. The seat of the Society is to be in San Francisco, but only half the meetings are to be held there, the other half being proposed to be held at the Lick Observatory during the fine weather of the summer months. A circular setting forth the objects and regulations of the new Society has been widely circulated amongst those most likely to be desirous of joining it.

THE LATE W. E. TEMPEL.—We greatly regret to have to record the death, on March 16, of the Arcetri observer, William Ernest Tempel, so well known as one of the most keen-sighted and careful observers of comets and nebulæ. Herr Tempel, though his astronomical reputation was entirely associated with Marseilles or Italy, was of German extraction, having been born on December 4, 1821, at Nieder-Cunersdorf. His parents were poor, and when he grew up he followed the profession of lithographic artist. He settled down at Venice in 1859, after several wanderings, and here first began his astronomical observations with a 4-inch Steinheil, which he had purchased for himself. Here he discovered his first comet, and the famous Merope nebula. The following year he went to Marseilles, where he acted for some time as assistant at the Observatory, then under the direction of M. Valz. Here he discovered six minor planets, and ten comets, two of which proved to be of short period, and a third was rendered not less important from its being the comet connected with the Leonid meteors. The outbreak of the Franco-German war obliged him to leave France, and he returned to Italy. Giving up his lithography, and devoting himself wholly to astronomy, he acted for four years as Assistant at the Brera Observatory, Milan, and in 1875 he became Astronomer, and practically Director, of the new Observatory at Arcetri, Florence. Five more comets were discovered by him either at Brera or Arcetri, and at this latter place he took up the study of nebulæ, of which he made a great number of exceedingly fine drawings. In latter years his health obliged him to give up the work of observing, but he had already won for himself a distinct and honourable position in the history of the science. He was elected Foreign Associate of the Royal Astronomical Society in 1881, and received the Astronomical Prize of the Lyncean Academy in 1879, besides several from the Imperial Academy of Sciences of Vienna.

THE COMPANION OF SIRIUS.—Mr. Burnham, observing Sirius with the 36-inch telescope of the Lick Observatory during the past winter, finds the place of the companion as under:—

Date of Observation.	Distance.	Position Angle.
1888·97	5″·27	13°·9.

He was not able to see any other near companion.

COMET 1888 *e* (BARNARD, 1888 SEPTEMBER 2).—Herr A. Berberich gives in the *Astr. Nachr.*, No. 2883, the results of a more detailed computation of the orbit of this comet. He has formed nine normal places from observations extending from 1888 September 5 to 1889 February 17, and made at different Observatories, and deduces the following elements :—

$$T = 1889 \text{ January } 31{\cdot}256389.$$

$$\left.\begin{aligned} \omega &= 340^\circ\ 29'\ 22''{\cdot}71 \\ \Omega &= 357\ 25\ 35{\cdot}00 \\ \iota &= 166\ 22\ 12{\cdot}20 \end{aligned}\right\} \text{Mean Eq. } 1889{\cdot}0.$$

$$\log q = 0{\cdot}2587773.$$

The ephemeris shows that the theoretical brightness of the comet will undergo very little change for several months to come, and it will be almost of the same brightness as it was on the night of discovery by the time the anniversary of that date is reached.

Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
April 6	23 33 55	0 6·7 S.	0·2997	0·4582	1·7
10	23 33 4	0 10·3 N.	0·3043	0·4548	1·7
14	23 32 4	0 27·0 N.	0·3090	0·4508	1·7
18	23 30 53	0 43·2 N.	0·3139	0·4459	1·7
22	23 29 28	0 58·8 N.	0·3189	0·4402	1·7
26	23 27 48	1 13·8 N.	0·3240	0·4337	1·8
30	23 25 51	1 28·1 N.	0·3292	0·4264	1·8

The brightness at discovery is taken as unity.

COMET 1888 *f* (BARNARD, 1888 OCTOBER 30).—The following ephemeris for Berlin midnight for this comet is by Dr. R. Spitaler, *Astr. Nachr.*, No. 2875 :—

1889.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Brightness.
April 6	9 21 22	37 14·3 N.	0·4830	0·4015	0·15
10	9 22 11	37 25·5 N.	0·4886	0·4171	0·14
14	9 23 22	37 33·4 N.	0·4941	0·4323	0·12
18	9 24 52	37 38·5 N.	0·4995	0·4472	0·11
22	9 26 42	37 41·0 N.	0·5049	0·4616	0·10

The brightness at discovery is taken as unity.

SATURN'S RING.—Mr. Keeler, writing to *Gould's Astronomical Journal* under date March 4, states that the very fine division on the outer ring of Saturn, detected with the 36-inch refractor early in 1888, has been recently seen again under specially favourable circumstances. The new division was about one-sixth of the breadth of ring A from its outer edge, and appeared as a distinct dark line of exceeding fineness. A dark shading extended inwards from the new division almost to the inner edge of the ring. The brightest part of the ring was the narrow strip lying outside the new division, and between it and the outer edge. The Lick observers consider this marking a permanent feature of the planet, but one which can only be observed with exceptional instrumental and atmospheric advantages. The shadow of the planet on the ring was carefully watched, but no deformation was detected.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 APRIL 7–13.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 7

Sun rises, 5h. 23m. ; souths, 12h. 2m. 4·7s. ; sets, 18h. 41m. : right asc. on meridian, 1h. 5·9m. ; decl. 7° 1′ N. Sidereal Time at Sunset, 7h. 46m.

Moon (at First Quarter on April 8, 14h.) rises, 9h. 17m. ; souths, 17h. 34m. ; sets, 1h. 53m.*: right asc. on meridian, 6h. 38·5m. ; decl. 22° 30′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. on meridian. h. m.	Declination on meridian. ° ′
Mercury..	5 8	11 2	16 56	0 5·5	1 57 S.
Venus ...	5 35	13 54	22 13	2 58·6	23 43 N.
Mars ...	5 58	13 14	20 30	2 18·0	13 49 N.
Jupiter ...	1 35	5 31	9 27	18 33·8	22 56 S.
Saturn ...	12 20	20 0	3 40*	9 5·1	17 55 N.
Uranus ...	18 46*	0 13	5 40	13 15·1	7 15 S.
Neptune..	7 5	14 50	22 35	3 54·5	18 40 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

April.	h.	
9	13	Venus stationary.
10	13	Saturn in conjunction with and 1° 9′ south of the Moon.

Saturn, April 7.—Outer major axis of outer ring = 43″·1 : outer minor axis of outer ring = 12″·3 : southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Apr. 11,	3 34	*m*
R Lyncis	6 52·2	55 29 N.	,, 9,		*M*
U Boötis	14 49·2	18 9 N.	,, 10,		*M*
δ Libræ	14 55·1	8 5 S.	,, 9,	1 34	*m*
U Coronæ	15 13·7	32 3 N.	,, 11,	2 13	*m*
W Scorpii	16 5·3	19 51 S.	,, 10,		*M*
R Ursæ Minoris	16 31·5	72 30 N.	,, 11,		*m*
U Ophiuchi	17 10·9	1 20 N.	,, 10,	2 32	*m*
			and at intervals of	20 8	
R Scuti	18 41·6	5 50 S.	Apr. 8,		*m*
S Delphini	20 38·0	16 41 N.	,, 13,		*m*
T Vulpeculæ	20 46·8	27 50 N.	,, 10,	22 0	*m*
			,, 12,	0 0	*M*
W Cygni	21 31·9	44 53 N.	,, 8,		*m*
δ Cephei	22 25·1	57 51 N.	,, 12,	1 0	*m*

M signifies maximum ; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near β Ursæ Majoris ...	164 ...	58 N. ...	April 9–12.
,, ζ ,, ,, ...	206 ...	57 N. ...	Slow; bright.
	249 ...	51 N. ...	April 9–12.

THE FORCES OF ELECTRIC OSCILLATIONS TREATED ACCORDING TO MAXWELL'S THEORY. BY DR. H. HERTZ.[1]

III.

The Interference Experiments.

IN order to ascertain the velocity of propagation of electric force in the equatorial plane, we brought it into interference with another wave advancing with corresponding constant velocity in a wire. The result was, that the successive interferences did not occur at equal distances, but followed more rapidly in the neighbourhood of the oscillator than at greater distances.

This behaviour was explained by the supposition that the total force could be decomposed into two parts, of which the one, the electrodynamic, travelled with the velocity of light, while the other, the electrostatic, travelled with a greater, perhaps an infinite, velocity.

According to our theory now, however, the force in question in the equatorial plane is—

$$Z = Elm^3 \left\{ -\frac{\sin(mr-nt)}{mr} - \frac{\cos(mr-nt)}{m^2r^2} + \frac{\sin(mr-nt)}{m^3r^3} \right\},$$

and this expression in no way splits up into two simple waves travelling with different velocities. If, then, the present theory is correct, the earlier explanation can only be an approximation to the truth.

We will investigate whether the present theory leads to a general explanation of the phenomenon.

First, we can write $Z = B \sin(nt - \delta_1)$, where the amplitude

$$B = \frac{El}{r^3}\sqrt{(1 - m^2r^2 + m^4r^4)},$$

and the phase δ_1 is determined by the equation—

$$\tan\delta_1 = \frac{\frac{\sin mr}{mr} + \frac{\cos mr}{m^2r^2} - \frac{\sin mr}{m^3r^3}}{\frac{\cos mr}{mr} - \frac{\sin mr}{m^2r^2} - \frac{\cos mr}{m^3r^3}};$$

which, after transformation, gives—

$$\delta_1 = mr - \tan^{-1}\frac{mr}{1 - m^2r^2}.$$

In Fig. 5, the quantity δ_1 is represented as a function of mr by the curve labelled δ_1. The length ab corresponds in the

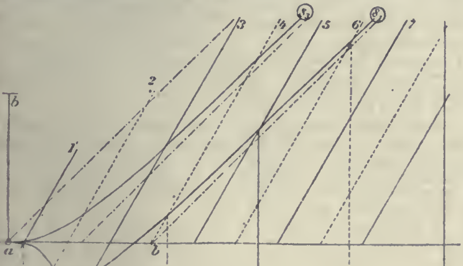


figure to the value of π, both for ordinates and abscissæ. If one considers, not mr, but r, as the variable abscissa, the length ab corresponds to the half wave-length.

In order to immediately attach the experiments which we wish to discuss, the abscissa axis is further divided into metres beneath the diagram. From the result of direct measurement (*Wied. Ann.*, xxxiv. p. 609, 1888), λ was equal to 4·8 metres, and thus the scale-length of a metre is determined. The zero of the scale is, however, not at the oscillator, but at a distance of 0·45 metre from it; for in this way the scale corresponds to the actual division of the base-line on which the position of the interferences was determined. One sees from the figure that the phase does not in general spread from the source, but its course is such that the waves arise at a distance of about $\frac{1}{4}\lambda$ from it, and give off thence a part to the conductor and a part out into space. At great distances, the phase is smaller by the value π than it would be if the waves had spread out with constant velocity from the source: the waves therefore behave, at great distances, as if they had traversed the first half wave-length with infinite speed.

The action, w, of the wire-waves at a definite position of the secondary conductor can now in any case be represented by the form $w = C\sin(nt - \delta_2)$; wherein the abbreviation—

$$\delta_2 = m_1 r + \delta = \frac{\pi r}{\lambda_1} + \delta$$

is used. λ_1 denotes the half wave-length of the waves in the wire, which in our experiment was 2·8 metres; δ, the phase of its action at the point $r = 0$, which we can arbitrarily change by adjustment of the length of the wire.

By this means we could change the amplitude, C, and give it such a magnitude that the action of the wave in the wire was approximately equal to that of the direct action. The phase of the interference depends, then, only on the difference of the phases δ_1 and δ_2. With the particular adjustment of the secondary circle to which our expression for w refers, both actions correspond (the interference has the sign +) if $\delta_1 - \delta_2$ is equal to zero, or a whole multiple of 2π; the actions disagree (the interference has the sign −) if $\delta_1 - \delta_2$ is equal to π, or any whole multiple of it. No interference occurs (the interference has the sign 0) if $\delta_1 - \delta_2$ is a whole multiple of $\frac{1}{2}\pi$.

We will now so determine δ that at the zero of the metre-divisions the phase of the interference has a definite value, δ_0, so that $\delta_1 = \delta_2 + \delta_0$.

The straight line 1 of our figure shall then represent the value of $\delta_2 + \delta_0$ as a function of the distance. The line is specially drawn with such a slant that for an increase of abscissa by $\lambda_1 = 2·8$ metres the ordinate increases by the value π, and is so arranged that it cuts the curve δ_1 in a point whose abscissa is the zero of the metre-divisions.

The lines 2, 3, 4, &c., represent further the course of the values $\delta_2 + \delta_0 - \frac{1}{2}\pi$, $\delta_2 + \delta_0 - \pi$, $\delta_2 + \delta_0 - \frac{3}{2}\pi$, &c. These lines are parallel to the line 1, and so drawn that they cut one and the same ordinate at distances of $\frac{1}{2}\pi$, and one and the same abscissa at distances of 1·4 metre.

Project now the section of these straight lines with the curve δ_1 on the axis of abscissæ below, and one gets immediately those distances for which $\delta_1 = \delta_2 + \delta_0 + \frac{1}{2}\pi$, $\delta_2 + \delta_0 + \pi$, $\delta_2 + \delta_0 + \frac{3}{2}\pi$, &c., for which, therefore, the phase of the interference increases from the origin by $\frac{1}{2}\pi$, π, $\frac{3}{2}\pi$.

One obtains immediately from the figures the following:—

If the interference at the zero of the base-line possesses the sign + (−), it vanishes for the first time at a distance of about 1 metre; it attains the sign − (+) at about 2·3 metres, vanishes again at 4·8 metres, and reverts back to the sign + (−) at about 7·6 metres; it is again 0 at 14 metres, and from thence onward the signs recur at fairly regular intervals. If at the zero of the base-line the interference was zero, it will be zero again at about 2·3, 7·6, and 14 metres, while it has a prominently positive or negative character at about 1 metre, 4·8 metres, 11 metres distance from the zero. For mean phases mean values serve.

If one compares with this result of theory the result of experiment, especially those interferences which occurred with arrangements of 100, 250, 400, 550 centimetres of wire (*Wied. Ann.*, xxxiv. p. 563), one will find a correspondence as complete as can be at all expected.

I do not succeed quite so well in calculating back to the interferences of the second kind (*l.c.*, p. 565). To get these we used an arrangement of secondary circle by which the integral force of induction through the closed circuit came prominently into account. If we regard the dimensions of the latter as vanishingly small, the integral is proportional to the rate of change of the magnetic field normal to the plane of the circle, and hence to the expression—

$$\frac{dP}{dt} = AElm^2n^2\left\{ -\frac{\cos(mr - nt)}{mr} + \frac{\sin(mr - nt)}{m^2r^2} \right\}.$$

[1] Translated and communicated by Dr. Oliver Lodge. Continued from p. 452.

Hence we get for the phase δ_3 this expression—

$$\tan \delta_3 = -\frac{\frac{\cos mr}{mr} - \frac{\sin mr}{m^2r^2}}{\frac{\sin mr}{mr} + \frac{\cos mr}{m^2r^2}}$$

or, after transformation—

$$\delta_3 = mr - \tan^{-1} mr.$$

The line δ_3 of our Fig. 5 represents this function. One sees that for this action the phase steadily increases direct from the origin. Those phenomena, therefore, which indicate a finite pace of propagation must make themselves apparent by interferences even close to the vibrator. So it shows itself in these experiments, and just herein consists the advantage which we derived from this kind of interference experiment. But the apparent velocity comes out greater in the neighbourhood of the vibrator than at a distance, and it is not to be denied that the phase of the interference must theoretically change less, but notably more quickly than was experimentally the case.

It appears to me probable that a more complete theory, one which does not consider both conductors as vanishingly small —perhaps, also, another estimate of the value of λ—would here afford a better correspondence.

It is of importance that even on Maxwell's theory the experiments cannot be explained without assuming a marked difference between the velocity of waves along wire and their velocity in free space.

(*To be continued.*)

NOTE ON THE USE OF GEISSLER'S TUBES FOR DETECTING ELECTRICAL OSCILLATIONS.

AT the suggestion of Prof. Lodge, I undertook to repeat in the Physical Laboratory of the University College, Liverpool, Hertz's celebrated experiments on electrical oscillations.

In performing these experiments, I was searching for means to make the effect of the electrical oscillations more easily observable, and I was induced to use for this purpose (1) Geissler's tubes, in order to strengthen the visible effect; and (2) the chemical action of the oscillating currents (paper soaked in solution of iodide of potash), in order to obtain a permanent trace of them.

For the present I will describe briefly the results of the use of Geissler's tubes.

In order to produce the electrical oscillations, I used a conductor consisting of two zinc plates, about 41·5 centimetres square, suspended in the same plane 55 centimetres apart; to each plate was fastened a No. 6 copper wire, which was finished off with a small brass knob. The two brass knobs were about 5 millimetres apart, and formed the *sparking gap*, as we shall call it. As receiver of the oscillations, I used, like Hertz, circles of No. 14 wire, 35 centimetres in radius.

After the example of Mr. F. T. Trouton (NATURE, February 21, p. 391), I will call the first conductor a *vibrator*, and the wire circles, or other receivers, resonators.

The vibrator was connected with a small coil, 20 centimetres long, supplied with an ordinary spring interrupter, and excited by four secondary cells.

If we connect one electrode of a convenient Geissler's tube with either side of the sparking gap of the resonator, currents pass through or into the tube, which lights up and so makes the effect of the electrical oscillations on the resonator visible even at a great distance.

Of the few tubes which were at my disposal, I found that the most convenient for this purpose was a small one with electrodes 8·5 centimetres apart, and filled with highly rarefied air. But spectral tubes 20 centimetres long and filled with hydrogen, oxygen, or nitrogen also gave good results.

With the first mentioned tube I perceived *a visible effect*, when the resonator was held horizontally in the plane containing the wires of the vibrator, and with the sparking gap turned towards it, at a distance of 4 metres from the vibrator. By this arrangement all the phenomena described by Hertz (*Wiedemann's Ann.*, xxxiv. p. 160, 1888) about the direction of the electrical lines of force can easily be shown.

A very instructive experiment is to show the directions of these lines by several resonators disposed round the vibrator. For this purpose I suggest the following apparatus:—

On a wooden frame mounted so as to be able to revolve on a vertical axis standing under the sparking gap of the vibrator are fastened several resonators, with their planes vertical and parallel respectively to the directions of the lines of force and the sparking gaps at the highest point. These resonators are supplied with Geissler's tubes. In this position of the resonators all the tubes will lighten up when the vibrator is working. But if the frame with the resonators moves round the vertical axis, the light of the tubes will become weaker, and, when the frame is turned 90°, the tubes will become quite dark; the planes of all the vibrators in this position being perpendicular to the directions of the lines of force. This change will occur inversely by turning the frame from 90° to 180°.

If, instead of one resonator, two are fastened to each point of the frame, one perpendicularly to the other, both being vertical, the changes in either of these will be contrary—that is to say, when the light in one set of the tubes becomes brighter it becomes weaker in the set perpendicular to it and *vice versâ*. Thus the strength of the light is, so to say, proportional to the magnitude of the components of the lines of force in the direction of the tubes.

If a disconnected Geissler's tube is held near the vibrator, it begins in a short time to light up, owing to oscillatory currents passing through it. The same effect is obtained if instead of holding the tube by the hand it reposes on an insulating body. This lighting occurs at all points near the vibrator, except about the sparking gap. The tube becomes quite dark if the hand or a conductor is interposed between it and the vibrator; on the contrary, the interposing of an insulating body causes no change in the tube. The tube becomes more sensitive if a portion of it is surrounded with tinfoil.

In this way the existence of electrical oscillations in space can be ascertained, and also the transparency of insulating bodies and the opacity of conductors for electrical oscillations can be demonstrated.

When the two electrodes of a Geissler's tube are connected with two different points of a resonator, the effect in the tube is produced by the difference of potential of the two points. If now we connect one point of the vibrator or the resonator with one electrode of the tube, the other electrode hanging free in the air or being earthed, we have an alternative current through the tube whenever the potential of the point connected with the electrode becomes different from zero, and thus the tube lights up. The effect is strengthened if one portion of the tube is surrounded by tinfoil. This is a very convenient arrangement for observing the form of the electrical oscillation in conductors.

If we investigate in this manner our circular resonator held vertically before the vibrator, with its plane parallel to it and the sparking gap upwards, we find that a tube hanging at the lower end of the vertical diameter of the circle, opposite to the sparking gap, remains quite dark, and lights up when moved to the right or to the left of this point. The light becomes brighter till the horizontal diameter is reached; further on the light begins to grow weaker till the sparking gap is attained, where the tube, however, continues to lighten. The light becomes weaker when the sparking gap is narrowed, and ceases when it is quite closed. Thus we see that the circular resonator possesses one node at its lowest point, two ventral segments at equal distances from the node and the sparking gap, and two minima of oscillation one on each side of the sparking gap.

That a node is situated at the point opposite to the sparking gap is also ascertained by observing that by touching this point with the finger or by hanging from it a piece of wire or by connecting it to earth, no change is to be remarked in the spark of the resonator. These manipulations, if applied to another point of the resonator, diminish the spark.

If the resonator is formed by a closed circle of wire, we find a node at each end of the vertical, and a segment at each end of the horizontal, diameter of the circle. The distance between the two nodes being here 110 centimetres, the wave-length is 220 centimetres, while the length of the primary wave is about 880 centimetres. Thus the wave-length in the resonator corresponds to the second higher octave of the fundamental oscillation.

If, instead of circular, we use linear resonators placed parallel to the vibrator, we must be very careful to distinguish between the effect produced directly from the vibrator in the Geissler tube and the effect caused by the oscillations of the resonator. In the case of the circular resonators, placed in the position above described, one need not trouble much about the direct effect of the vibrator, this being very small in the neighbourhood of the vibrator's sparking gap.

To prevent the direct action a small uninsulated metal screen can be placed between the vibrator and the tube, or the tube can be hung by a long and fine wire, in order to be removed from the sphere of the direct action of the vibrator. The best plan is, however, to surround the tube by wire gauze, which stops the direct action of the vibrator on the tube, and yet permits the tube to be observed. The absence of direct action can be ascertained in the different positions which the tube takes by insulating it from the resonator without changing its position, and noticing if it becomes quite dark.

If we place before the vibrator a resonator, consisting of a straight wire 220 centimetres long (I used copper wire No. 6), we find, by the tubes, that nodes exist in the middle and the two ends of the wire, consequently two segments at 55 centimetres from each end of the wire.

(The above-described circular resonator can be likened to a linear one which is curved to a circle and its two ends soldered together, thus the two nodes of the ends becoming one single node.)

If this straight wire is cut in the middle, a torrent of sparks passes between the separated ends, even if they are removed several millimetres apart. If then we examine each half of the wire, we find that it possesses a node in the middle and two segments, each at one end, but the node is not so well defined as in the case of the uncut wire; there is no single point the potential of which remains continually equal to zero, but a line in which the difference of potential from zero is a minimum. This complicated form of oscillation is produced by the fact that the forces acting in this resonator are not equal at all points or symmetrically distributed with respect to it, as in the case of the long resonator. The oscillations of the short resonator may be compared to those of a rod which is not firmly fixed by its middle. The state of these oscillations is not stable. If one or both ends of this resonator are touched by the finger, they become nodes, and a well-defined segment appears at the middle of the resonator. If the Geissler tube be connected with this middle point, it begins to light up when the ends of the resonator are touched, and ceases to light the moment the fingers are removed; the contrary takes place if the tube be connected with one end of the resonator. This phenomenon is analogous to the change of the form of the vibration of a rod when fixed by its middle or by its ends.

Quite similar is the mode of oscillation of a resonator 220 centimetres long disposed on one side of the vibrator; it possesses also a node at the middle not well defined, and a segment at each end.

To conclude, I will describe the mode of oscillation of a resonator, 110 centimetres long, disposed parallel and symmetrically to the vibrator. This resonator possesses one node in the middle, very clearly defined, and a segment at either end. This form of oscillation is the same as would occur in the long resonator if one-fourth of its length from each end were cut off.

In the case of the latter resonator and of the long one, which is also symmetrically disposed to the vibrator, the oscillations are very stable, and much stronger than in the case of the resonators placed on one side of the vibrator. The mode of the oscillations of these symmetrical resonators is not disturbed by touching them by the fingers at any point, although the mode of oscillation is disturbed if we touch the unsymmetrical resonators at any point whatever.

The experiments described must be performed in a dark room, and much care be used in the choice of the proper Geissler tubes. Tubes containing mercury are very sensitive, and they become more so if the mercury be allowed to flow several times from one end of the tube to the other.

Liverpool, March. E. J. DRAGOUMIS.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 21.—"An Experimental Investigation of the Circumstances under which a Change of the Velocity in the Propagation of the Ignition of an Explosive Gaseous Mixture takes place in Closed and Open Vessels. Part I. Chronographic Measurements." By Frederick J. Smith, M.A., Millard Lecturer on Mechanics, Trinity College, Oxford. Communicated by A. G. Vernon Harcourt, F.R.S.

It bas been noticed by several investigators, viz. MM. Berthelot and Vielle, MM. Mallard and Le Chatelier, and Prof. H. B. Dixon, F.R.S., that explosive gaseous mixtures after ignition do not reach their maximum velocity of propagation at once, but that a certain maximum velocity is attained soon after initial ignition.

In order to investigate this period, which may be called the acceleration period of an explosion, chronographic measurements of a peculiar nature were found necessary.

It was at once evident that but little advance in this branch of the subject of explosions could be made unless exceedingly minute periods of time could be measured with certainty.

A new form of chronograph has been devised to meet as far as possible all the requirements of the case, by means of the instrument. The following results have been obtained:—

(1) The $\frac{1}{20000}$ of a second can be measured with ease, and periods of time differing from $\frac{1}{10}$ of a second to $\frac{1}{20000}$ of a second can be recorded on the same moving surface.

(2) The surface which receives the record moves at a velocity which is practically constant during the traverse of 5 cm.; also its velocity can be varied between wide limits.

(3) A large number of time records can be made side by side, all records being made in straight lines.

(4) Fractions of recorded vibrations of a fork can be subdivided by means of a micrometer microscope. This is not the case with vibrations recorded on a surface attached to a pendulum, where the velocity varies from zero up to a maximum at the middle of the swing.

The electro-magnetic styli, by means of which events are marked, are so constructed that their period of "latency" is almost absolutely constant, and their electro-magnets are so wound that no sparking takes place on breaking the circuit.

A moving surface is carried on a carriage, which is propelled by means of a falling weight, which after a certain velocity has been attained is removed: the surface then moves with a velocity which is found to be practically constant for the limits between which a time record is made.

The chronograph is used in conjunction with a steel tube in which the explosions take place. At even distances along the axis of the tube, conducting bridges, eight to ten in number, of Dutch metal insulated from the tube, are placed; each bridge is connected electrically with a recording stylus, so that as each bridge is broken by the explosion, a mark is made on the surface of the chronograph; these markings when duly interpreted provide data for constructing a curve, which indicates the rate at which the velocity of the explosion is changing during its propagation.

The rest of the paper treats of the methods by means of which the errors due to the use of electro-magnets in chronographic work have been dealt with and reduced as far as possible.

Chemical Society, March 7.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—The decomposition of carbon disulphide by shock; a lecture experiment, by Prof. T. E. Thorpe, F.R.S. The author, in studying the action of the fluid alloy of potassium and sodium on carbon disulphide, obtained a yellowish-brown solid substance which exploded with great violence when subjected to pressure or friction. If the explosion occurred in contact with carbon disulphide, that substance was resolved into its elements. A similar decomposition of carbon disulphide into carbon and sulphur can readily be effected by exploding a charge of 0·05 gramme of fulminate within a stout glass tube containing carbon disulphide vapour, and the experiment affords a good illustration for class purposes of the resolution of an endothermic compound into its elements by sudden shock.—The determination of the constitution of the heteronucleal $\alpha\beta$- and $\beta\beta$-di-derivatives of naphthalene, by Prof. H. E. Armstrong and Mr. W. P. Wynne. A preliminary note on the constitution of the three chloramidonaphthalenesulphonic acids obtained by sulphonating α-chloro-β-amidonaphthalene hydrochloride with weakly fuming sulphuric acid.—The action of chloroform and alcoholic potash on phenylhydrazine, by Dr. S. Ruhemann.

March 28.—Annual General Meeting.—The following is an abstract of the Annual Report, read by the President, Mr. W. Crookes, F.R.S.:—The pleasant duty again devolves on me to present to you the annual report on the state of the Chemical Society during the twelve months just past. The following statement shows the numerical position of the Society—

Number of Fellows (March 28, 1888) ...	... 1534
Present number of Fellows	... 1614
Increase	80

103 papers have been communicated to the Society this session. Our library continues to increase, and every year becomes richer in rare volumes and books of reference. The duplicate library for lending is also becoming increasingly useful. The expenditure under this head for the current year is £308 5*s.* 6*d.*

I must now ask your attention to an event of which none of you can be ignorant, which, though not exclusively relating to chemistry, bears closely upon it and upon the future of British science. I refer to the protest against the examination system in education which appeared in November last. That protest had long been in the air. For years past, men who take the trouble to observe and to reflect have come to the conclusion that competitive examination is injurious to the individual, injurious to the race, and that it starves original research at the root. They have convinced themselves that if we flag in scientific investigation, that if a large and increasing proportion of professorships and of leading positions in industrial establishments, both in the home kingdom and in the colonies, are filled by aliens, the fault lies mainly with our educational system. Men trained chiefly to pass examinations either in theoretical or practical departments cannot equal those who have been schooled in actual research, trained to accurately observe and draw correct inferences from facts. All the earlier protests were desultory, and calculated to produce no lasting impression ; but the recent manifesto is the expression of the collective opinion of many earnest representative men and women. Hence it cannot be slighted as the mere outcry of a faction, a sect, a school, or an interest. A most satisfactory feature is the adhesion to the protest of men who formerly were in favour of competitive examination as the test for entrance into the civil or military service of the State. Prof. Max Müller, of Oxford, frankly admits he now considers competition to be a mistake, and avers that the failure springs not only from the manner in which the system has been worked, but is involved in its very nature. But if this protest is to avail it must be energetically followed up, for I must repeat what I have before declared, that the position of science in Britain is far from satisfactory. Though the number of articles devoted to research in German Transactions and journals exceeds those in our own publications, we must remember that the population of the German Empire is greater than that of the United Kingdom by at least one-fourth ; further, that the *savants* of Russia, of the Austrian Empire, of Switzerland, of Holland, and Scandinavia, largely select German journals as their medium of publication. Not a few English and American scientific men follow the same course. Hence, as regards quantity, our share in the world's scientific work is more considerable than appears at the first glance. Further, I think that if deficient in quantity English research excels in quality. If we do less detail work we furnish a larger proportion of generalizations and laws than most of our rivals. As the discoverers of laws and generalizations, Black, Boyle, Dalton, Faraday, Graham, Joule, Newton, Wollaston, and Young are household words in the laboratory—yet none of these men were the products of the examination system. There is another evil against which I must strongly protest. I refer to the system of "sealed papers." Everyone knows that on the Continent, more especially in France, it is common for anyone who has, as he thinks, approached the solution of some important question, to deposit a sealed sketch of his incomplete results with the President or Secretary of some learned Society. The sketch may lie *perdu* for years, until the author requests it may be opened and read before the Society. The practice arose from a desire that the author's priority should be guaranteed against others who might lay claim to his ideas. But priority can be quite as effectually secured by a brief preliminary notice read before some Society or sent to some journal, the author thus reserving to himself the further investigation of the subject. Among men of honour such reservations are invariably respected. But the "sealed paper" system lends itself to something which borders unpleasantly upon fraud. Suppose an investigator takes up some question, sees that it admits of two or more solutions, or that various hypotheses present themselves to him as possible. To work out the matter conclusively might require much time and trouble. He therefore writes out each hypothesis, and incloses them separately in "sealed papers," duly numbered, carefully retaining copies. In process of time some other investigator, ignorant of what the first author has done, takes up the subject, and works out one of these hypotheses to demonstration. So soon as his supplementary memoir is before the world the first investigator requests that the "sealed paper" No. 2 or No. 3 be opened and read. The new theory, laboriously considered and worked out, is found to have been anticipated, and the man who has really done the work is robbed of much of his credit. The seeming anticipator says nothing about the contents of other "sealed papers," in which he has proposed totally different hypotheses: these he now leaves to oblivion. I think the Fellows of our Society will agree with me that a system which thus enables a man to reap the fruit of another man's experiments does not deserve to be naturalized in England. There is a further abuse to which attention may usefully be drawn. It sometimes happens a man of science will send an account of researches he has completed to two journals simultaneously, English or foreign, leaving each editor under the impression that he is the sole recipient of the communication. Or, still worse, a man reads a paper before our Society, and sends it to some foreign journal, so that it may figure in print before it appears in the Society's Transactions. To this subject I felt compelled to refer when I had the honour of addressing you last year. And you are now aware, your Council declines to publish any memoir which has previously appeared in a foreign journal, unless specially recommended by the Publication Committee and approved by the Council. The reasons for this resolution are not hard to seek. Not merely is the reputation of the Society, as the original channel of the researches in question, imperilled, or at least obscured, but a serious waste of time and labour is inflicted upon anyone who needs to read up the literature of the subject. We in England are by no means the only sinners in this respect. It often happens that memoirs which have been read before the Paris Academy of Sciences reappear as "original matter" in certain French journals. I cannot pass over a discovery made this season by Prof. Krüss concerning nickel and cobalt. As at first reported it seemed that these two metals might be eliminated from our text-books, and that two or three new substances would take their place. Had this been the case, it would undoubtedly have been one of the greatest steps in pure chemistry taken this century. It now appears that each of the two metals contains a common impurity, which Prof. Krüss has been the first to detect and isolate. Nickel and cobalt thus purified will still retain their individuality, though their accepted properties, physical and chemical, will need careful revision. In any case the discovery is most instructive, warning us how careful we should be to have firm ground under our feet. It is almost humiliating that two metals which have been subjected to infinite research and scrutiny should now be found to contain such a proportion of unsuspected impurity. You are aware that at the ballots for the election of Fellows half an hour or more of valuable time is spent in a manner which, to say the least, is not very interesting. An attempt has been made to save time by taking the ballot in the library, after the meeting, but so many Fellows leave before the end of the meeting that the number remaining has not been found sufficient to meet the requirement of the by-laws. Your Council have from time to time had this matter under discussion, and at their last meeting, on March 21, it was resolved "that in future the balloting for Fellows be conducted by means of papers." The best manner of carrying out this resolution will be a subject for future arrangement. A posthumous memoir on the compressibility of hydrogen, by the late Prof. Wroblewski, reminds us of the sad and untimely death of this meritorious and distinguished worker in physical chemistry. His death, as most of us doubtless are aware, was due to the frightful burns which he received from the overturning or explosion of a paraffin lamp. In the memoir in question Prof. Wroblewski treats of the compressibility of hydrogen at 99°, at 0°, at $-103°\cdot5$ (boiling-point of ethylene), and at $-182°\cdot4$ (boiling-point of oxygen), for pressures ranging from 1 to 70 atmospheres. From the results the following data were calculated : critical temperature $-240°$; critical pressure, 13·3 atmospheres ; critical volume, 0·00335. Hence it appears very doubtful whether M. Pictet or M. Cailletet really succeeded in liquefying hydrogen. Last year I had the pleasure to announce that one of our Fellows, Mr. Newlands, had received the "Davy Medal" of the Royal Society for his splendid discovery of the Periodic Law of the Chemical Elements. I may also be allowed to state that to me, your President, the Royal Society has likewise awarded the same distinction for my researches on the behaviour of substances under the influence of the electric discharge in a high vacuum, with especial reference to their spectroscopic reactions. Hence it has been suggested that I might not unprofitably claim your attention this evening for a history of the so-called rare earths, as they have been brought to light and discriminated by the aid of the spectroscope. [We print elsewhere Mr. Crookes's address on this subject.]

Linnean Society, March 21.—Mr. Carruthers, F.R.S., President, in the chair.—Mr. T. Christy exhibited the pod (36 inches in length) of an Apocynaceous plant received from Gaboon as *Strophanthus*, but believed to be allied to the *Holarrhena*.—Prof. Stewart, referring to the specimens of *Noctilio leporinus* exhibited at the last meeting of the Society, stated that he had examined the contents of the stomachs submitted to him by Mr. Harting, and had found without doubt fragments of fish, scales, and fin-rays, and a portion of the lower jaw of a small fish, proving the correctness of the assertions which had been made regarding the piscivorous habits of this bat.—Mr. W. B. Hemsley furnished a report on the botanical collections made on Christmas Island during the voyage of the *Egeria*. This included a complete list of the plants collected, with remarks on their general distribution, the author being of opinion that the flora of this island, which lies about 200 miles south of the western end of Java, was more nearly related to that of the Malayan Archipelago than to that of Australia. Mr. C. B. Clarke, commenting on the author's observations on the buttresses of trees, described some remarkable instances which he had seen of this singular mode of growth. Mr. J. G. Baker, referring to the Ferns which had been collected, noticed their affinities and distribution. Mr. R. A. Rolfe commented on three species of Orchids which had been brought home by this Expedition, all of which were new. Mr. Thiselton Dyer, referring to Mr. Lister's Report to the British Association on the zoological collections from this island, in which it was stated that the character of the avifauna was Australian, considered that this was not borne out by an examination of the flora, which was decidedly Malayan.—A paper was then read by Mr. R. A. Rolfe on the sexual forms of *Catasetum*, with special reference to the researches of Darwin and others. The purport of Darwin's paper (Journ. Linnean Soc., 1862) was to show that *Catasetum tridentatum* had been seen by Schomburgk to produce three different kinds of flowers, belonging to the same number of supposed genera, all on the same plant, and that the three represented respectively the male, female, and hermaphrodite states of the species. Mr. Rolfe showed that Schomburgk's remarks applied to two distinct species, *C. tridentatum* and *C. barbatum*, the females of which resembled each other so closely that they were thought to be one and the same—namely, *Monacanthus viridis*. Neither of these, however, belonged to the true plant of that name, which was really the female of another species—namely, *C. cernuum*, a fact hitherto unsuspected. The key of the situation was that the females of several species resemble each other very closely, and to three of them the name *Monacanthus viridis* had been applied.—After some critical remarks by the President and Mr. Bull, a paper by Mr. MacOwan was read, on some new Cape plants.

Geological Society, March 6.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—On the subdivisions of the Speeton Clay, by G. W. Lamplugh. Communicated by Mr. Clement Reid. The reading of this paper was followed by a discussion, in which Prof. Judd, Mr. Strahan, Prof. Blake, Mr. Hudleston, and Mr. Herries took part.—Notes on the geology of Madagascar, by the Rev. R. Baron. Communicated by the Director-General of the Geological Survey. With an appendix on some fossils from Madagascar, by Mr. R. Bullen Newton. The central highlands of Madagascar consist of gneiss and other crystalline rocks, the general strike of which is parallel with the main axis of the island, and also, roughly, with that of the crystalline rocks of the mainland. The gneiss is frequently hornblendic; its orthoclase is often pink; triclinic felspar often occurs in places; biotite is the most common mica, but muscovite is not uncommon; magnetite is generally present, often in considerable quantities. The gneiss is often decayed to great depths, forming a red soil, and the loosened rock is deeply eaten into by streams. The harder masses of gneiss, having resisted decay, stand out in blocks, and have been mistaken for travelled boulders of glacial origin. Other more or less crystalline rocks are mica-schists, chlorite-schists, crystalline limestone, quartzite (with which graphite is often associated), and clay-slate. Bosses of intrusive granite rise through the gneiss. That east of the capital contains porphyritic crystals of felspar which near the northern edge of the granite are arranged roughly in a linear direction; here also the granite contains angular fragments of gneiss. For the most part the granite of Madagascar is clearly intrusive, but this may not always be the case. The volcanic rocks are of much interest. The highest mountains, those lying to the south-west of the capital, consist, in their higher parts, of a mass of lava, for the most part basaltic, but with some sanidine-trachyte. The lava-streams are sometimes twenty-five miles long, and successive flows, up to 500 feet in thickness, are exposed by the valleys. From the great denudation which this area has undergone, and from the fact that no cones now remain, we may assume that this volcanic series is of some antiquity. Of the newer volcanic series there are numerous very perfect cones, dotting the surface of the gneiss in many places. No active volcano now exists in the island, but the occasional emission of carbonic acid gas, the occurrence of numerous hot springs and deposits of siliceous sinter, and the frequency of small earthquake-shocks, seem to show that volcanic forces are only dormant and not entirely extinct. The ashes generally lie most thickly on the side of the cone between north and west; this is accounted for by the prevalence of the south-east trade-winds. The volcanic areas are ranged roughly in a linear direction, corresponding with the longer axis of the island. Sedimentary rocks occur mainly on the western and southern sides of the island. The relations of these to each other have not yet been determined; but from the fossils (referred to the European standard) it seems that the following formations are represented: Eocene, Upper Cretaceous, Neocomian, Oxfordian, Lower Oolites, Lias. Possibly some of the slaty beds may turn out to be Silurian or Cambrian. The crystalline schists, &c., are probably, for the most part at least, Archæan. Recent deposits fringe the coasts, and are largely developed on the southern part of the island. East of the central line of watershed there is a long depression containing a wide alluvial deposit, probably an old lake-bed. Terraces fringe its sides in many places. The lagoons of the eastern coast are due to alluvial deposits. The paper concluded with some remarks on the geological antiquity of the island, its separation dating from early Pliocene times, if not earlier. This is the conclusion arrived at by Wallace from its fauna; the author's detailed researches into its flora, recently described before the Linnean Society, show that while about five-sixths of its *genera* of plants are also found elsewhere, chiefly in tropical countries, at least four-fifths of its *species* are peculiar to Madagascar. The appendix, drawn up by Mr. R. Bullen Newton, consisted of notes upon the fossils collected by the author, with tables, and descriptions of two new species—namely, *Astarte* (?) *Baroni* and *Sphæra madagascariensis*, both from deposits of Lower Oolitic age.—Notes on the petrographical characters of some rocks collected in Madagascar by the Rev. R. Baron, by Dr. F. H. Hatch. Some remarks on Mr. Baron's paper were made by the President, Dr. Geikie, Mr. H. B. Woodward, and Mr. Topley.

PARIS.

Academy of Sciences, March 25.—M. Des Cloizeaux, President, in the chair.—On the achromatism of interferences, by M. Mascart. The conclusions arrived at by Cornu and Stokes are here applied to the particular cases of interference fringes and of Newton's rings. In the phenomenon of W. Herschel's fringes the condition of achromatism is shown to be—

$$\frac{\cos^2 i}{\sin i} = L \frac{\sin A}{\cos r}.$$

—Remarks accompanying presentation of Prof. Karl Pearson's work, "The Electrical Researches of Barré de Saint-Venant" (Cambridge, 1889), by M. Boussinesq. The period from 1850 to 1886, covered by this important treatise, comprises the most remarkable researches, by the late M. de Saint-Venant, on torsion, flexion, live resistance, the distribution of elasticities in heterotrope bodies, plasticodynamics, &c. The work, which will be found of great service to English physicists, geometricians, and engineers, unfamiliar with the French language, forms the first part of the second volume of the series begun by Todhunter on the "History of the Theory of Elasticity."—On elliptical polarization by vitreous reflection, by M. A. Potier. Rejecting Cauchy's assumption of evanescent longitudinal waves, the author here develops a theory in which he takes as his starting-point the differential equations of the vibratory movement. The principle and results of this theory were already announced at the meeting of the French Association for the Advancement of the Sciences in 1872.—Researches on the cultivation of the potato, by M. Aimé Girard. The author here deals with the progressive development of the plant, and arrives at the general conclusion that the origin of the starch is to be sought in the leaves, where it is probably represented in its initial form by saccharose, or

some analogous sugar. By its twofold decomposition this sugar becomes on the one hand the generator of the cellular tissue, on the other of the starch which is stored up in that tissue.—On the peroxides of cobalt and nickel, and on the volumetric analysis of these metals, by M. Adolphe Carnot. The action of potash combined with that of chlorine, bromine, iodine, or of an alkaline hypochlorite, yields in cobalt and nickel solutions certain black granular precipitates almost identical in appearance. Herrenschmidt, however, has pointed out that the peroxide of cobalt thus obtained has a brown colour, while the peroxide of nickel remains black under the microscope. M. Carnot here describes a series of experiments carried out for the purpose of determining the state of oxidation of the metals in these various precipitates. The general result is that the brown oxide obtained by precipitating cobalt with hydrogen dioxide and caustic potash at the boiling-point has the exact composition of the sesquioxide, Co_2O_3, and that the black oxide of nickel, precipitated by hypochlorite or by bromine and potash, is the sesquioxide, Ni_2O_3.—On the limits of the errors that may be committed in assaying fine gold, by M. Paul Charpentier. The figures here given are the result of about 300 assays executed by the author at the laboratory of the French Mint.—On the initial phase of electrolysis, by M. Piltschikoff. A protracted study of the phenomenon of retardation in the electrolytic process leads to the following results. The minimum electromotive force required to at once set up a visible electrolysis does not depend within certain limits on the nature of the salt, nor on the concentration of the solution (gold, zinc, sulphate of zinc; platinum, copper, sulphate of copper, nitrate of copper, gold, platinum or silver, &c.). The minimum does not depend perceptibly either on the heat of combination of the two metals, or on their contact electromotive force; but it depends essentially on the physical state of the cathode (negative pole), which may modify the resulting figures as much as 20 or even 25 per cent.—On the electric transport of salts in solution, by M. A. Chassy. The special case is here considered of a non-electrolyzed metallic salt, a salt of zinc, for instance, in a mixture of salts of copper and zinc.—On the glycol-ether of chloral, by M. de Forcrand. The author has prepared this compound,

$$CCl_3.CH(OH).OC_2H_4OH,$$

in the crystallized state, by combining molecular proportions of chloral and glycol at the ordinary temperature. It is soluble in water, and melts at 42° C., which is also the melting-point of chloral ethylate, according to M. Berthelot.—Determination of the heats of combustion of metaldehyde, erythrite, aud tricarballylic acid, by M. Louguinine. These experiments have been carried out by means of the calorimetric apparatus under precisely the same conditions as those already published.—Papers were contributed by MM. J. Héricourt and Ch. Richet, on the varying toxic effects of the blood of the dog transfused into the rabbit; by M. V. Galtier, on the liability of sheep and other animals to contract infectious pneumo-enteritis, hitherto regarded as a disease peculiar to the pig; by M. Joannes Chatin, on the homologies of the inferior lobes in the brain of fishes; and by MM. Jules de Guerne and Jules Richard, on the fresh-water fauna of Greenland.

BERLIN.

Physical Society, March 8.—Prof. von Helmholtz, President, in the chair.—Dr. Rubens described the experiments which he had made on the selective reflection of light by metals. The method employed was as follows: the light emitted by an incandescent plate of zirconium was concentrated by a lens on to a mirror-surface of the metal under investigation, and the reflected rays were then allowed to fall into a spectroscope with flint-glass prism, whose ocular had been replaced by a bolometer. In this way the intensity of each part of the spectrum could be determined. The next step consisted in removing the mirror and putting the glowing zirconium in the place of the virtual image of the first source of light, in such a way that the rays of light, coming from the point previously occupied by the mirror, pursued the same course as in the first experiment. These rays were then allowed to fall into the spectroscope, and the intensity of each part of the spectrum thus formed by light which had undergone no change by reflection was measured by the bolometer. The intensity was determined at fifteen different points in the spectra, extending from near F in the blue into the ultra-red down to the wavelength 2μ. The changes produced in the light by reflection from the metals were represented by curves whose abscissæ corresponded to wave-lengths while their ordinates corresponded to the intensities of the several rays after reflection. The results thus obtained showed that silver possesses even for blue rays a very considerable reflexive power, which gradually increases and reaches its maximum in the red, at which maximum the intensity of the reflected light then remains constant even for rays of the greatest wave-length. Gold possesses a much smaller reflexive power for blue and green rays; the curve then rises very rapidly to a maximum in the yellow and falls again towards the red. Copper reflects the blue and green rays even less than gold does: its reflexive power then increases rapidly into the red, and then somewhat more slowly, until in the ultra-red it reaches a value equal to that for silver. Iron and nickel gave very similar curves, rising at first somewhat rapidly, but subsequently more slowly and continuously into the ultra-red, without however reaching the maximal values observed for copper or silver. On the basis of these experimental values for the reflexive power of the above five metals, the speaker had calculated their coefficients of extinction and refraction for red and blue light, making use of Cauchy's and Beer's formulæ. From this it was possible to deduce the dispersive powers of the metals, and to compare their indices of refraction with those which had been experimentally determined by Prof. Kundt: the agreement was in most cases very close.—Prof. Preyer gave an account of some letters of Robert Meyer which are shortly to be published. They were written in the years 1842 and 1844 to his friend Dr. William Griesinger. Prof. Preyer read out several characteristic passages from these letters, in which Meyer states how he arrived at his discovery of the conservation of energy, and from which his firm belief in the correctness of his theory is quite apparent. No less characteristic is the way in which Meyer takes pains to explain his theory to his medical friend, who was but little experienced in physical matters, and to put it before him in a way which he could easily understand.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Class-book of Geography (revised edition): C. B. Clarke (Macmillan).—A Treatise on Chemistry, vol. iii., Part 5: Roscoe and Schorlemmer (Macmillan).—The Principles of Empirical or Inductive Logic: J. Venn (Macmillan).—Borneo; Entdeckungsreisen und Untersuchungen; Gegenwärtiger Stand der Geologischen Kenntnisse; Verbreitung der Nutzbaren Mineralien: Dr. T. Posewitz (Berlin, Friedländer).—Tägliche Oscillation des Barometers: J. Hann (Wien).—Journal of Physiology, February (Cambridge).—Records of the Geological Survey of India, vol. xxii., Part 1 (Calcutta).

CONTENTS.

THURSDAY, APRIL 11, 1889.

BRITISH UREDINEÆ AND USTILAGINEÆ.

A Monograph of the British Uredineæ and Ustilagineæ. With an Account of their Biology, including the Methods of observing the Germination of their Spores and of their Experimental Culture. By Charles B. Plowright, F.L.S., M.R.C.S. Illustrated with Woodcuts and Eight Plates. (London: Kegan Paul, Trench, and Co., 1889.)

MR. PLOWRIGHT'S monograph will at once take its rank as the chief English authority on the interesting groups of parasitic Fungi to which it relates. The results of the author's important original investigations are here incorporated with those of other observers to form a work which brings our knowledge of these plants thoroughly up to date. The greater part of the book is systematic, the detailed description of species being preceded by a general account of the structure and life-history of the two orders. These introductory chapters will probably appeal most to the reader who is not a specialist in mycology, though the biological notes attached to the specific descriptions are also of great interest, and effectually relieve the dryness which is usually inseparable from purely taxonomic work.

In the first chapter a short summary of the biology of the Uredineæ is given. This is, perhaps, if anything, rather too short, but of course some previous knowledge in the reader is assumed. The Uredineæ, which are now probably the best known family of pleomorphic Fungi, are arranged by the author, in agreement with Schröter and Winter, in biological groups, according to the varying combinations in which their different forms of reproductive organs are present. These groups are made use of for the division of the larger genera into sub-genera. The system adopted may be illustrated by the classification of the species in the largest genus, Puccinia, in which all the modifications are represented. The sub-genera are six in number. First we have Eupuccinia, characterized by the presence of all the four reproductive structures: spermogonia, æcidiospores, uredospores, and teleutospores. This sub-genus is again divided into Anteupuccinia, which is autœcious, all the forms occurring on the same host-plant, and Heteropuccinia, in which heterœcism prevails, the spermogonia and æcidiospores being developed on one host, and the uredospores and teleutospores on another plant belonging to a distinct genus. The other sub-genera of Puccinia are all autœcious. In Brachypuccinia the æcidiospores are absent, while in Hemipuccinia the spermogonia also disappear, only uredospores and teleutospores being developed. In Pucciniopsis only æcidiospores and teleutospores are usually present, while in both the remaining sub-genera, Micropuccinia and Leptopuccinia, the teleutospores alone occur, the two groups differing from one another in the fact that the teleutospores of Micropuccinia are of the usual type, requiring a period of rest before germination, while those of Leptopuccinia germinate on the host-plant as soon as they are ripe. The essential character of the teleutospores is the production of a small promycelium which immediately bears promycelial spores (sporidia), the latter being the bodies which directly infect the host. Thus the spores of Endophyllum are classed as teleutospores, on account of their method of germination, though in all other respects they agree with the æcidiospores of other genera. The author regards the teleutospores as the constant characteristic of the Uredineæ, believing that their apparent absence in many forms is due to our imperfect knowledge of their life-history.

The second chapter is devoted to the mycelium of the Uredineæ. Here the description of the cell-contents strikes us as rather inadequate, but the subject is a difficult one to investigate. The often remarkable influence of the localized mycelium on the tissue of the host is well described.

In the next chapter those puzzling organs, the spermogonia, are described, and their functions discussed. The author and others have observed that in sugar-solution the spermatia pullulate, like yeast-cells. It was not, however, found possible to infect the host-plant with them. Still the author inclines to the view that the so-called spermatia are conidia rather than fertilizing bodies, a view which the observations of Möller on the spermatia of Lichens certainly render probable. If Mr. Massee's isolated observation of an antheridium in the Æcidium form of *Uromyces Poæ* should be confirmed, we shall have to seek the male organs of the Uredineæ elsewhere than in their spermogonia.

In the chapter on the æcidiospores there is a want of clearness in describing their development. The account given recalls the now exploded Schleidenian theory of "free cell-formation," whereas the æcidiospores are in fact formed by ordinary cell-division.

The uredospores and teleutospores are described in the two following chapters, which call for no special remark. Most of the facts brought forward will be familiar to those who know de Bary's work on the Fungi.

Chapter VII. deals with the interesting phenomenon of heterœcism, and the curious history of its discovery in the case of the mildew of wheat is well told. The old observations, dating back to the middle of the last century, on the influence of the barberry in producing this disease in corn, are described, and in an appendix the text of the "Barberry Law of Massachusetts" (published in 1755) is given. This law enacted that, "Whereas it has been found by experience that the Blasting of Wheat and other English Grain is often occasioned by Barberry Bushes," these bushes should be extirpated throughout the province. The true explanation of this mysterious power for evil of the barberry was first given by Sir Joseph Banks in 1805, and shortly afterwards a Danish schoolmaster, Schoeler, made the first successful experiments in infecting wheat with the barberry fungus. Naturally the purely systematic botanists held out for a long time against the popular belief; and it was not until 1865 that the connection between the Æcidium of the barberry and the Uredo and Puccinia of the wheat was finally established by de Bary, who thus at last brought the views of scientific men into harmony with those which had long been held by practical farmers. Forty-seven heterœcious species of Uredineæ are now known, and the life-history of eleven of these was first worked out by the author.

The remaining chapters of the introduction deal with

the Ustilagineæ. The two groups have little in common, and it must not be supposed that their association in this work implies any near relationship between them. The bond of union is rather to be found in their common biological character as more or less injurious parasites.

In Chapter VIII., the mycelium of the Ustilagineæ is described, and attention is specially called to the fact that it usually spreads throughout the tissues of the host-plant, thus differing from the localized mycelium of most Uredineæ.

The next chapter treats of the development of the "teleutospores" (resting-spores) of the Ustilagineæ. The singular processes by which the "spore-balls" arise in such genera as Soroporium, Tubercinia, and Urocystis are clearly described in accordance with the researches of von Waldheim, Woronin, de Bary, and others. The facts are not, as a rule, new, but they are well put together. The germination of the teleutospores is described in the following chapter. The extraordinary processes of conjugation, in which both the promycelium itself and the sporidia so often take part, are described; and due weight is given to Brefeld's important discovery that these cell-unions do not take place when the spore is allowed to germinate in a food-solution, instead of in pure water. There are many points here which invite more detailed notice, but this may be the better dispensed with, as most of the facts have already been brought before English readers, in the translation of de Bary's work and in some of Prof. Marshall Ward's papers.

The subject of Chapter XI., the infection of the host-plants by the Ustilagineæ, is of great practical interest. In the case of the bunt (*Tilletia tritici*), it is well known that the sporidia borne on the promycelium of the teleutospores infect the embryo of the germinating grain, the germ-tubes penetrating the cells of the leaf-sheath. But how smut (*Ustilago segetum*) infects wheat has long been a mystery. The spores ripen in the young flowers, and have disappeared long before the grain is mature. Attempts at infection of the grain or seedling are almost always unsuccessful; and, on the other hand, the protective dressings of the grain, which are so effectual against bunt, are of no avail as against the attacks of smut. The experiments of Jensen render it most probable that the plant is infected by the spores while flowering, and that either the ovum itself is entered by the mycelium, or that the spores remain dormant in the grain until its germination, and that then the parasite "grows with the growth, and strengthens with the strength" of the young plant. It would appear, then, that the only remedy against the attacks of the Fungus would lie in the destruction of all affected ears at the earliest stage when the disease is visible, and before the spores have ripened.

The last two chapters of the introduction are among the most interesting to specialists, but do not require any analysis here. They deal with the culture of spores, and with the artificial infection of plants. Under the latter head it may be noted that only the Uredineæ are taken into account. The directions given are those of a master of the subject, and cannot fail to be a most useful guide to those who intend to undertake such investigations for themselves.

The longest, and no doubt the most valuable, part of the work consists in the description of all British species of the two families; pp. 119–271 being devoted to the Uredineæ, and pp. 272–301 to the Ustilagineæ. Attention has already been called to the admirable biological notes by which the descriptions of all the more important species are accompanied. In the specific diagnoses, the æcidiospores (when present) are first described; then the uredospores, and, lastly the teleutospores. Next follows a complete list of synonyms, and then the host-plant or plants are enumerated, after which the biology of the species is discussed. The account of the Ustilagineæ is completed by the description of a few genera, such as Graphiola and Protomyces, of doubtful systematic position.

A glossary is appended to the work, and in some of the explanations of terms there is room for criticism. Thus, in defining *conidium* as "an asexual spore," it seems to be too easily assumed that the other spores are sexual. The word *endochrome* is no longer required, and might perhaps be allowed to become obsolete. A *germ-pore* is not an "opening," but a pit, and a *hypha* does not necessarily consist of "an elongated cell." The word *periblem* means the young cortex itself, not "that part of the root of the host-plant which lies beneath the cortex," while *sterigma* is certainly *not* "the same as basidium."

The eight plates contain a great number of good figures, many original, others taken from the works of de Bary Brefeld, Woronin, &c.

A list of the authors quoted is given, and the book is especially well provided with indices, which are three in number. The first, a very useful one, gives the names of all the host-plants of the Uredineæ and Ustilagineæ respectively. Then we have a general biological index; and, lastly, an index of species, including all the synonyms.

The book is exceedingly well got up, but we must protest against the vivid yellow of the cover, presumably intended to recall the colour of the uredospores! Perhaps the decorous brown of the teleutospores would have been more becoming if symbolical colouring was wanted.

D. H. S.

THOMAS ANDREWS.

The Scientific Papers of the late Thomas Andrews, M.D., F.R.S., Vice-President and Professor of Chemistry, Queen's College, Belfast. With a Memoir by P. G. Tait, M.A., Sec. R.S.E., and A. Crum Brown, M.D., F.R.S., Professors in the University of Edinburgh. (London: Macmillan and Co., 1889.)

WE have here in a compact form the biography and scientific works of a man who has left his mark on the science of his time.

Born at Belfast in 1813, Thomas Andrews after acquiring the rudiments of his education at two excellent public schools in his native town, went at the age of fifteen to the University of Glasgow, where he attended the classes of chemistry and natural philosophy for one or two sessions; and in the following years he continued his studies at Paris, Dublin, and Edinburgh, taking the degree of M.D. at Edinburgh in 1835. He immediately began to practice as a physician at Belfast, and also to teach chemistry as Professor in the Belfast College, which was a higher department of the Belfast Academical Insti-

tution. In 1845, the scheme for the creation of the Queen's Colleges was launched, and their Presidents and Vice-Presidents were appointed as a preparatory step to the building of the Colleges. Andrews at this time received his appointment as Vice-President, and he was the first Professor of Chemistry, both which offices he retained till the failure of his health in 1879. His whole life-time was thus—with the partial exception of his medical practice from 1836 to 1845—occupied in scientific teaching and investigation; and from the early age of fifteen, when he published in the *Philosophical Magazine* a paper on the action of a blowpipe flame on other flames, he never ceased to devote himself to original research.

He was never in a hurry to rush into print, but took care to be accurate and thorough in an investigation before announcing his results. Regularly day by day he was to be found at work for hours in his laboratory, patiently conducting with his own hands every detail of his elaborate observations.

The most important of his researches relate to heat of combination, the nature and properties of ozone, and the transition of such substances as carbonic acid from the gaseous to the liquid state. But besides these, we find in the present collection brief papers on a variety of subjects. One contains an account of experiments on the conducting power of flame for electricity, showing that the current from a single cell could be transmitted through a circuit part of which consisted of an alcohol flame. Another describes the attainment of a very high vacuum by a good ordinary air-pump aided by the introduction of carbonic acid into the receiver and the absorption of the last traces of this gas and of aqueous vapour by caustic potash and sulphuric acid. Another gives a comparison of the conducting powers of different gases for heat, as shown by their cooling action on a platinum wire kept incandescent by a current. In all these subjects he was early in the field, and obtained results much in advance of those obtained by his predecessors.

An instance of his careful criticism is afforded by one of the latest papers in the volume—a lecture on recent improvements in magneto-electric machines—in which he points out that Paccinotti's machine, if the inventor's original description of it can be relied on, makes its contacts (for collecting the currents from the ring) in the wrong places. The criticism is certainly justified by the figure and accompanying description which are reproduced from Paccinotti's paper; but from inquiries which we made at the Paris Electrical Exhibition, where the machine was on view, we believe the fault was in the description and not in the machine itself.

As regards ozone, Andrews appears to have been the first to establish the following points:—

(1) That the peculiar substance obtained by the action of the electric spark on oxygen is identical with that obtained in the electrolysis of water, and with that obtained in the slow oxidation of phosphorus.

(2) That it is not a compound body, but is oxygen in an altered or allotropic condition.

In subsequent experiments, with the assistance of Prof. Tait (who was at that time Professor of Mathematics in Belfast), he compared the amount of contraction produced by the partial conversion of oxygen into ozone with the amount of ozone thus obtained as tested by chemical action, and hence deduced the density of ozone.

His researches in various branches of the subject of heat of combination were spread over many years, and were, for the most part, conducted at a date when an accurate thermometer for measuring small differences of temperature could only be obtained by making it for oneself. The subsequent researches of Favre and Silbermann, wherever they differed to any large extent from his, have since been shown to be erroneous, and his results agree fairly well with the latest and best determinations yet obtained.

But his permanent fame will rest mainly on his discovery of the continuous transition which can be made from the gaseous to the liquid state of a substance, or from the liquid to the gaseous. The main result which he established is best set forth by the geometrical illustration employed by his colleague, Prof. James Thomson. Let the volume, pressure, and temperature of a given mass of carbonic acid be represented by the three rectangular co-ordinates of a surface; volume being represented by height, while the pressure and temperature co-ordinates are horizontal. The surface will resemble the side of a mountain which is precipitous in one part, but in another part furnishes a gradual ascent by which the summit can be reached. The ground-plan of the precipice is the curve of boiling-points, and the height of the precipice at each point of the curve represents the increase of volume in passing from the liquid state to the gaseous. As the temperature increases, the precipice diminishes in height, and finally runs off to nothing at a point whose horizontal co-ordinates are the "critical temperature" and "critical pressure." At higher temperatures there is no boiling-point, and in place of the precipice there is a gradual ascent, by means of which the precipice can be rounded and the summit attained. Starting from the ground below the precipice (that is, from the liquid state), the ground above the precipice (that is, the gaseous state) can thus be attained without any kind of discontinuity.

Cagniard de Latour and Drion, who preceded Andrews in this field of research, failed to obtain this result, because their method of experiment placed only one independent variable at their disposal. The substance was inclosed in a sealed tube, and there was no way of altering its pressure except by altering its temperature. Andrews used a screw plunger, which enabled him to increase and diminish the volume independently of the temperature. By making simultaneous measures of volume, pressure, and temperature for various values of the two independent variables, he was able to map out the surface, and Prof. J. Thomson constructed a wooden model of it from the data thus obtained.

In subsequent researches he investigated the effect of mixing various quantities of nitrogen with carbonic acid, and found that the critical point was largely shifted by such admixtures.

Shortly before his health broke down, he devised an ingenious apparatus for making successive additions of a known column of mercury to the pressure of a gas confined in a long tube, hoping in this way to be able to test departures from Boyle's law at very high pressures without employing a column of mercury of unwieldy length. In

the reference to this subject in the preface there appears to be an oversight. The effect of a certain cycle of operations to be performed with the apparatus is described and then we read :—

"A connected series of vessels of this kind will enable the experimenter to apply a measured pressure of an amount depending on the number of vessels."

Instead of a connected series of vessels, it is only necessary to repeat, time after time, with the one vessel, the cycle of operations which has been described; and this we understood to be Dr. Andrews's intention. The apparatus was not brought into actual use, nor even constructed; preliminary trials having shown that screw plungers working in mercury (which were an essential part of the design) could not be prevented from leaking.

The various papers in this volume, and especially the Presidential Address, show Dr. Andrews to have been not only an accurate and original worker, but a man of wide culture and refined literary taste. The editors have done their work carefully and well.

J. D. E.

MACH'S "HISTORY OF MECHANICS."

Die Mechanik in ihrer Entwickelung historisch-critisch dargestellt. (An Historical and Critical Sketch of the Development of the Principles of Mechanics.) By Dr. E. Mach, Professor of Physics in the University of Prague. Second Edition. (Leipzig: F. A. Brockhaus, 1889.)

THE first edition of this work, which forms Vol. LIX. of the "International Scientific Series," appeared in 1883. With the exception of a few short appendices and the correction of misprints, it is identical with the original edition; but we are glad to take the present opportunity of calling attention to a book which, while unpretentious in form, is one of exceptional value to students, and especially to teachers, of the subject with which it deals.

The book has not been translated into English, and we understand that the English publishers did not consider it sufficiently popular in form to be included in the English series. This is much to be regretted. The work is one which certainly ought to be translated, as it would be most helpful to a large class of students and teachers who are unable to read it in German.

In the course of rather fewer than 500 pages the author gives his readers a well-constructed outline of the development of the science of mechanics from Archimedes down to the present time, accompanied by well-reasoned criticisms and discussions of the significance and relative importance of the various steps which he chronicles. The first chapter is devoted to the development of the principles of statics, and we would specially direct attention to the masterly manner in which the author shows the fallacies underlying the attempts of some of the early philosophers to derive the principle of the parallelogram of forces, or an equivalent one, from *a priori* notions, without appeal to experiment. The proposition commonly known as the parallelogram of forces may either be proved by direct experiment or by deduction from some such experimental principle as Newton's second law of motion. The advantage of the latter method consists, as the author points out, in the fact that he nature and extent of the experimental evidence for Newton's second law, or its equivalent, cause it to carry with it a greater certainty of its accuracy than is possible for a direct experimental demonstration of the proposition. This is a point to which it is most important to call attention, for, although Thomson and Tait have long since cleared away from the better class of text-books, and from the minds of the higher class of students, the fog which had accumulated around this essentially simple proposition, much of our school teaching is still enshrouded by it.

The second chapter treats of the growth of the principles of dynamics, understanding this in the more restricted sense of what Thomson and Tait called kinetics. This is of great interest and value throughout, but there are one or two points to which we would direct special attention.

The deduction of the approximate time of swing of a simple pendulum vibrating in a small arc, from a rectilinear simple harmonic motion, is, or at any rate should be, well known to students who have had the advantage of instruction from a Professor at one of our Universities; but it is quite time that this very simple method of obtaining an important relation should take the place of the artificial and cumbrous methods which still disfigure some of the elementary text-books in common use. The criticism of Newton's exposition of the ideas of time, space, motion, and mass, is also worthy of careful study. These two chapters are of quite an elementary character, and may be read with advantage even by students whose mathematical acquirements are of the slenderest.

The third chapter treats of the further application of principles, and the deductive development of mechanics. It does not, like the first two chapters, appeal to the beginner, but will be most helpful to a student who has already made some progress in the subject.

The fifth chapter bears the heading, "The Formal Development of Mechanics." It contains an interesting discussion of isoperimetric problems, and a brief account of the analytical method of treatment introduced by Lagrange. It also contains a section mainly devoted to an account of the theological vagaries of some of the great mathematicians and natural philosophers. This section is not of very great interest or value, and may have been inserted merely to give a popular flavour to what is essentially a scientific book.

The volume concludes with a very brief chapter on the relations of mechanics to other branches of knowledge.

G. W. DE T.

OUR BOOK SHELF.

Das Klima des ausser-tropischen Südafrika, mit Berücksichtigung der geographischen und wirtschaftlichen Beziehungen nach klimatischen Provinzen dargestellt. Von Dr. Karl Dove. 160 pp. and 3 charts. (Göttingen: Vandenhoeck and Ruprecht, 1888.)

METEOROLOGISTS must welcome the reappearance of the name of Dove among the contributors to climatological knowledge, and the present work does no discredit to the name. It is an endeavour to give a conspectus of the climate of South Africa as a whole; and the author ekes out the actual meteorological results, which are somewhat scanty in parts, by evidence derived chiefly from the indigenous flora of the several districts, which

he has collected from the published records of various travellers, such as Livingstone, Serpa Pinto, Fritsch, and others.

He limits the area of his discussion by biological considerations, as he defines the extreme southern limit of tropical Africa to be that fixed by the cultivation of the date-palm and the existence of the tsetse fly.

We have said that the records of observations are somewhat scanty, and this remark will be justified when we point out that from many stations the results for two years, or even less, are printed. The figures, such as they are, have been, however, most conscientiously discussed.

The area is divided into four great districts, classified according to the period of occurrence of the rainy season, viz. (1) the winter rains, (2) the intermediate region of spring and autumn rains, (3) the heavy summer rains, (4) the West Coast. Under (1), as subdivisions, we have the South-West Province, the Western Karroo, and the Little Namaqua Land. Under (2), the South Coast, South Karroo, North Karroo, and the South-East Mountain Land. Under (3), the Table-land of the Upper Orange River, the North Transvaal, the Kalahari, and the Great Namaqua and Damara Land.

After treating of these several regions at considerable length, Dr. Dove proceeds to discuss the possible development of agriculture in the different districts. His panacea for the Kalahari and some other tracts, with pure sandy surface, in the northern part of the area, is to introduce the date-palm.

He concludes the work with a discussion of the rainfall and its distribution, with some remarks on the question of the alleged deterioration of the climate by the drying up of the country. This effect he considers, with Mr. Gamble, to be merely the outcome of reckless forest destruction.

He points out the brilliant results obtained, at comparatively small cost, by the construction of reservoirs, as at Beaufort and at Van Wyk's Vley. R. H. S.

Chambers's Encyclopædia. New Edition. Vol. III. (London and Edinburgh: W. and R. Chambers, 1889.).

It may be enough to say of the third volume of the new edition of "Chambers's Encyclopædia" that it falls in no respect below the high level maintained in the preceding volumes. The editor is working upon a well-conceived plan, and he has every reason to be satisfied with the manner in which individual subjects are dealt with by his contributors. Scientific subjects continue to receive the attention which properly belongs to them in such a work as this. Tbe treatment of coal, coral islands, and geology generally has been intrusted to Prof. James Geikie, and his articles are admirable examples of compact and lucid exposition. Mr. J. Arthur Thomson writes of caterpillars, cells, crabs, &c.; Dr. Leonard Dobbin, of chemistry; Dr. Alexander Buchan, of climate; Mr. R. T. Omond, of clouds; and Dr. R. A. Lundie, of colour-blindness. Mr. C. J. Woodward has an article on crystallography, and Dr. W. Peddie treats of dew and diffusion. Of the articles on Darwin and the Darwinian theory, the former is contributed by Mr. Grant Allen, the latter by Prof. Patrick Geddes. There are a good many geographical articles, among which we may especially note the article on China, by Prof. Legge; that on the Congo, by Sir Francis de Winton; and that on Constantinople, by Mr. Stanley Lane-Poole. So far as we have been able to test the various papers, we have found them carefully written and thoroughly trustworthy.

The Elementary Principles of Electric Lighting. By A. A. C. Swinton. Second Edition. (London: Lockwood.)

The author explains generally the different apparatus used in electric lighting, and the broad princples of their working, using the "water-works" theory of the electric current, but at the same time carefully explaining that this is only done for the sake of convenience. An unfortunate mistake has been made in the diagram of the continuous-current dynamo (p. 24), where the coils are shown as wound in a different *sense* on the two limbs of the field-magnet. The book is, however, a remarkably clear exposition of the subject, and at the same time a model of conciseness.

The Natural History and Epidemiology of Cholera. By Sir J. Fayrer. (London: Churchill, 1888.)

The above formed the subject of the annual oration delivered by Sir Joseph Fayrer before the Fellows of the Medical Society.

The author deals at length in a most interesting way with the history of the disease, and then proceeds to enlarge upon its geographical distribution, habits, conditions, and epidemic movement. The ætiology of cholera is then dealt with, together with a review of those general and special precautionary measures it is desirable to adopt.

Throughout, the essay is written in a clear and interesting manner, and from the vast experience of the author in the subject the oration will well repay a careful perusal. William Robert Smith.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Halo and Mock Suns.

This morning a very distinct halo accompanied by mock suns on either side was seen here. As the latter, owing to the altitude of the sun, were at a considerable distance outside the halo, I think the following details are worth putting on record. At 11h. 12m., Berne time, the sun's altitude being 48° 30′, the distance from the halo to the left-hand mock sun was 9° 15′. The parhelic circle was plainly visible, reaching from the sun slightly beyond the mock suns. Each of the latter consisted of a reddish patch shading off into white and then into blue on the side away from the sun. From the brightest red to the brightest blue was about 2°, and the measurement 9° 15′ was taken from half-way between these to the nearest point of the circle dividing the red from the blue of the halo. It is difficult or impossible to measure such faint objects with the sextant. So I held a pencil at *both* arms' length, and noted the length on the pencil corresponding to the desired angle. Holding the pencil with both hands gives it a very definite distance from the eye, provided the position of the body and the altitude of the object be not much altered. Paying attention to these points I measured the angle subtended at my eye by a certain length on the side of a house, both with the pencil and a sextant. The angle 9° 15′ was found thence by simple proportion. I think the error of this measurement can hardly exceed 30′. The halo of course was the common one of 22°. James C. McConnel.

Davos Platz, April 5.

On the Connection between Earth Currents and Changes in Solar Activity.

May it not be that, in the recent experiments of Mr. Hertz on the effect of ultra-violet light on electric discharge, we have an explanation of the relation existing between disturbances on the solar surface and disturbances in earth currents?

The evidence for such a connection is obtained from the Greenwich records.

If we make the not very violent assumption that two clouds differ in potential from each other and from the earth, it will be seen that the earth will act as a condenser, and underneath each cloud will be collected a charge of opposite sign.

With sunlight, Hertz failed to find any marked effect, prob-

ably on account of absorption in the cloud regions of our atmosphere, which, as Langley has shown, takes up with great avidity the violet and ultra-violet rays.

May it not be that in clouds we have conditions especially favourable to the production of the Hertz effect? If so, the discharge from one cloud to another would be accompanied by an earth current in the opposite direction, as in the theory proposed by Prof. Stokes, in which a decrease of resistance is produced by an increase of heat from the sun.

Hertz found his effect (*Wied. Ann.*, xxxi. p. 993) much more marked in a medium under diminished pressure.

Under 300 millimetres of mercury, he finds that the ultra-violet radiation will nearly *quadruple* the length of spark obtained without it, while under ordinary atmospheric pressure it would scarcely *double* it. But this is the very circumstance which is realized in the case of clouds.

There is also reason to think that solar outbursts are especially rich in these rays of short wave-length which are required to explain the phenomena. HENRY CREW.

Haverford College, U.S.A., March 22.

Hertz's Equations in the Field of a Rectilinear Vibrator.

RECURRING to Hertz's equations for the field of the rectilinear vibrator, it appears to me that, while his conclusions are sound as regards the forces at points very distant from the vibrator, they require modification for the rest of the field. In fact, the principles upon which the question is investigated require that the electromotive force in the direction of z should become evanescent close to the vibrator (the axis of z).

The general form of Π is either—

$$\frac{\mathrm{M}\sin\frac{\rho}{\lambda}}{\rho}\cdot\sin nt,\ \text{or}\ \frac{\mathrm{M}\cos\frac{\rho}{\lambda}}{\rho}\cdot\sin nt,$$

where λ large, and $\lambda n = \frac{1}{\mathrm{A}}$, or, of course, the sum of the two forms.

In assuming for points near the origin (say the middle point of the vibrator) the approximate expression—

$$\frac{\mathrm{M}}{\rho}\sin nt,$$

Hertz, in point of fact, takes the second of the above forms for Π, for this reduces to $\frac{\mathrm{M}}{\lambda}\cdot\sin nt$ when $\frac{\rho}{\lambda}$ is very small.

But this assumption makes both Π and Z infinitely great close to the vibrator. Whereas, by assuming the former of the two forms, or—

$$\frac{\mathrm{M}\sin\frac{\rho}{\lambda}}{\rho}\cdot\sin nt,$$

i.e. near the origin $\Pi = \frac{\mathrm{M}}{\lambda}\sin nt$, we get, as a general expression for Z—

$$\mathrm{Z} = \mathrm{M}\left\{\left(\frac{1}{\rho^3} - \frac{3z^2}{\rho^5} + \frac{z^2}{\lambda^2\rho^3} - \frac{1}{\lambda^2\rho}\right)\sin\frac{\rho}{\lambda} - \left(\frac{1}{\lambda\rho^2} - \frac{3z^2}{\lambda\rho^4}\right)\cos\frac{\rho}{\lambda}\right\}\sin nt,$$

and, as ρ is indefinitely diminished, this reduces to—

$$-\frac{2}{3}\frac{\mathrm{M}}{\lambda^3}\sin nt$$

as a limiting value.

For distant portions of the field, Hertz's results as to the laws and amplitudes of the forces electric and magnetic remain unaltered.

Of course, the whole investigation, with such a simple assumption as to the nature of the field, must be regarded as only approximate. For any given form of vibrator—as, for example, a straight wire connecting two spheres—the exact treatment will be very difficult. In the simplest conceivable case of a spherical metal sheet with an induced Q_n distribution left to itself, the analysis is intricate (see a paper by Prof. J. J. Thomson to the Mathematical Society of London, January 1884). H. W. WATSON.

Berkswell, March 29.

Early History of Lightning-Conductors.

CAN any of your readers refer me to the sources of some of the late Mr. Richard Anderson's information with regard to the early history of the lightning-conductor? (1) On p. 27 of the third edition (1885) of his book, "Lightning-Conductors," he states that Franklin, in the 1758 issue of "Poor Richard," gave directions for the erection of lightning-conductors. (2) On p. 25 he refers to Prof. Winthorp, of Boston, having, in 1755, defended the lightning-conductor against a parson who had attributed a Massachusetts earthquake to the innovation. I should be much obliged for any reference to a library where a copy of "Poor Richard" for 1758 could be found; or, again, for any information with regard to Winthorp's defence of the lightning-conductor.

Prof. Meidinger, of Karlsruhe, who is preparing a second edition of his "History of Lightning-Conductors," is extremely desirous of verifying these details of their early history, and I should be glad if any of your readers could supply me with information for him on these points. KARL PEARSON.

University College, April 9.

The Satellite of Procyon.

MR. J. M. BARR's suggestion (NATURE, March 28, p. 510), as to the use of photography to ascertain whether there is any close companion or satellite to Procyon, would be considered a very desirable one by astronomers, in order to set at rest the question whether a companion can actually be discovered near the assumed place of the hypothetical one, of which the elements were given by Dr. Auwers in 1861, from investigations of the irregularity in the proper motion of Procyon observed by Bessel in 1844, and by Mädler in 1851. The orbit was computed on the assumption of a circular motion in a plane perpendicular to the line of sight round a point about 1″·2 distant, having a period of about 40 years, the position angle for 1873 being about 90°, so that the present angle would be about 234°, or about 9° per annum.

I fear, however, serious instrumental difficulties would arise in obscuring such a brilliant object as Procyon in a large telescope by a screen, so as to get the impress on a plate of a probably faint companion at the extremely close distance of two to three seconds of arc.

This difficulty, no doubt, must have presented itself to the minds of the astronomers at the Lick Observatory, California, or they would have tried the sensitive plate with the 34-inch photo lens of the great refractor, instead of examining Procyon visually with the 36-inch glass, as was done by Mr. S. W. Burnham on the early morning of November 18 last, with the following record:—"*Procyon.*—Carefully examined with all powers up to 3300 on the 36-inch under favourable conditions. Large star single, and no near companion."

If this means that no companion was seen within 10″ or 12″ radius, it makes the matter very perplexing, as Otto Struve measured a supposed new companion in 1873 with the 15-inch refractor at Pulkowa—the mean of several measures for March 28 being P. angle 90°·24, and distance 12″·49, and for 1874 (April 10) P. angle 99°·6, and distance 11″·67. This companion was looked for at Washington with the 26-inch refractor on several occasions from November 1873 till January 1876, and by the three Clarks (father and two sons) with the McCormick 26½-inch refractor, then completed at Cambridgeport, Massachusetts, but Struve's companion could not be seen with either instrument, and I am not aware that it has since been seen by Struve himself with the new Russian 30-inch refractor. The Washington observers at that time, however, gave estimated places for three new companions, supposed to be seen by them as follows:—

No. 1. Position angle, about 10°, and distance about 6″.
,, 2. ,, ,, ,, 36° ,, ,, ,, 8″·8
,, 3. ,, ,, ,, 50° ,, ,, ,, 10″

These appear (if they have an existence at all) to have been missed with the 36-inch glass at the Lick Observatory, as above referred to.

It is a singular coincidence that the position-angles of the companion supposed to have been seen by Otto Struve in 1873 and 1874 agreed with the orbital places computed by Dr. Auwers, but its distance involved the assumption of an enormous mass to Procyon for the parallax assigned to the principal star. ISAAC W. WARD.

Belfast, April 1.

Factors of Numbers.

THE processes given by Mr. Busk at p. 413 of NATURE are an interesting step towards the *practical* solution of the difficult problem of finding the factors of any number. In this article the processes are put in an algebraic form, which both shows more clearly the nature of the processes, and brings out the conditions necessary for their *practical* success (*i.e.* with any moderate labour): it will appear that with high numbers the labour involved would be prohibitory except in[1] favourable cases.

Let N be the number to be resolved into factors. Rejecting even numbers as obviously divisible by 2, *odd numbers only* require to be considered. If two integers, A, B, can be found such that—

$$N = A^2 - B^2,$$

the problem is solved, the factors being $(A + B)$, $(A - B)$. There is one universal solution which includes primes, viz.

$$A + B = N, \quad A - B = 1; \quad A = \tfrac{1}{2}(N + 1), \quad B = \tfrac{1}{2}(N - 1).$$

The problem is to find other solutions, if any exist. Certain limits may be at once assigned to A, B, viz.

(1) A, B are *minima* together, viz.

$A = \sqrt{N}$, $B = 0$, when N is a perfect square.

$A = \sqrt{N + \alpha}$ (the integer next $> \sqrt{N}$), and $B = \sqrt{A^2 - N} = \alpha$, when N is not a perfect square.

(2) A, B are *maxima* together, viz. when they are successive integers. This gives the universal solution above.

This gives a very wide range, wider for B than for A, viz.

$$A \text{ from } \sqrt{N} \text{ to } \tfrac{1}{2}(N + 1), \quad B \text{ from } 0 \text{ to } \tfrac{1}{2}(N - 1).$$

The two processes of Mr. Busk, somewhat generalized, amount virtually to this. Try first if N be a perfect square: if so, the factors are $\sqrt{N}$, $\sqrt{N}$. Next, if N be not a perfect square, assume any trial integer value for either A or B (within above limits, of course). Then, if either $(A^2 - N)$, $(B^2 + N)$ be a perfect square, it is the other sought square B^2 or A^2, and the thing is done.

But, if not, let A be increased, or let B be decreased by some integer r, such that $(A + r)$, $B - r)$ lie within above limits; then, if either $\{(A + r)^2 - N\}$ or $\{B - r)^2 + N\}$ be a perfect square, it is the other sought square, viz. B^2 or A^2, and the thing is done.

To do this thoroughly, *i.e.* to make *certain of not missing* the right value of $(A + r)$ or $(B - r)$, it seems absolutely necessary to work systematically, *i.e.* either—

(i.) begin with the *minimum* value A = integer next $> \sqrt{N}$, and work upwards, or—

(ii.) begin with the *maximum* value $B = \tfrac{1}{2}(N - 1)$, and work downwards,

trying *all* integer values of r in succession, $r = 1, 2, 3$, &c., until a perfect square is reached, or until, finally, the maximum value of r is reached, given by—

(i.) $A + r = \tfrac{1}{2}(N + 1)$, which gives $B = \tfrac{1}{2}(N - 1)$,

(ii.) $B - r = 0$, which gives $A^2 = N$, which is by hypothesis not a perfect square,

which ends the process, and shows conclusively—if no perfect square be reached earlier—that N is a prime.

An important practical help in working either process is given by Mr. Busk, in a simple way of forming the successive quantities $\{(A + r)^2 - N\}$, $\{(B - r)^2 + N\}$ by the successive addition or subtraction of a series of simple "differences," thus—

(0) Write down the starting quantity, $(A^2 - N)$ or $(B^2 + N)$.

(1) Add $(2A + 1)$ or subtract $(2B - 1)$, giving results $\{(A + 1)^2 - N\}$ or $\{(B - 1)^2 + N\}$,

(2) Add $(2A + 3)$ or subtract $(2B - 3)$ *more*, giving results $\{(A + 2)^2 - N\}$ or $\{(B - 2)^2 + N\}$,

and so on; and as the rth step—

(r) Add $(2A + \overline{2r - 1})$ or subtract $(2B - \overline{2r - 1})$ *more*, giving results $\{(A + r)^2 - N\}$ or $\{(B - r)^2 + N\}$.

Nothing simpler could be wished than this as a process, especially as it is exactly suited to be done mechanically upon an arithmometer.

The labour liable to be involved in the work is a serious practical drawback. Both processes are rapid when r is small, and

[1] Mr. Busk's examples are favourable cases.

tedious when r is large. Process (i.) is most rapid when the factors are nearly equal, and process (ii.) when they are extremely unequal; but as these conditions cannot be recognized *a priori*, selection of either process is only guesswork. A *suitable* selection of the starting numbers A, B, *i.e.* by taking A higher than the minimum (and yet not too high), or by taking B lower than the maximum (and yet not too low), *may* of course immensely shorten the process; but such selection is at present pure[1] guesswork. In fact, if with such arbitrary starting values of A, B, a perfect square is not reached by the end of the process, *no conclusion can be drawn*, but the process must be tried again with values of A, B nearer to the really safe starting values. Both processes are most tedious of all for prime numbers, when the number of steps (r) required is—

$$\text{In (i.), } r = \tfrac{1}{2}(N + 1) - \sqrt{N + \alpha}; \text{ in (ii.), } r = \tfrac{1}{2}(N - 1),$$

a number so large as to be practically prohibitory for high numbers.

Some shortening process is much required. One such is proposed (on p. 414) for odd numbers, but (unless it has been misunderstood by the writer) it is certainly not so *in general*. It appears to amount to this:—

If N be not a perfect square, subtract it from the two next higher squares, thus forming $\{(A + 1)^2 - N\}$ and $\{A^2 - N\}$. If either of these be perfect squares, the question is solved by what precedes; but, if not, subtract them from any two successive higher squares of say $(C + 1)$, C, such that $(C - A)$ is an odd number, thus forming—

$$[(C + 1)^2 - \{(A + 1)^2 - N\}] \text{ and } [C^2 - (A^2 - N)],$$

and divide each of these by their difference, *i.e.* by $2(A - C)$.

If they be not evenly divisible, increase the number C by the even integers, 2, 4, 6, &c., successively, trying the divisions again at each step, until after say m steps, the two results

$$[(C + 2m + 1)^2 - \{(A + 1)^2 - N\}], \quad [(C + 2m)^2 - (A^2 - N)],$$

are both evenly divisible by their difference, *i.e.* by $2(A - \overline{C + 2m})$. To the quotients so formed add the original quantity $(A + 1)$ or A, as the case may be.

The two resulting quantities will be found (on reduction) to reduce alike to the simple form—

$$\tfrac{1}{2}\left(\frac{N}{P} + P\right), \text{ where } P = (A - \overline{C + 2m}) \text{ for shortness,}$$

and this turns out to be actually the larger of the two numbers whose square is sought, since its square exceeds N by a perfect square, for—

$$\left[\tfrac{1}{2}\left(\frac{N}{P} + P\right)\right]^2 - N = \left[\tfrac{1}{2}\left(\frac{N}{P} - P\right)\right]^2 \text{ a perfect square,}$$

and the two factors are now seen to be $\frac{N}{P}$ and P.

The process thus appears to be really a roundabout way of finding by repeated trial the smaller factor P or $(A - \overline{C + 2m})$. Direct trial division of N by the series of factors A, $(A - 2)$, $(A - 4)$, &c., would *probably be simpler*. In applying the author's process it seems essential—in order to *avoid missing the right value* of $(C + 2m)$—to start with the lowest value $C = 0$ or 1 (according as A is odd or even), and work steadily on until an even division by $(A - \overline{C + 2m})$ is reached, ending finally with the value $A - \overline{C + 2m} = 1$, which would show conclusively—if no even division be reached earlier—that N is a prime; but if the start be made with a higher value of C, and no even division be met with till the final step of $A - \overline{C + 2m} = 1$, then no[2] conclusion can be drawn, and a fresh start must be made with a lower value of C.

The process will be rapid when m is small—*i.e.* when the

[1] Mr. Busk's example of process (ii.) (NATURE, p. 415) is a good instance of a purely *lucky* success in starting with B = integer next $< \sqrt{N}$. Let the same be tried on N = 69, 93, 123, &c., and it will fail (such a start being, in fact, illegitimate). Mr. W. H. H. Hudson's statement (NATURE, p. 511), that process (ii.) is "not one of general application," failing, for instance, for N = 323171, is a mistake: it fails solely from starting with B too low. The values in process (ii.) are: A = 2250, $B - r = 2177$, and $N = 4427 \times 73$; this process will of course fail if started with a value of B < 2177. This is a good instance of a case very tedious by either process: in fact, the number of steps necessary (if worked without guesswork) will be found to be $r = 1681$ by process (i.) and 159.408 by process (ii.), which are practically prohibitory.

[2] Mr. Busk's example on p. 414 of NATURE is a good instance of this. Applying it to the number N = 73, the start is made with the value C = 6; the process ends really with showing that $73 = 37^2 - 36^2$, and does not (of itself) warrant the inference that 73 is a prime.

factors are nearly equal, and will be tedious when m is large—*i.e.* when the factors are very unequal; and most tedious of all when N is a prime, when the number of steps (m) required will be $m = \frac{1}{2}(A - 1)$ or $\frac{1}{2}(A - 2)$, according as A is odd or even, which is obviously a very high number for high numbers N.

A different "shortening process" is proposed (by Mr. Hudson) on p. 511 of NATURE, which amounts to this. When the two numbers $(A + r)$ and $\{(A + r)^2 - N\}$, or, again, when the two numbers $(B - r)$ and $\{(B - r)^2 + N\}$, have a common measure, that common measure is (as is easily seen) one of the factors of N; and, if it can be recognized, at once solves the question. Unfortunately, this will be in general of little *practical* help, except when one of the four numbers operated on is quite small, as otherwise it is not easy to recognize (quickly) the fact of there being a common measure.

ALLAN CUNNINGHAM,
Lieut.-Col. R.E.

Chatham, March 30.

THE GEOGRAPHICAL RESULTS OF MR. STANLEY'S EXPEDITION.

IT is evident from Mr. Stanley's stirring letters, which during the past week have cast all other topics into the shade, that pioneering in Africa is not yet at an end, and that that strange continent has not yielded up its last wonder to knowledge. The letters are suggestive of many things. Much could be said in admiration of the heroism and generalship displayed; much as to the difficulties encountered and the sufferings and losses sustained; much as to the route selected, and much as to the conduct of the party left at Yambuya. But in the first place this is hardly the proper place to speak of these aspects of the expedition, and in the second place it is only fair to wait for the full narrative before venturing upon criticism. No one who knows Mr. Stanley had ever any doubt of his success, or could ever believe that he would allow himself to die before accomplishing his work. It is clear that to anyone who has it in him to do heroic deeds there is still ample scope in Africa.

What we have to do with here are the geographical results of Mr. Stanley's expedition. And here again we are met by the fact that the expedition was not properly one of exploration; at least, this feature was only secondary to the main object of the expedition, the "relief" of Emin Pasha, himself a contributor to science of high rank. Again, even the communication to the Royal Geographical Society can only be regarded as a few preliminary notes on the additions made to our knowledge of one of the most interesting regions in Africa; for the full results, which cannot but be of high value and interest, we must wait for Mr. Stanley's full narrative, which will doubtless include the results obtained by the scientific members of his staff. As the region through which the expedition passed was previously entirely unknown, fresh additions to our knowledge were inevitable. As to the character of this region, it is evident that, so far as time and danger and difficulties are concerned, no worse route could have been chosen. It is now well known that the Committee and Mr. Stanley yielded to influences which ought not to have weighed with them, in view of the main purpose of the Expedition, and that Mr. Stanley's own preference would have been for the East Coast route. Had this route been selected, no doubt there might have been difficulties with the Masai; forests would have had to be traversed, deserts crossed, and swamps trudged through; but all these obstacles combined would have been trifling compared with the terrors of the Aruwimi jungles, and their suspicious and ferocious inhabitants. However, Science has nothing to complain of: the gain has been all on her side.

Mr. Stanley has passed through one of the great blanks of Central Africa. Much of it was untrodden even by the deadly foot of the Arab slaver. Dr. Junker just touched its northern fringe; he had reached the Nepoko River apparently in its upper course; but from about 3° N. to about 4° S., and between the Upper Congo on the west and the lakes on the east, we have virtually a great blank. It is the northern part of this blank which Mr. Stanley has enabled us to fill in; and when he comes home he will probably be able to tell us more than we yet know. In the particular region with which he was concerned we wanted to know the course of the Aruwimi and its tributaries; the character of the country and people through which it passes; the position and extent of the lake (Muta Nzigé) to the south of Albert Nyanza, and its relation either to the Nile or the Congo. Some of these problems Mr. Stanley has solved; others, no doubt, he will have solved by this time.

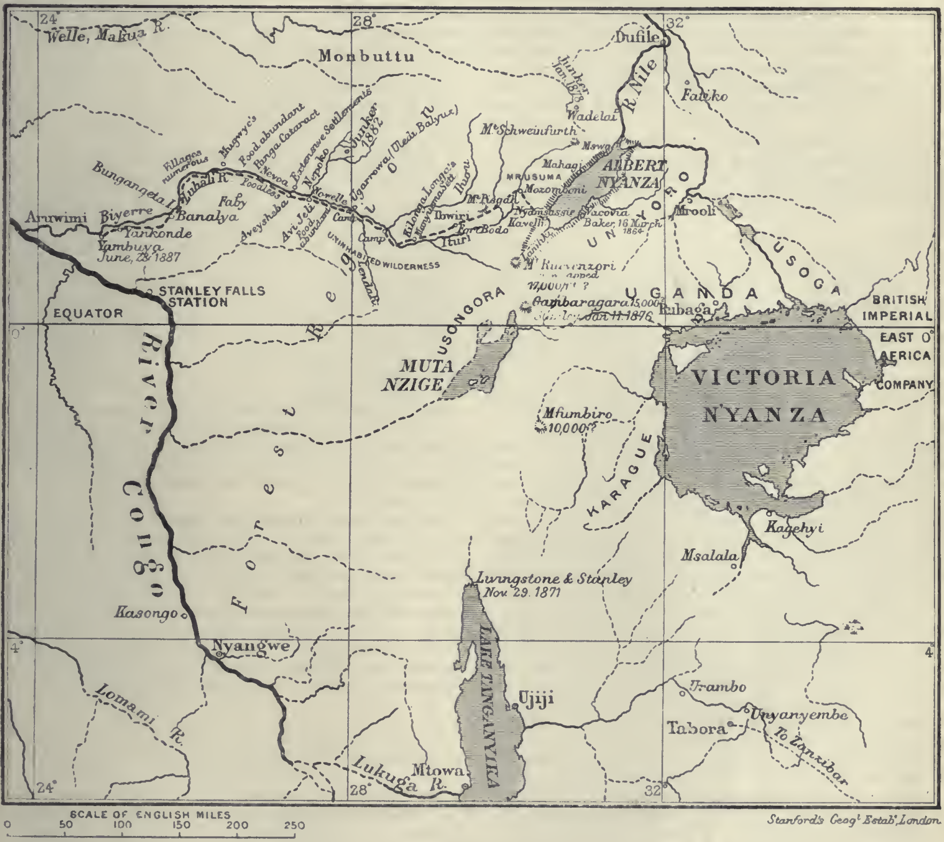
One thing is clear, the Expedition passed through the northern section of what is probably the greatest forest region in Africa, extending from about 3° N. to 4° S., and from about 23° to 30° E. Junker met with it on the Nepoko, and Livingstone in his weary journey from Tanganyika to Nyangwe. It was dense enough in both cases, but nothing apparently compared with what Stanley found it to be on the Aruwimi. The route, he tells us, was covered with creepers varying from $\frac{1}{4}$ of an inch to 15 inches in thickness, swinging across the path in bowlines or loops, sometimes matted and twisted together; also of a low, dense brush occupying the sites of old clearings which had to be carved through before a passage was possible. Where the clearings had been abandoned for some years was found a young forest, the spaces between the trees choked with climbing plants and vegetable creepers. This had to be tunnelled through before an inch of progress could be made. Mr. Stanley's description of the character and extent of this forest in his letter to Mr. Bruce is quite worth quoting:—

"Take a thick Scottish copse, dripping with rain; imagine this copse to be a mere undergrowth, nourished under the impenetrable shade of ancient trees, ranging from 100 to 180 feet high; briars and thorns abundant; lazy creeks meandering through the depths of the jungle, and sometimes a deep affluent of a great river. Imagine this forest and jungle in all stages of decay and growth—old trees falling, leaning perilously over, fallen prostrate; ants and insects of all kinds, sizes, and colours murmuring around, monkeys and chimpanzees above, queer noises of birds and animals, crashes in the jungle as troops of elephants rush away; dwarfs with poisoned arrows securely hidden behind some buttress or in some dark recess; strong brown-bodied aborigines with terribly sharp spears, standing poised, still as dead stumps; rain pattering down on you every other day in the year; an impure atmosphere, with its dread consequences, fever and dysentery; gloom throughout the day, and darkness almost palpable throughout the night; and then, if you will imagine such a forest extending the entire distance from Plymouth to Peterhead, you will have a fair idea of some of the inconveniences endured by us from June 28 to December 5, 1887, and from June 1, 1888, to the present date, to continue again from the present date till about December 10, 1888, when I hope then to say a last farewell to the Congo forest."

Here, then, we have a forest region very different from any other forest region of any extent in Africa. Prof. Drummond, in his recent book on Africa, describes very clearly the typical forest of Central and Southern Africa; the trees mostly standing apart, with very little brushwood, and in many places no difficulty in penetrating it even with a Cape cart. The rank exuberance of the Aruwimi forest can hardly be due to the abundance of water in the shape of lakes and rivers; for away south in the region recently traversed by Mr. Arnot, the region described by Livingstone as a great sponge, where the feeders of the Zambesi, the Congo, and other great rivers, take their rise, and on the east of which lie Tanganyika and Bangweolo

lakes, we find, so far as we know at present, no such dense bush, though the grass is high, and rank, and thick enough. Mr. Stanley attempts to account for the abundance of water and the thickness of the forest by the moisture carried over the continent from the wide Atlantic, by the winds which blow landwards through a great part of the year. But as a comparatively cold current sweeps along the coast from the south, these winds may be colder than the surface of the land over which they pass, and so may decline to part with their moisture. But this is a point for careful investigation; and it may after all be found that the rain of the rainiest region of Africa comes not from the Atlantic but the Indian Ocean, with its moisture-laden monsoons; and so we should have here a phenomenon analogous to that which prevails in the South American continent, the forests of which resemble in many features those of the region through which Mr. Stanley has passed.

The forest itself is not more interesting than its human denizens. Mr. Stanley mentions the names of many tribes living along the river, and judging from their names they seem all more or less of Bantu affinities. But we are here



verging on the limits of the Negro peoples, so that when we obtain full information it may be found that the Aruwimi tribes are much mixed. But it will be of the greatest interest to ascertain what has been the effect upon these peoples of their sad and depressing and ever-saturated surroundings; and to compare the results with what we find to be the case in more open country with people of the same type. That there have been changes in the population of the region is evident from the great heaps of oyster-shells met with by Mr. Stanley, some of them covered by several feet of earth. One important piece of information Mr. Stanley gives us concerning these forest tribes. Nejambi Rapids, about 250 miles above the junction of the Aruwimi and the Congo, marks the division between two different kinds of architecture and language. Below, the cone huts are to be found; above the rapids we have villages, long and straight, of detached square huts surrounded by tall logs, which form separate courts, and add materially to the strength of the village. Many precautions are adopted against attacks by poisoned arrows. Mr. Stanley lost several men by these arrows, and Lieutenant

Stairs had a narrow escape. It was afterwards found that the poison is manufactured from the dried bodies of red ants or pismires ground into powder, cooked in palm-oil, and smeared over the wooden points of the arrows. As might have been expected, the forest is haunted by myriads of insects of every variety, and it is to be hoped that a harvest of these have been gathered for the delight of the entomologists at home.

Mr. Stanley's description of the daily course of things in the forest region is worth quoting:—

"The mornings generally were stern and sombre, the sky covered with lowering and heavy clouds, at other times thick mist buried everything, clearing off about 9 a.m., sometimes not till 11 a.m. Nothing stirs then; insect life is still asleep, the forest is still as death, the dark river, darkened by lofty walls of thick forest and vegetation, is silent as a grave; our heart-throbs seem almost clamorous, and our inmost thoughts loud. If no rain follows this darkness, the sun appears from behind the cloudy masses, the mist disappears, life wakens up before its brilliancy. Butterflies scurry through the air, a solitary ibis croaks an alarm, a diver flies across the stream, the forest is full of a strange murmur, and somewhere up-river booms the alarum drum. The quick-sighted natives have seen us, voices vociferate challenges, there is a flash of spears, and hostile passions are aroused."

As to the river itself, the Aruwimi, or Ituri (it has several other names), is, with its windings, about 800 miles long, from its mouth in the Congo to its source almost on the edge of Albert Nyanza, though the course in a direct line is probably not more than 400 miles. The banks of the river, covered with forest from the Congo to the Nepoko (which is, after all, only a branch of the main river), are uniformly low, here and there rising to about 40 feet. Above the Nepoko, hills begin to crop up more frequently, palms are more numerous, and the woods show the tall, white-stemmed trees so characteristic of the slopes of the Lower Congo. While there are rapids at several places above Yambuya, above the Nepoko navigation becomes much more difficult, and rapids more frequent, while two considerable falls are met with. The land rises steadily until about 400 miles above Yambuya, the river is contracted into a rushing stream about 100 yards wide, banked by the steep walls of a cañon, the slopes and summits of which are clothed with wood. Whatever changes the face of the land may show, the forest covers peak, hill, ridge, valley, plain,—everywhere it is continuous, never broken, except at such clearings as man has made. Mr. Stanley very graphically compares the country traversed by his expedition to the long glacis of a fort rising from the Congo to a height of 5000 to 6000 feet; down the slope flows the Aruwimi, one of whose feeders runs almost within sight of Albert Nyanza, to which there is a sudden drop of 2900 feet.

"The main Ituri, at the distance of 680 miles from its mouth, is 125 yards wide, 9 feet deep, and has a current of 3 knots. It appears to run parallel with the Nyanza. Near that group of cones and hills, affectionately named Mount Schweinfurth, Mount Junker, and Mount Speke, I would place its highest source. Draw three or four respectable streams draining into it from the crest of plateau overlooking the Albert Nyanza, and two or three respectable streams flowing into it from north-westerly; let the main stream flow south-west to near N. lat. 1°; give it a bow-like form N. lat. 1° to N. lat. 1° 50′; then let it flow with curves and bends down to N. lat. 1° 17′ near Yambuya, and you have a sketch of the course of the Aruwimi or Ituri from the highest source down to its mouth, and the length of this Congo tributary will be 800 miles."

Here, then, we have remarkable hydrographical conditions. Only a few minutes' walk separates the feeders of the Congo and the Nile in this particular region. On the other side, again, are found streams flowing into the south of Victoria Nyanza rising close to others which run into Lake Tanganyika, which again, through the Lukuga, is believed to be a feeder of the Congo. Still further south are found the main Congo stream and its feeders rising in such close proximity to the source of the Zambesi that it is difficult to discriminate between the one and the other. Mr. Stanley's own lake, the Muta Nzige, of which he heard again when in the neighbourhood, very probably belongs not to the Nile but the Congo. All this is full of interest, and geographers will look with impatience for the publication of Mr. Stanley's detailed narrative.

Another fact of great interest Mr. Stanley refers to—the existence of a snowy mountain which may rival Kilimanjaro (19,000 feet), in the neighbourhood of Mount Gambaragara, or Gordon Bennett, between Albert Nyanza and Muta Nzige. This may be Mount Gordon Bennett itself, but Mr. Stanley does not think so, and he is supported by the few data which he furnishes. It would be quite in accordance with what we find in other parts of the world that a group of high peaks should be found together.

One other point of geographical interest is Mr. Stanley's observation that the Albert Nyanza is rapidly decreasing in size. A century or perhaps more ago, the lake must have been twelve or fifteen miles longer, and considerably broader opposite Mbakovia, than it is now. With the wearing away of the reefs obstructing the Nile below Wadelai, the lake has rapidly receded, and is still doing so, to the astonishment of Emin Pasha, who first saw Lake Albert seven or eight years ago. It is to be hoped that Mr. Stanley will find time further to investigate this subject, as well as to explore the country between the Albert Nyanza and Muta Nzige, settle the position and outline of the latter, and ascertain precisely to what river system it belongs.

The abruptness with which the forest comes to an end and the rich grass lands begin, about eighty miles from Albert Nyanza, is another point deserving special attention, and can only be explained when we have accurate observations of the rainfall and other conditions that go to form climate.

Such are some of the more important geographical results of Mr. Stanley's expedition, so far as we can gather from his preliminary letters; others may be derived from the map which accompanies his papers. More will no doubt follow. It is to be hoped that the rumour of Emin's return is not true, or at least that if he is coming to Europe he has left his province in efficient hands. In the interests of science as well as of humanity, it is important that the province which Emin has held so long may not be allowed to relapse into barbarism.

J. S. K.

A NEW PERMIAN RHYNCHOCEPHALIAN REPTILE.[1]

AMONG the many publications which have recently startled the palæontological world, one of the most important is unquestionably Dr. Hermann Credner's description of *Palæohatteria*, a new Permian Rhynchocephalian from the Plauen beds near Dresden—beds which have supplied the same author with copious material of Stegocephalians, both in the perfect and larval stages, the subject of his well-known admirable monographs. Great interest attaches to the present discovery from a purely zoological point of view, owing to the close relationship of this, one of the earliest of Reptiles, to the existing New Zealand *Sphenodon* (or *Hatteria*), the anatomy of which was first made known some twenty years ago by Dr. Günther in his classical paper in the Philosophical Trans-

[1] H. Credner, "Die Stegocephalen und Saurier aus dem Rothliegenden des Plauenschen Grundes bei Dresden," vii. Theil. *Palæohatteria longicaudata* (*Zeitschr. Deutsch. Geol. Ges.*, 1888, pp. 487–557, Pl. xxiv.-xxvi.).

actions (vol. clvii., 1867). Since that time *Sphenodon* (of which very few specimens were then known, and which was even supposed to be nearly extinct) has been found in abundance on various small islands in the Bay of Plenty, and has come into the hands of many anatomists, to the great benefit of reptilian morphology. An investigation of the development of this type is, unfortunately, still a desideratum, which, when supplied, cannot fail to throw great light upon the phylogeny of the Reptilia.

On the ground of its osteological structure and of the absence of copulatory organs, *Sphenodon* was recognized by Dr. Günther as the type of a distinct order, the *Rhynchocephalia*, a view in which he has been followed by Prof. Cope, who even goes further, and, very correctly as we think, approximates them to the *Plesiosauria* and *Chelonia*. However, the authority which attaches to the views of Prof. Huxley, who demurred to the ordinal separation of the *Rhynchocephalia* from the *Lacertilia*, has deterred a great number of systematists from accepting the order, and among these we find Dr. Credner still terming *Hatteria* a Saurian. Now, if the Saurians are to include the Rhynchocephalians, it seems unnecessary to divide the Reptiles into orders at all; we may safely say that, as far as our present knowledge goes, the difference between the Rhynchocephalians and the Lacertilians is ten times greater than that between the former and the Plesiosaurians, or between Crocodilians and Dinosaurians, and many times more so than between *Lacertilia* and *Ophidia*. The two latter are accepted as orders by those who refuse that rank to the *Rhynchocephalia*, but they hardly deserve to be looked upon as more than sub-orders in a group to be termed, in virtue of the law of priority, the *Squamata*. The *Rhynchocephalia* must be regarded as the most generalized of all recent and, perhaps, of all known Reptiles; in many points they approach the Stegocephalous Batrachians, and it is possible that the common ancestors of the *Chelonia*, the *Plesiosauria*, and the *Lacertilia* would fall in this order.

The following is Dr. Credner's definition of the new genus *Palæohatteria*:—

Habitus, that of a long-tailed lizard, 16 to 18 inches long, with robust limbs.

Vertebral column consisting of about six cervical, twenty dorsal, three or four distinct sacral, and fifty to fifty-five caudal vertebræ. Vertebral centra, solid amphicœlous sheaths which constrict but do not interrupt the notochord. Neural arches united to the centra by suture. Dorsal vertebræ with long anterior, caudal vertebræ with long posterior articular processes; no transverse processes; spinous processes of dorsal vertebræ elevated, with rounded upper border, decreasing rapidly on the tail to small tubercles, more and more posterior in position, and finally entirely disappearing. Small wedge-shaped intercentra between all præ-caudal and the anterior caudal centra; from the sixth caudal vertebra they are modified into chevrons. All præ-sacral, sacral, and the seven anterior caudal vertebræ with ribs. Dorsal ribs long and curved; cervicals straight; last dorsals short and feebly curved; sacrals short and stout; caudals short, hooked. Proximal extremity of ribs expanded, not divided into capitulum and tuberculum.

Skull pointed and compressed; orbits large and circular, with sclerotic ring; nostrils small, anterior; latero-temporal fossæ comparatively small. Dentition acrodont, the teeth acute and conical, slightly curved backwards at the extremity; a thin coating of dentine, which on the inner side of the basal third shows slight grooves. Præ-maxillaries distinct, each with three or four slender somewhat more strongly-curved teeth. Maxillary extending high up, armed with sixteen to eighteen teeth, of which the sixth and seventh are enlarged. Nasals nearly as long as the frontals. A large lachrymal between præ-frontal and maxillary. Jugal bordering the orbit inferiorly, bifurcating posteriorly into an ascending and a horizontal branch; the former forms with the post-orbital and the post-frontal a vertical post-orbital bar, the latter a horizontal bar with the quadrate.[1] Squamosal curved, fan-shaped, in contact anteriorly with the post-orbital, posteriorly with the horizontal branch of the jugal and with the quadrate. Basisphenoid a trapezoid plate with short lateral processes, with two small perforations near its anterior extremity, which tapers to the pointed præ-sphenoid. Vomer with hatchet-shaped groups of small teeth. Palatines with a series of teeth, parallel to the maxillaries, on the outer border. Mandibular ramus slender, straight, without coronoid process, formed of articular, angular, supra-angular, dentary, and probably also opercular and splenial.

Pectoral arch consisting of a long styliform episternum [interclavicle], which expands anteriorly into a small rhombic plate; two elbow-bent clavicles, overlapping the inner side of the episternal expansion; two crescentic scapulæ, truncate at each end and strongly thickened on the posterior border; and two non-perforated roundish coracoids.

Pelvis consisting of three paired elements, viz. a short massive ilium, with crest-like upper expansion and two diverging lower processes; a triangular posteriorly produced ischium; and a transversely oval, plate-shaped pubis with obturator foramen.

Limbs strong and stout, the posterior a little longer than the anterior. Distal extremity of humerus much expanded, with ectepicondylar foramen. Carpus with eight or nine ossified elements; tarsus formed of calcaneum, astragalus, and five tarsalia; five metacarpals and five metatarsals; five digits to both limbs, the first with two, the second with three, the third with four, the fourth with five, and the fifth with three phalanges, of which the distal is a sharp curved claw.

Abdominal ribs probably present, and formed of numerous small filiform bones.

An *armour* of small oat-seed-shaped scales, forming posteriorly diverging series, restricted to the ventral region between both pairs of limbs.

Among the most salient features in the structure of the new Reptile, as compared with other Rhynchocephalians, are the pelvis and the tarsus. The crest-like expansion of the costal border of the ilium and the bifurcation of its acetabular extremity, are, to a certain extent, Crocodilian or Dinosaurian, whilst the pubis and ischium appear to us to bear the greatest resemblance to the same in Plesiosaurians. It is also in the latter group that we have to look for so primitive and Batrachian-like a tarsus; for the tarsus of *Palæohatteria* affords an exact repetition of that of the likewise Permo-Carboniferous Plesiosaurian *Stereosternum*, which, on the ground of its five distal tarsalia has been made the type of an order, *Proganosauria*, by Dr. Baur. The close similarity of the dentition of *Palæohatteria* and that of certain contemporary Stegocephalians, especially *Dendrerpeton* and *Hylonomus*, is highly suggestive of relationship, and we are not surprised to hear from Dr. Credner himself of his having at first felt uncertain as to the class to which the fossil should be referred. Although unquestionably related to *Sphenodon*, *Palæohatteria* has, we venture to think, yet hardly a claim to enter the *Hatteriidæ*, and it would have been better had the author established for it a new family. The archaic condition of the humerus with both ent- and ect-epicondylar foramens, the presence of uncinate processes to the ribs, the absence of a ventral armour, the presence of a coronoid process in the mandible, and the share taken by the maxillary in the formation of the border of the orbit, a character common to the Chelonians and Plesiosaurians and certain Lizards, but not found in the Stegocephalians, the Ichthyosaurians, the Crocodilians, nor, we believe, in the other Rhynchocephalians, are

[1] What Dr. Credner calls "quadrate" is in reality quadrate + quadrato-jugal.

sufficient grounds for regarding the *Hatteriidæ*, with the single genus *Sphenodon*, as a different family.

Dr. Credner's paper also contains, incidentally, information on *Proterosaurus*, the structure of which is still, in spite of Prof. Seeley's recent investigations, very imperfectly known. In an example preserved in Freiberg, the author has discovered the interclavicle and clavicles, the former element closely resembling the same in *Palæohatteria*, whilst the latter is distinguished by its plate-like proximal expansion, which bears special resemblance to the so-called lateral pectoral plates of certain Stegocephalians. It appears almost certain that *Proterosaurus* was a Rhynchocephalian, but in many respects more specialized than *Palæohatteria*, intercentra being present only between the anterior cervical vertebræ, and the tarsus containing only six elements—three in the proximal and three in the distal row. G. A. BOULENGER.

THE SPECTRUM OF THE RINGS OF SATURN.

AN interesting note on the spectrum of Saturn's rings was communicated to the Royal Society on February 7 by Mr. Norman Lockyer. It has long been known that the rings are considerably more luminous than the planet, and the photographs by the Brothers Henry show that this is truer for the blue light than the more visible rays. It is therefore possible that they shine partly by their own light, and since it is now universally acknowledged that they consist of small bodies in motion, their spectrum has an important bearing on the meteoric hypothesis. Mr. Lockyer suggested that the additional luminosity might be due to collisions, and in order to determine whether the collisions were of sufficient intensity to produce incandescent vapours or not, he asked one of his assistants, Mr. Porter, to obtain a photograph of the spectrum. This was done at the Astronomical Laboratory at South Kensington, with a spectroscope having two prisms of 60° attached to the eye-end of the 10-inch equatorial. The photograph was taken with an exposure of about two hours, and shows decided indications of bright lines. Mr. Lockyer says:—"It is altogether too early to announce this as an established fact, but I think it well to send this note, in order that other observers with more powerful optical appliances and a better climate than that of London may investigate the question."

It is therefore very desirable that further inquiry should be made, both by photographic and eye observations. The bright flutings of carbon at wave-lengths 517, 474, and 564 should receive particular attention, the flutings being easily obtained for comparison from the flame of a spirit-lamp or wax vesta. Brightnesses may possibly occur also in the positions of the magnesium flutings at λ 500 and 521, the lead fluting at λ 546, and the manganese fluting at λ 558, all of which may be conveniently obtained for comparison by volatilizing the chlorides of these substances in the flame of a spirit-lamp or Bunsen burner.

It may be expected that the brightenings will be very feeble, owing to the masking effects of the more abundant solar light, so that the photographic method will probably give the best results on account of its power of integration.

In the same note, Mr. Lockyer states that "other considerations point to the possibility that bright lines or bands may be found in the spectrum of Uranus." A. F.

ON THE SPEED OF THE ELECTRIC TRANSMISSION OF SIGNALS THROUGH SUBMARINE CABLES AND LAND WIRES.

ELECTRO-TELEGRAPHIC operations for the determination of differences of longitude are usually so arranged as to furnish determinations of the speed of transmission of the electric signals. Each of two stations which are telegraphically connected is provided with a clock, and usually with a chronograph also; thus the clock-times at either station may be registered at will on the chronographs at both stations. The difference between the times indicated by the two clocks at any moment is thus readily ascertained, and two values of it will be obtained, one with the current transmitted in one direction, the other with it transmitted in the opposite direction. The difference between these two values indicates the sum of the speeds in both directions; and half the difference is usually called the "retardation on the line," as it indicates the amount by which every signal, on arrival at its destination, is slow on the time of its inception.

This method of determining the velocity is very simple and very exact; it does not require a knowledge of the errors of the clocks, or even of their rates, for the rates cannot sensibly alter in the brief interval between the signals with reversed currents, which need never exceed a few minutes.

The operations of two officers of the Indian Survey, Lieut.-Colonel Campbell, R.E., and Major Heaviside, R.E., for determining the differences of longitude between Bombay, Suez, and Aden,[1] furnish measures of the speed through two submarine cables which happen to be practically identical, though one cable was 355 knots, or as much as one-fourth, longer than the other cable. I gave the figures to Mr. W. H. Preece, of the Postal Telegraph Department, and he has found that they are very closely accordant with the theoretical speeds, calculated with due recognition of the different electrical conditions of the two cables. This is shown in the following table, in which Mr. Preece also gives the corresponding values by calculation and observation for the French Atlantic cable:—

Cable.	Length (knots).	Resistance (R) per knot.	Capacity (K) per knot.	KR per knot.	$L^2 \times KR$ per knot.	Thomson's constant, $a = {}^{\cdot}0232 \times KR$.	Apparatus time const. $a \times 1{}^{\cdot}55$.	Observed speed.
						sec.	sec.	sec.
Suez-Aden ...	1464	10·26	·358	3·67	7865162	·1809	·280	·280
Aden-Bombay ...	1819	6·60	·361	2·38	7874851	·1811	·281	·284
French Atlantic..	2584	2·93	·43	1·26	8413090	·196	·303	·3

For the speed of electric transmission through land lines, German geodesists have constructed an empirical formula on the assumption that the speed $= xl + yl^2$, l being the length of the line, and x and y two constants to be determined by observation. It is shown in the *Astronomische-geodätische Arbeiten* for the years 1883–84 and 1885–86, that, expressing l in kilometres,

$$\text{the speed} = 0{\cdot}0000208\text{s.}\ l + 0{\cdot}0000000206\text{s.}\ l^2$$

on the evidence of seventeen longitudinal arcs; and that on employing this formula to calculate the corresponding speeds in six arcs subsequently measured, the values obtained were found to differ 38 per cent. on the average from the observed speeds, and were generally quicker. This formula, however, takes no account of any differences in the electrical conditions of the lines. It gives 0·206s. and 0·302s. as the speeds of transmission through land lines 2700 and 3360 kilometres long, the lengths of the cables Suez-Aden and Aden-Bombay. The formula, however, cannot be legitimately applied to such long lines, for the longest of the seventeen on which the speeds were determined by observation, that from Berlin to Paris, was only 1230 kilometres. J. T. WALKER.

13 Cromwell Road, London, April 3.

[1] See vol. ix. of the "Account of the Operations of the Great Trigonometrical Survey of India." (Dehra Dun, 1883.)

NOTES.

THE Bakerian Lecture will be delivered to-day before the Royal Society by Profs. Rücker, F.R.S., and Thorpe, F.R.S. The subject is the Magnetic Survey of the British Isles for the epoch January 1, 1886, on which these gentlemen have been engaged for five years. They have made observations at more than two hundred stations, and have thus completed the first survey of the United Kingdom in which all three elements—the declination, dip, and horizontal force—have been determined for all parts. In addition to the general survey by means of which the directions of the isogonals, isoclinals, and lines of equal horizontal force have been found, special surveys have been made of selected districts in order to investigate the magnitude, direction, and causes of local magnetic disturbance. The principles which are justified by these inquiries have been applied to the whole country, with results which are likely to prove of interest both to physicists and geologists. We hope to give an account of the lecture in an early number.

PROF. MENDELEEF, the celebrated Russian chemist, has accepted the invitation of the Council of the Chemical Society to deliver the Faraday Lecture; he will probably give his mature views on the Periodic Law of the Elements, with which his name is so indissolubly connected. The lecture will be delivered by permission of the Managers of the Royal Institution in their theatre, on the evening of June 4. The Fellows of the Society will entertain Prof. Mendeleef at a dinner at the Holborn Restaurant the next evening; and the President, Dr. W. J. Russell, F.R.S., will hold a reception at the Grosvenor Gallery on June 7.

M. CHEVREUL, the famous French chemist, died on the morning of April 9 at the age of 102. He was born at Angers in 1786. During his long life he held many official appointments, and by his work as a chemist he secured an eminent place among the men of science of the present century. He constantly kept in view the possible applications of science to industry. Some of his discoveries have exercised an important influence on the manufacture of silk, and his researches relating to "fatty bodies of animal origin" marked an era in the development of various industries dependent on organic chemistry. M. Chevreul was a man of active and cheerful temperament, and extreme old age did not prevent him from continuing the studies in which he had found a perennial source of interest.

THE death is announced of the Finnish botanist, Prof. Sextus Otto Lindberg; and of Dr. Hermann Theodor Geyler, Director of the Botanical Gardens at Frankfurt. Dr. Geyler was born at Schwarzbach, in Saxe-Weimar, on January 15, 1835.

IN the House of Commons, on Tuesday, in connection with the vote for expenses incurred in the erection and maintenance of the buildings of the Department of Science and Art, Mr. Acland called attention to the inadequacy of the buildings used by the Normal School of Science. Mr. Acland's appeal, which was in no way exaggerated, received the cordial support of Sir H. Roscoe, who spoke of the condition of the buildings as "really a disgrace." Hon. members, he said, were hoping before long to have a technical Bill; but what would be the good of it unless proper teachers were provided? And proper teachers it was impossible to obtain unless their schools were what they ought to be. In almost every centre of population in foreign countries these normal schools were to be found, but there was only one in England. This had been built for one purpose, and converted to quite different purposes for which it was unfitted. The honour of the country was at stake, and this state of things had been borne quite long enough. Mr. Plunket, in his reply, declined to give any definite pledge about the matter. He had visited the buildings several times, and as one of the results of the debate of last year, the First Lord of the Treasury, the Chancellor of the Exchequer, the Vice-President of the Council, himself, and others made a most careful visit to every part of the building. One of the conclusions arrtved at was, that before additional buildings were erected it would be a good thing to get rid of a good deal of the material filling up the galleries at the present time. A Committee, consisting, among others, of Sir H. Roscoe, Lord Rayleigh, Sir B. Samuelson, and Lord F. Hervey, had been appointed for the purpose of weeding out those galleries in the science collections alone. Until that Committee had reported, it was impossible to take any further steps or to come to any definite decision with reference to increased expenditure to extend the buildings which at present existed.

A DEPUTATION from the National Association for the Promotion of Technical Education waited on Sir William Hart Dyke on Monday afternoon in the Conference Room at the House of Commons. In answer to the deputation, which was introduced by Lord Hartington, as President of the Association, Sir William Hart Dyke said the Government had the cause of technical education very much at heart, and would do their utmost to pass a Bill dealing with the subject during the present session. They would take into careful consideration the recommendations of the Association. He promised, meanwhile, that he would lay on the table of the House of Commons a memorandum explaining some of the difficult points of the Code in special reference to the instructions to be given to inspectors in carrying out the new provisions of the Code. He also promised that the subjects in which instruction might be given in evening schools should be increased from two to four.

AT the thirtieth session of the Institution of Naval Architects, yesterday, an important paper on the designs for the new battle-ships was read by Mr. W. H. White, F.R.S., Assistant Controller of the Navy and Director of Naval Construction.

THE Zoological Museum at Leyden, one of the most considerable on the Continent, has narrowly escaped a terrible disaster. On Monday, the 1st of this month, a fire broke out, and all the resources of the officials and of the town were taxed to extinguish it. Indeed, it was not got under until a considerable portion of the collection of specimens of hollow-horned Ruminants had been destroyed. Had the accident, which arose from the defect of a flue, taken place at night instead of in the afternoon, when plenty of assistance was promptly at hand, it is believed that the whole Museum would have perished. The authorities of other Museums, especially those which contain many spirit preparations, should not neglect this warning.

WE have already mentioned that an international meeting of zoologists will be held in Paris in August. The President will be M. Milne-Edwards, and some important questions will be submitted for consideration. Among them will be the question of the unification of the language of zoology in classification and specific denotation. M. R. Blanchard has prepared an important report on the subject, which will be published shortly in the *Revue Scientifique*, and form the basis for the discussions at the Congress.

THE Physiological Congress which is to be held in Basle in September will be attended by many French physiologists, if all those who propose to go are able to carry out their intention.

THE King of Sweden has selected Herr Ehrenheim, ex-Minister of State, as President of the forthcoming Oriental Congress in Stockholm. It is announced that Prof. R. L.

Beasley will represent the University of Cambridge; Oxford has not yet chosen its representative. The Shah and several Indian princes will send representatives.

THE Central Society of Agriculture of France has conferred upon Prof. J. C. Ewart the title of honorary member in recognition of the services he has rendered to the fishery industry by his "many and eminent labours."

THE *Kew Bulletin* for April 1888 contained a list of new garden plants described to the end of the year 1887. The *Bulletin* for the present month consists of a list of those described and published during the year 1888. The list has been extended to include the descriptions of new plants (and name alterations) which have appeared in several horticultural periodicals, that were not included in the former list. The number of new garden plants annually described in various English and foreign periodicals renders it a matter of considerable difficulty to botanists and horticulturists to keep them in view. The publication of a complete annual list of new garden plants is, therefore, as the editor explains, indispensable to the maintenance of a correct nomenclature, especially in the smaller botanical establishments in correspondence with Kew, as these, for the most part, are only scantily provided with horticultural periodicals. The editor also points out that such a list will afford information respecting new plants under cultivation at Kew, many of which will be distributed in the regular course of exchange.

A SEVERE shock of earthquake was noticed over the whole of South-East Japan on February 18. At Yokohama and at Tokio many houses were damaged. The shock continued for 6m. 12s., and was felt at Sendai, a town 45 geographical miles to the north of Tokio.

ON March 20, about 10 p.m., a slight shock of earthquake was felt near Carlstad, in Sweden. It was followed by a more severe shock at 3 a.m.

SEVERAL meteors have been observed recently in Scandinavia. On the evening of March 14, a brilliant meteor was seen at Molde, in North-West Norway, bluish-white in colour, and going in an easterly direction; and on March 21, at about 10 p.m., another was seen at Sarpsborg, on the Christiania fjord. It radiated in the west, and went in a southerly direction, displaying, during its passage, rainbow colours, and leaving a reddish trail behind. What may have been the same meteor was seen off the coast of Gothenburg, at 10.22 p.m., a considerable distance further south. It was very brilliant, and bluish-green in colour, and its passage—in a direction south-west-north—occupied fifty seconds.

THE atomic weight of chromium has been redetermined by Mr. Rawson, of University College, Liverpool. Previous determinations, of which there have been many, have resulted in placing the value somewhere between 52·0 and 52·5. The method employed by Mr. Rawson appears to have been an exceptionally accurate one, there being no transference from one vessel to another, no filtrations and consequently no burning of filter papers. The plan of operations consisted in first reducing a known weight of pure ammonium dichromate with alcohol and hydrochloric acid to chromic chloride, and subsequent estimation of the oxide produced by direct precipitation with ammonia. The purest obtainable ammonium dichromate, $(NH_4)_2Cr_2O_7$, was repeatedly recrystallized, dried for a couple of days at 100° C., and finally for a fortnight in a desiccator. The water used in the estimations was trebly distilled, and, on evaporation of 100 cubic centimetres, no residue was obtained. The purest commercial hydrochloric acid was redistilled, and the distillate in like manner gave no residue on evaporation. Similarly, the ammonia solution and the absolute alcohol were redistilled, until absolutely free from impurities. The method of vibrations was employed in weighing, and the platinum dish used in the operations was counterpoised by a similar dish of nearly equal weight, the slight difference being made up by a piece of platinum-foil. The two dishes were always treated alike: when one was placed on the water-bath, the other was also placed on a water-bath, and for the same time; they were both ignited for the same length of time, and, after ignition, were simultaneously placed in desiccators. In this way the usual errors in weighing platinum after such treatment were eliminated. During the experiments, the dishes were protected by covers of platinum-foil, which were not, however, weighed with them. In the actual experiments, the finely ground ammonium dichromate was weighed in one of the dishes, 10 c.c. of water added, and when the salt had dissolved 10 c.c. of hydrochloric acid. In small quantities at a time, 10 c.c. of the alcohol were subsequently added, and the whole evaporated to complete dryness on the water-bath. After repetition of this treatment to insure complete reduction, the residue was dissolved in 10 c.c. of water and 2 c.c. of the ammonia added; after an interval another 10 c.c. of water and 3 c.c. of ammonia were added, and the whole evaporated again to complete dryness. The dish was then heated in an air-bath to 140° for five hours, and afterwards to redness for an hour in a muffle furnace. The residual oxide, after weighing till constant, was found to be perfectly pure, and of a fine green colour; on washing with water and evaporating the washings, no residue was obtained. In calculating the results, to which all the known corrections were applied, it was assumed that $O = 15·96$ and $N = 14·02$, both well-determined numbers. The mean of the values from six experiments, the maximum difference between which was only 0·120, gives for the atomic weight of chromium 52·061. Hence chromium appears likely to possess a whole-number atomic weight, and it cannot but be admitted as remarkable that so many of the later stœchiometrical investigations, conducted with all the modern experimental refinements, have yielded values approximating to true multiples of the atomic weight of hydrogen.

MR. W. E. BEALE writes to the *Times* from Folkington Manor, Polegate, Sussex, April 4:—"On this estate is to be seen a nest, which has evidently been built partly by a thrush and partly by a hedge-sparrow. The nest itself is of the ordinary size of the thrush's nest; but, instead of being lined with mud, it is lined with horsehair, wool, and moss. The birds seem to have been good friends during the laying of their eggs. On Monday last there were three sparrow's eggs in the nest, and five thrush's. But on visiting the nest to-day it was found that the sparrow's eggs had been destroyed. The birds appear to have quarrelled when it came to the question of which should sit on them, and the thrush asserted its rights, not, however, without a struggle on the part of the sparrow, one of the thrush's eggs being broken, one missing, and three being perfect."

ACCORDING to the *American Field*, wild boars have become very numerous in the deep recesses of the Shawangunk Mountains, that border Orange and Sullivan Counties, N.Y. They are the genuine Black Forest wild boars of Europe, the descendants of nine formidable and ferocious boars and sows which Mr. Otto Plock, of New York, imported some few years ago for the purpose of annihilating the snakes and vermin that infested his estate near the Shawangunk Mountains. After the boars had eaten up all the snakes and vermin in the inclosure, they longed for more, and dug under the wire fencing and escaped to the mountains, where they have since bred and multiplied. They are so ferocious that the most daring hunter is said to hesitate before attacking them. They have immense heads, huge tusks and shoulders, and lank hind-parts.

Mr. T. Workman writes to us from Belfast that, having fallen into a doze on the afternoon of Friday, March 29, he was awakened by the undoubted hum of a mosquito, and thought himself back in Singapore, where he was about fourteen months ago. His first impulse was to drive away the nuisance, but curiosity to know whether it really was what he thought made him forbear, and he was soon rewarded by its settling on his right eyebrow and inserting its stiletto, with the usual sharp result, both to him and to it. Mr. Workman incloses in his letter a sketch of the insect. A mosquito in March in the north-east of Ireland is certainly a rare phenomenon.

The first part of the Transactions of the meeting held in August 1888, in Paris, for the study of tuberculosis in man and animals, has just been published by M. Masson. It is a volume of 500 pages, and contains much interesting matter.

Messrs. Macmillan and Co. have issued the second part of vol. ii. of the new and thoroughly revised edition of "A Treatise on Chemistry," by Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. In this volume, the authors treat of metals and their compounds.

A valuable list of the Coleopterous fauna of the Liverpool district, by Mr. John W. Ellis, has been reprinted from the Proceedings of the Liverpool Biological Society. The list contains nearly 30 per cent. of the British beetles.

The second part of Prof. E. Strasburger's "Histologische Beiträge" (Jena, G. Fischer) has been published. In this part, which is illustrated with four lithographic plates, the author deals with the growth of vegetable cellular membranes.

Messrs. G. Bell and Sons are about to publish "Names and Synonyms of British Plants," by the Rev. G. Egerton-Warburton. This is a complete alphabetical list of known British plants, giving, under each, references to its description in Sowerby's "English Botany" (Syme Boswell), "The London Catalogue of Plants," and the "Floras" of Babington, Bentham, and Hooker. The correct pronunciation of the names is indicated by accents. A list of the most usual synonyms is appended.

We have received vol. ii., No. 2, of the Proceedings of the Tokio Physical Society. It is worthy of note that the report of the Society is printed in Japanese, in Roman letters, while the various papers are in English. Amongst the contributions in the present number are: effects of stress on magnetization of nickel, by H. Nagaoka; thermal conductivity of marble, by K. Yamagawa; an apparatus for purifying mercury, by H. Nagaoka.

The next Congress and Exhibition of the Sanitary Institute will be held in Worcester at the end of September. Arrangements are in progress, and will be published shortly.

At the Central Institution of the City and Guilds of London Institute, Mr. T. Bolas will deliver a course of six lectures on photography, on Wednesday evenings, at 7.30, beginning on May 8. Lectures I. and II. will deal with the use of artificial light in photography; lectures III. and IV. with photo-mechanical printing methods; and lectures V. and VI. with direct contact printing methods.

The London Geological Field Class under the direction of Prof. H. G. Seeley, F.R.S., will begin the summer excursions this year on May 4, and will continue them on Saturday afternoons thereafter till the end of June. Intending students should apply at once for tickets to the Honorary Secretary, Mr. Walter Lewinton, Lundy House, Willoughby Road, Hampstead, N.W.

The additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. Cox; a Sinaitic Ibex (*Capra sinaitica*) from Mount Sinai, presented by Sir James Anderson; four Black Swans (*Cygnus atratus*) from Australia, presented by Messrs. James and Alex. Brown; a Raven (*Corvus corax*), British, presented by Mr. G. F. Hastings; a Collared Fruit Bat (*Cynonycteris collaris*), a Side-striped Jackal (*Canis lateralis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

The Luminosity of Venus.—Venus being now favourably situated, it is a convenient time for making observations with reference to the question of the cause of the recorded so-called phosphorescence. If there be any extensive phosphorescence, as first suggested by Sir William Herschel, or if there be an atmosphere occasionally illuminated by electrical discharges similar to those which produce auroræ in our own atmosphere, or even if there be a meteoritic bombardment, the light observed may possibly give indications of a spectrum of bright lines or bands.

The Spectra of R Leonis and R Hydræ.—A *Wolsingham Observatory Circular* (No. 23, April 2, 1889), by Mr. Espin, states that: "The spectra of R Leonis and R Hydræ contain bright (hydrogen?) lines, first seen on February 25. Observations confirmed, through the kindness of Mr. Common, by Mr. Taylor, at Ealing, who sees two in R Leonis and one in R Hydræ." The spectra of these two important variables have hitherto been simply described as being of Group II (Lockyer), indicating, according to Mr. Lockyer, carbon-fluting radiation and metallic-fluting absorption. Mr. Espin's observations are of very great interest in connection with the meteoric theory as to the cause of variability in this class of stars. It will be remembered that the variability is ascribed to the effects of a cometic swarm revolving round a central one, the maximum occurring at periastron passage, when the revolving swarm passes through the outliers of the central one. It was predicted that, under these conditions, bright lines would make their appearance, and the prediction has now been verified in the most complete manner. Both the stars observed by Mr. Espin were near their maxima, that of R Leonis occurring on March 23, and that of R Hydræ on February 17 (*Ann. du Bur. des Long.*, 1889). It may also be remembered that bright lines have been seen in R Cygni and *o* Ceti when near their maxima. The meteoric theory is therefore greatly strengthened by these observations. The importance of making further observations of these stars, with special reference to the disappearance of the bright lines, is obvious.

The Sun-spot Minimum.—Prof. Tacchini has recently communicated to the *Lyncei* of Rome (vol. v. series 4*a*, March 3, 1889), a note by Prof. Riccò on the days on which the solar surface was entirely free from spots, during the years 1885–6–7–8. It appears from the tables given that in 1885 there were only six days on which no spots were visible, whilst there were fifty-one in 1886, ninety-eight in 1887, and 140 in 1888. The maximum number of blank days occurring in one month was in November 1886, there being no spots on twenty-six days of that month. There were twenty blank days in October 1886, eighteen in July, and sixteen in May of the same year. A second ta le is given, showing the total numbers of days on which no sp s were visible during the years 1872–88 inclusive, and also the greatest number of consecutive days on which there were no spots during the same years. From this it appears that the greatest number of blank days was 248 in 1878, the last minimum period, whilst in 1872, 1882, and 1884, spots were visible every day. It is suggested that the approaching minimum will occur in 1889–90. In 1879 there were thirty-nine consecutive days on which no spots were recorded, this being the maximum number in any one year; the greatest number since then was seventeen in 1888.

Discovery of a New Comet.—A faint comet was discovered by Mr. E. E. Barnard, Lick Observatory, on March 31. The position of the object was as follows:—

March 31, 17h. 19m. G.M.T., R.A. 5h. 20m. 50s.; N.P.D. 73° 53′ 0″.

It has since been observed at Copenhagen:—

April 4, 8h. 51m. G.M.T.; R.A = 5h. 17m. 56s.; N.P.D. = 74° 0′ 55″.

Observations of Variable Stars in 1888.—Mr. Sawyer publishes the results of his observations of variable stars made in 1888, in *Gould's Astronomical Journal*, No. 190. Amongst the principal of these were U Orionis (Gore's variable), observed at maximum 1888 December 26; Mira Ceti at maximum 1887 November 10; W. Cygni at minimum 1889 January 1, since when it has brightened fast. ρ Persei was considered as having been at minimum, 1888 November 7; ε Aurigæ about 1889 January 15. R Scuti seems to have been just three weeks in advance of the ephemeris in Nature; R Lyræ and U Monocerotis corresponded to the predicted times pretty closely. R Virginis also was estimated to be at maximum only one day later than given by the ephemeris; *g* Herculis was recorded as at maximum 1888 June 3 and September 11, and at minimum July 18.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *APRIL* 14–20.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 14

Sun rises, 5h. 7m.; souths, 12h. 0m. 11·5s.; sets, 18h. 53m.: right asc. on meridian, 1h. 31·6m.; decl. 9° 35′ N. Sidereal Time at Sunset, 8h. 25m.

Moon (Full on April 15, 22h.) rises, 17h. 3m.; souths, 23h. 20m.; sets, 5h. 21m.*: right asc. on meridian, 12h. 52·9m.; decl. 0° 14′ S.

Planet.	Rises. h. m.		Souths. h. m.		Sets. h. m.		Right asc. and declination on meridian. h. m.		° ′
Mercury..	4 58	...	11 20	...	17 42	...	0 51·6	...	3 31 N.
Venus.....	5 6	...	13 25	...	21 44	...	2 56·9	...	23 44 N.
Mars	5 40	...	13 6	...	20 32	...	2 37·8	...	15 31 N.
Jupiter....	1 9	...	5 5	...	9 1	...	18 35·2	...	22 55 S.
Saturn....	11 52	...	19 32	...	3 12*	...	9 5·0	...	17 55 N.
Uranus ...	18 14	...	23 41	...	5 8*	...	13 13·8	...	7 7 S.
Neptune..	6 39	...	14 24	...	22 9	...	3 55·4	...	18 43 N.

* Indicates that the setting is that of the following morning.

April.		h.		
14	...	14	...	Saturn stationary.
20	...	20	...	Jupiter in conjunction with and 0° 19′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.		Decl. ° ′			h. m.	
U Cephei	0 52·5	...	81 17 N.	...	Apr. 16,	3 14	*m*
Algol	3 1·0	...	40 32 N.	...	,, 17,	23 43	*m*
R Corvi	12 13·9	...	18 38 S.	...	,, 16,		*M*
Y Virginis	12 28·1	...	3 49 S.	...	,, 17,		*m*
δ Libræ	14 55·1	...	8 5 S.	...	,, 16,	1 8	*m*
U Coronæ	15 13·7	...	32 3 N.	...	,, 17,	23 55	*m*
S Coronæ	15 16·9	...	31 46 N.	...	,, 14,		*M*
U Ophiuchi... ...	17 10·9	...	1 20 N.	...	,, 15,	3 18	*m*
					,, 15,	23 26	*m*
β Lyræ...	18 46·0	...	33 14 N.	...	,, 15,	22 0	m_2
					,, 19,	3 30	*M*
R Lyræ	18 52·0	...	43 48 N.	...	,, 20,		*M*
χ Cygni	19 46·3	...	32 38 N.	...	,, 17,		*M*
S Sagittæ	19 51·0	...	16 20 N.	...	,, 17,	4 0	*m*
S Cygni	20 3·2	...	57 40 N.	...	,, 14,		*M*
X Cygni	20 39·0	...	35 11 N.	...	,, 15,	22 0	*m*
T Vulpeculæ ...	20 46·8	...	27 50 N.	...	,, 20,	9 0	*M*
δ Cephei	22 25·1	...	57 51 N.	...	,, 19,	1 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

Meteor-Showers.

	R.A. °		Decl. °		
Near ν Virginis	174	...	7 N.	...	Slow; bright.
,, β Serpentis	233	...	16 N.	...	Very swift.
,, π Herculis	256	...	37 N.	...	Swift.
The *Lyrids*	269	...	33 N.	...	April 19, 20. Swift.
	272	...	20 N.	...	Swift.
From Vulpeculæ	299	...	24 N.	...	April 19, 20. Swift.

GEOGRAPHICAL NOTES.

The Founder's Medal of the Royal Geographical Society has been awarded to Mr. A. D. Carey, of the India Civil Service, for his remarkable journey in Central Asia, at his own risk and expense, during which he travelled over a distance of 4750 miles, through regions which have never before been visited by an Englishman, and very rarely by any European. The Patron's Medal has been awarded to Dr. G. Radde, Director of the Natural History Museum, Tiflis, for a life devoted to the promotion of scientific geography, as a traveller, observer, and author. The Murchison Grant has been given to Mr. F. S. Arnot, towards providing and conveying a suitable present to the Chief Chitambo, of Ilala, as a recompense for his services in connection with the removal of the body and personal property of Dr. Livingstone in 1872. Mr. Arnot also receives the Cuthbert Peek Grant, in recognition of the interest and value of his seven years' travels in Central Africa. The Back Premium has been awarded to Mr. F. C. Selous, in acknowledgment of the geographical work accomplished by him in his recent journey in Mashona Land and north of the Zambesi; and the Gill Memorial to Mr. M. J. Ogle, of the Indian Survey Department, in recognition of his excellent survey work in Eastern Assam, in Manipur, and in Northern and Western Burma. The three new honorary corresponding members are Captain Dutton, of the U.S. Survey; M. Maunoir, secretary of the Paris Geographical Society; and Dr. Ballivian, a Bolivian geographer. Sir Mountstuart Grant Duff will succeed General Richard Strachey as President.

On Monday night Mr. Harold W. Topham read to the Royal Geographical Society an account of his visit last summer to the Glaciers of Alaska and Mount St. Elias. Speaking of Glacier Bay, near Sitka, Mr. Topham said that into it many large glaciers descend, only one of which, the Muir, has been explored. It is thirty miles long, and its breadth, where it runs out into the sea, is one mile. It is decreasing very rapidly, so rapidly, indeed, that the sailors assert that they can, year by year, distinguish the difference in its size. The height of the ice wall at the foot of the glacier, where it is washed by the sea, was 319 feet in 1886, whilst last year it was 266 feet, a decrease of 53 feet. A cairn which had been erected to ascertain the rate of motion of the glacier, was found almost overthrown into a crevasse. Mr. Topham speaks of the magnificence of the mountain range stretching from Cross Sound to Yakatat. The peaks reach up 16,000 feet, the whole of which height is at once presented to the view. Their bases are washed by the sea, into which their glaciers descend. Many of these glaciers are singularly free from moraine. They are exceedingly steep, and are broken up into innumerable ice falls. The St. Elias Alps, from Cross Sound to Fairweather, run close to the sea. They then curve inland, and sweep round Yakatat Bay at a distance of about thirty miles from the water. There are many fine peaks in the range, and the eye wanders on from summit to summit till it rests upon the finest of all, Mount St. Elias, at the far north-west of the range. Lieutenant Allen stated this peak to be 19,500 feet high. It is the highest mountain in North America. To the north and west of Yakatat Bay all is ice. It is a vast plain of ice, stretching back sixty miles or more, and running eighty miles along the coast. At a place fifty miles up the glacier from the bay, the party found they were only 650 feet above the sea. This gives a fall of only 13 feet to the mile. The rate of progress, therefore, of the ice, must be very small, and this is proved by the quantity of scrub and trees which grow upon the terminal moraine upon the top of the ice. The moraine is several miles broad, and is covered with brush of alder and willow and spruce. The party proceeded by boat from Yakatat to opposite Mount St. Elias. They proceed up the Yahtsétah River. Seven miles from the sea, the river issues from beneath the ice, and it brings with it such a quantity of dirt, that the water is of a greyish-white. The river, where it issues from the ice, is about 50 feet broad, but it divides and subdivides to such an extent that at its mouth it is about seven miles in width. The west bank is composed entirely of ice. Where the river issues from under the ice, the latter is 500 feet thick, and possesses a moraine several miles wide, the last mile of which, the one nearest the edge of the glacier, is covered with thick brush. Through this brush they had some difficulty in forcing their way to the open glacier beyond. The best way of describing the moraines upon the Malaspina Glacier is to liken the

surface of the ice to a very choppy sea, on to which has rained innumerable stones and rocks. The depressions are often 100 feet deep. On this moraine were found shale and slate granite quartz, with sulphates and pyrites and copper. After several ineffectual attempts Mr. Topham and his companions decided to ascend St. Elias on the south-west side, west of the Chaix Hills. The party ultimately, from their camp 1500 feet high, reached the brink of the crater, 7600 feet above sea-level and 5000 above the Tyndall glacier; another six hours found them at a height of 11,461 feet. They were then on the northern and upper rim of the crater, and judged the summit to be another seven or eight thousand feet above. The crater is full of ice, and upon its precipitous cliffs are a number of overhanging glaciers, splashed, as it were, upon the rocks and unattached from the snow-fields above. This is characteristic of a number of the glaciers in the neighbourhood. There they are —right on the rocks, with yawning crevices upon them broken up and ready to topple over upon you. Perhaps in a few years they will have melted entirely away. Everything, Mr. Topham states, around St. Elias bears evidence to the conclusion that the long period of ice through which the land has been passing is now coming to an end; a conclusion which is certainly rash. Mr. Topham gave a detailed description of the panorama to be seen from the highest point reached. There is, he states, vegetation upon the south-east slopes of the hills to a height of 1500 feet above the glacier. The greatest height at which he found vegetation, exclusive of lichens, was 4500 feet above the sea, but the place was exposed to the full glare of the sun, and no other vegetation was found for an interval of 1500 feet below. A characteristic of the Alaskan glaciers is the curious way in which small isolated bits of moraine show up here and there above the ice. For example, you may walk down the centre of the Tyndall upon white ice without seeing more than a few stones to suggest the existence of a moraine, and suddenly you will come upon an island of *débris*, disconnected from any regular moraine. It springs from nowhere, is quite isolated, and appears to have no reason for being there.

BIOLOGICAL NOTES.

THE RATTLE OF THE RATTLESNAKE.—The habit of sloughing is common to all serpents: a short time before the removal of the old skin takes place, the new epiderm makes its appearance beneath the old. The mode of growth of the new and the removal of the old is the same in all snakes, with the exception that, in those with a rattle, that portion of the slough that covers the tip of the tail is retained to form one of the rings of the rattle. The attachment is simply mechanical; the rings are merely the sloughs off the end of the tail. The terminal bone of the tail is formed of vertebræ that have coalesced and changed in great measure their shape; in the different species the number of vertebræ included in this bone varies considerably, and sometimes it varies in individuals of the same species. With the purpose of indicating the manner of growth of the rattle, and as far as possible determining its origin, Mr. S. Garman has followed up its appearance in several species, full details of which, with figures, have been lately published. In the very young rattlesnake, while the vertebræ are still separate, there is no rattle, but about a week after birth a well-marked button is seen; with the first slough the first ring is set free, the button being pushed forward, and a third button is gradually perfected. In time the traces of the vertebræ in the terminal bone are almost obliterated; the bone becomes thickened, pushed forward at its edges, and otherwise enlarged. In a full-grown rattlesnake the hinder seven of the rings belong to the period of the snake's most rapid growth—they form the "tapering rattle" formerly used in classification of the species; while four of the rings and the button are formed while the gain in size was less rapid, and form the "parallelogrammic rattle" of the old classifiers. Many serpents besides those possessed of a "crepitaculum" are addicted to making a rattling noise by vibrations of the end of their tails. In illustration of the extent to which the tail has been modified in different cases, Mr. Garman figures the tails of several species, among others that of *Ancistrodon contortrix*, Lin., the copperhead of the United States. The tip of its tail is directed downwards as well as a little backwards; most often the button has one or two swellings in a degree resembling those on a ring of the rattle. A living specimen of this snake, kept for a year or more, would take to rattling on the floor whenever it was irritated; the sound was made by the terminal inch of the tail, this part being swung from side to side in the segment of a circle, so that the tip might strike downward. The result was a tolerable imitation of the sound made by a small rattlesnake.—(*Bulletin Museum Comp. Anatomy*, vol. xiii. No. 10, August 1888.)

A NEW SPECIES OF LAMINARIA.—The discovery in the Mediterranean Sea, midway between Marseilles and Algiers, of a Laminaria, not only new to the shores of Europe, but an addition to the group—one, too, neither small in size nor obscure in its characteristics—is a very interesting fact for botanists. *Laminaria rodriguezii* has been described by Dr. Ed. Bornet in a recent number of the Proceedings of the Botanical Society of France. It was taken by M. J. Rodriguez a few miles south of Port Mahon, on a rocky bottom, in a depth of from 125 to 150 metres. It was also taken on the east and north coasts of Minorca. It appeared to be abundant in the first-mentioned of these localities. The fronds grow to a height of 2 metres. In general aspect, consistence, and colour this new species somewhat resembles *L. saccharina*, but it cannot be for a moment confounded with this well-known form. It is attached to the stones upon which it grows by a series of little root-like processes, which emanate from stolons running over the surfaces of the stones. From these stolons the young fronds arise, and in specimens with adult fronds, a whole colony of small fronds will be found springing from the stolons. *Lam. bongardiana* and *L. longipes* of Kamchatka, *L. japonica* from Japan, and *L. sinclairi* from California, are the only known species, with simple fronds, which possess these rooting stolons, but none of these can be confounded with the present new form. Of the five species of Laminariaceæ which have been from time to time recorded as occurring in the Mediterranean, this is the only one that is without any doubt a native. *Phyllaria reniformis* may possibly be indigenous, but *Ph. purpurascens*, *Lam. saccharina*, and *Sac. bulbosa* are almost certainly waifs that have been only met with in the neighbourhood of ports. The *Lam. saccharina*, Ardissone, found growing at Syracuse, in Sicily, proves, however, to be Bornet's new species, which is the sole representative on the Atlantic sea-board of the Pacific Ocean forms above referred to. —(*Bull. de la Soc. Bot. de France*, tome xxxv. pl. 5.)

THE ENVELOPES IN NOSTOCACEÆ.—M. Maurice Gomont has printed a brief abstract of his researches on the investing envelopes of the filamentous Nostocs. The thallus in these consists of the simple row of cells, the trichome, and the protective envelope, more or less marked (the gaine); when the hormogones are dispersed, this latter disappears. In a 33 to a 50 per cent. solution of chromic acid, the gaine becomes swollen and dissolves, leaving only a tube-like pellicle; next the protoplasm of the trichome cells becomes greatly changed, leaving the cell-walls clearly defined. These consist of an external layer, seemingly intermediate between the membrane met with in the hyphæ of Fungi and the cuticle of the higher plants; it has a remarkable power of resisting the action of acids: in a 33 per cent. solution of chromic acid or in concentrated sulphuric acid, it remains unchanged for a space of twenty-four hours; it is insoluble in hydrochloric or acetic acids, or in caustic potash; it is dissolved in a 50 per cent. solution of chromic acid, but only after several hours; with aniline or fuchsine it assumes a brighter hue than ordinary cuticle. The interior layer gives the reactions of cellulose. The chemcal properties of the gaine prove it to be a true cuticle.—(*Journal de Botanique* for 1888.)

THE SCOTTISH METEOROLOGICAL SOCIETY.

AT the half-yearly meeting of the Society, held on Monday, April 1, it was stated in the Report of the Council that new stations had recently been added in the Newington District of Edinburgh, and in the Botanic Garden, these additions to the observing staff being regarded with much satisfaction, particularly in view of the facilities which a somewhat thickly planted series of stations in Midlothian offer, in the observation of the physical data required in investigating the various meteorological gradients, as proposed by the late Mr. T. Stevenson. Dr. Archibald Geikie, Prof. Crum Brown, and Prof. Bayley Balfour were elected Members of Council.

The inspection of the fishery barometers of the Meteorological Council at fifty-four of the fishing ports on the Scottish coasts has now been completed by Mr. Dickson, who gave much

attention, by short lectures to the fishermen, conversations with them, and otherwise, to awaken an interest in weather forecasts and their intelligent interpretation. Though the giving of the lectures is practically limited to the Saturdays, when the fishermen are disengaged, yet opportunity was taken to deliver eleven lectures, which were attended by audiences varying from 40 to 250. The method of proceeding was to give, by the help of weather charts, a short explanation of the law of storms, and an account of the weather of the week immediately preceding the lecture. The fishermen were then invited to ask questions, and raise discussions on the subjects of lecture.

During the winter Prof. Balfour engaged Mr. Turnbull to give fourteen lectures to the *employées* of the Garden on meteorology, in which marked prominence was given to the practical side of the science, explaining and teaching them to handle each instrument—why it is placed in the position it occupies, and not elsewhere; and showing the methods of reducing the observations. The efficient training of a body of men from which the Council largely draws its observers is a matter of no small importance. A suitable site has been procured in Fort William for the proposed low-level observatory, and plans of the buildings prepared by their architect, Mr. Sydney Mitchell, and submitted to the Directors and the Meteorological Council, and approved of. The plans and specifications are at present in the hands of the contractors, and the building will forthwith be commenced.

Mr. Herbertson exhibited to the meeting an instrument, named the stephanome, designed by Prof. Tait, for use at the Ben Nevis Observatory for measuring the angular size of halos, fog-bows, glories, &c.; also a valuable collection of sixteen photographs taken at the Observatory, of which the following are of special interest: a cirrus cloud in the northern horizon, taken at midnight in June, when the clouds are seen to be brightly illuminated; St Elmo's Fire, at 11 p.m. on the top of the stove-pipe; and views of the Observatory after continued fog and strong wind, but no fall of snow, when everything is covered with long crystals of ice formed out of the fog.

Dr. Buchan read a paper on the distribution of storms round the Scottish coasts, based on the observations made at the lighthouses during the past seven years. The year is divided by the equinoxes into two strongly contrasted portions as regards storms of wind. The minimum occurs in July, and the maximum in January. Over the whole country there is an annual average of 431 hours of storm occurrence. Dividing Scotland into seven districts, the following is the order of occurrence: Firth of Clyde, 327 hours; Tweed to Aberdeen, 373 hours; Aberdeen to Caithness, 379 hours; Fort William to Islay, 408 hours; Cape Wrath to Mull, 435 hours; the Irish Sea, 508 hours; and Orkney and Shetland, 562 hours. From a report prepared by Mr. Omond it appears that, on an average of the past five years, the wind at the Ben Nevis Observatory has risen to or exceeded the rate of 45 miles an hour, 849 hours per annum.

Mr. H. N. Dickson read a paper on "The Weather Lore of Scottish Fishermen." The fishermen had a very complete and generally accurate knowledge of weather phenomena as far as it was purely a matter of observation. In the course of his inquiries he had got a great deal of miscellaneous information from them on prognostications. The prognostications which received the greatest acceptance among the fishermen were those of halo, coronæ, and mock sunrises. It is a belief current from Aberdeen to Wick that, if a sun-dog preceded the sun, it was a sign of good weather, but if it followed the sun it was a sign of bad weather. Another very general belief in prognostications was the existence of spiders' webs amongst the cordage of ships and in sails. That was a very general belief all along the coast. There was another prognostication which was currently believed in by the fishermen, taken from the occurrence of broken rainbows, which are called "packmen," from the fact that the packmen sold pieces of coloured ribbon. As regards the cirrus cloud, in Shetland and Orkney and on certain parts of the west coast, but not on the east coast, there was an almost universal belief in "weather-heads." If these "weather-heads" ran in the direction of north-east to south-west, it was a sign of good weather, but if it ran south-east to north-west, it was an unfavourable sign. If the aurora rises in the north, and does not come past the zenith, it is a sign of good weather; but should the streamer extend beyond, a gale of south wind is expected. The only other point with regard to the aurora was that in Shetland it was supposed to be near a very severe gale if the aurora emitted a sound resembling the shaking of a blanket. Another prognostication, very interesting in its way, and which all fishermen had seen, is the "false dawn." The "false dawn" was when the dawn seemed to break, and then disappeared. There was some question as to whether it was a prognostication. At St. Andrews they were almost unanimous in believing it as a prognostication, and in other places he got individuals who believed it was a sign of good or bad weather. It was interesting in this way that he had never heard of the "false dawn" as a prognostication before, and he made some investigations as to whether it was common in other parts of the world, and he found it was also current among the Negroes of South America. In Shetland there was a class of prognostications which did not appear anywhere else. It was a sign of a coming gale if the surface of the water became stiff and bubbles remained in the wake of a boat, and if the wake of a boat remained visible for an unusually long time. Another prognostication was known as "cheepers." A sound was heard as if a lot of little birds were floating above the boat, and gave a sort of cheeping sound. That was also called "foul air" by another class of fishermen. In the Outer Hebrides the state of the air was almost the only thing the men paid attention to. It was current all down the west coast that a heavy surf was the sign of a gale approaching, but on the east coast one did not hear much of the heavy surf. He had found among the fishermen much less superstition than they usually got credit for, especially at the largest stations. In the smaller stations, where the boats were very small, there was still a good deal of superstition. In the larger stations, where the boats were large and the men went far out to sea, there was a great deal of faith in weather prognostications and a strong desire for instruction.

Mr. H. N. Dickson also added a note on the temperature of the water round the east coast of Scotland. The curve of the daily variation of temperature in the North Sea was as nearly as possible symmetrical above and below the mean. That was the case where there was reason to believe the water was almost stationary. In observations taken in the North Atlantic and on the west coast of Scotland in warm currents of water, as long as the curve was below the mean, it was almost quite straight, and when above the mean the maximum was intensified and sharpened. In observations taken in the cold Polar current off the Island of Jan Mayen the opposite was the case, the curve being deep below the mean and flat above it.

Mr. Philip Sewell gave a few notes of a voyage he made to Siberia last summer. From the temperature observations and other information submitted, he considered trading to the mouths of the Obi and Yenissei to be practicable in ordinary summers.

TWO-NOSED CATENARIES.[1]

THE curve to be given to an ideal linear chain or rib under uniform-vertical-load area between itself and a horizontal straight line is well known to be a Transformed Catenary, having its ordinates in a constant ratio to the corresponding ordinates of a Common Catenary inverted, with the horizontal straight line as directrix (Rankine, "Civil Engineering"; Church, "Mechanics of Materials," &c.).

Thus, the equation of the Common Catenary being—

$$\frac{y}{m} = \cosh\frac{x}{m},$$

using the notation of the hyperbolic functions, then the equation of the Transformed Catenary will be—

$$\frac{y}{m} = r\cosh\frac{x}{m},$$

r being a fraction, greater or less than unity.

The authors of this paper appear to have been the first to notice the elegant mathematical fact that, for values of r numerically less than $\frac{1}{3}\sqrt{3} = 0{\cdot}577$, the Transformed Catenary possesses two points, equidistant from the vertex, at which the curvature is a maximum; so that in the practical design of masonry arches, which are almost always made circular, a better

[1] "Two-Nosed Catenaries and their Application to the Design of Segmental Arches." By T. Alexander, C.E., Professor of Engineering, Trinity College, Dublin, and A. W. Thomson, B.Sc., A.M.I.C.E., Lecturer in the Glasgow and West of Scotland Technical College. (From the Transactions of the Royal Irish Academy, vol. xxix. Part 3, 1888.)

approximation to the true theoretical shape of the arch is attained either by taking the "three-point circle," passing through the vertex A and the two points of equal curvature B_2 and B_2', or by taking the "described circle," touching at B_1 and B_1', or by taking the "inscribed circle," touching at A and again internally at B_3 and B_3' beyond the points of maximum curvature B_1 and B_1', instead of taking, as customary, the circle of curvature at the vertex; and the authors show that if an elliptical arch is described, the proper approximation to its shape is obtained from an orthogonal projection of one of these circles.

The points B_1 and B_1' are called the *noses* of the transformed catenary, and give the name to the paper.

The transformed catenary, which may be taken as the line of thrust, is shown to lie below the "described circle," and above the "three-point circle," so that by taking these or similar circles for the boundaries of the ring of the arch, the proper stability of the arch is secured.

The mathematical treatment of the Catenary given by the authors would gain considerably in elegance by the employment of the hyperbolic functions, now no longer to be disregarded; thus, instead of writing—

$$y = r\frac{m}{2}\left(e^{\frac{x}{m}} + e^{-\frac{x}{m}}\right)$$

$$\tan\theta = \frac{dy}{dx} = \frac{r}{2}\left(e^{\frac{x}{m}} - e^{-\frac{x}{m}}\right),$$

the notation—

$$\frac{y}{m} = r\cosh\frac{x}{m},\ \tan\theta = r\sinh\frac{x}{m},$$

should be employed; and for purposes of numerical calculation of these hyperbolic functions, it is only necessary to notice that if $\tan\theta = \sinh u$, $\sec\theta = \cosh u$, then $u = \text{hyp.}\log(\sec\theta + \tan\theta)$; so that the table of u, already calculated by Euler, used in conjunction with the tables of the ordinary circular functions, will give us the numerical values of the hyperbolic fnnctions; for large values of u, when θ denotes an angle being nearly a right angle, the approximate equation—

$$\log\cosh u = \log\sinh u = u\log e - \log 2$$

being sufficient.

Tables of numerical results are given at the end of the paper, with practical illustrations, for the benefit of architects and engineers, and supplementary tables are added for the immediate designing of brick, sandstone, and granite arches, with circular soffits; so that these investigations should prove decidedly useful to those engaged in the design of similar structures.

A. G. G.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xi. No. 3 (Baltimore, April 1889.)—Were it not for the size of the pages, this number might be taken to be a number of the *Mathematische Annalen*, seeing that out of its ninety-eight pages sixty-eight are written in German. The first memoir, by Oskar Bolza (pp. 195–214), is entitled "On the Construction of Intransitive Groups," and touches on points discussed by Jordan ("Traité des Substitutions"), Capelli ("Sopra l'Isomorfismo dei Gruppi di Sostituzioni"), Netto ("Substitutionentheorie"), Cayley ("Theory of Groups"), and Dyck ("Gruppentheoretische Studien"). This is followed by a short note by Karl Heun (pp. 215–20), on "Die herstellung einer linearen Differentialgleichung aus einem gegebenen Element der Integralfunction." Next we have an important memoir by Koenigsberger (pp. 221–82), "Ueber die Reduction von Integralen transcendenter Functionen." The closing note, by Dr. Franklin (pp. 283–92), "On the Double Periodicity of the Elliptic Functions," *inter alia*, proves a theorem of a bicircular quartic, enunciated by Clifford (see Crofton, L.M.Soc. Proc., vol. ii), and also results due to Siebeck and Greenhill, but all are established here from a different point of view.

Rivista Scientifico-Industriale, February 15.—Researches on the thermo-electric conductivity of magnetized iron (concluded), by Prof. Ercole Fossati. From these experiments the author concludes generally that the electric conductivity of iron either suffers no change under transverse magnetization or undergoes some increase; this increase, however, is much less than the diminished conductivity of iron magnetized in the longitudinal direction. This inference agrees perfectly with the deductions he had already arrived at experimenting with iron conductors of varying dimensions.—Some experiments with a new battery, by Prof. Augusto Righi. Excellent results have been obtained from the battery here described, which consists of 108 condensers disposed in six groups of eighteen each, one above the other, in order to obtain high potentials. The outer armatures communicate with the conductors of a Holtz machine, the central with the ground, and a capacitity is thus obtained equal to that of $\frac{18}{6} = 3$ jars, united in a single battery with armatures communicating directly with the two conductors of the machine. The capacity of the whole system is thus 18,810 electrostatic units, or about $\frac{1}{18}$ microfarad.

Rendiconti del Reale Istituto Lombardo, February 28.—Notice of the late Prof. Giuseppe Meneghini, by Prof. T. Taramelli. In this biographic memoir a summary account is given of the services rendered to geological studies by the eminent naturalist, who was born in Padua in 1811, and died in January of the present year. Meneghini is best known as joint translator, with Savi, of Murchison's work on the Alps, Apennines, and Carpathians, and by his systematic monograph on the fossils of the Upper Lias in Lombardy and Central Italy. His last publication was a paper on the Cambrian trilobites of the Iglesiente district, and his name will always be associated rather with the palæontological than the stratigraphic or petrographic side of geology.—Meteorological observations made at the Royal Brera Observatory, Milan, during the month of February.

THE last issue of the *Memoirs of the St. Petersburg Society of Naturalists* (vol. xix., Botany) contains, besides several very interesting short papers in the Proceedings, a new contribution to the flora of Novgorod, by A. Antonoff, which raises the number of species of flowering plants discovered in the Government of Novgorod to 700; a note on the comparative anatomy of the tissues in the leaves of *Salicineæ* as a basis for classification, by V. Dobrovlanski; and a suggestive description of the flora of the Shenkursk and Kholmogory districts of Archangelsk, by N. Kuznetsoff. Owing to the extension of a subsoil of limestone, which is much warmed by the sun during the summer, the flora of the region contains a number of species belonging to a more congenial climate, while several species characteristic of those latitudes are wanting. On the other hand, owing to its proximity to the Urals, and the existence on the west of such a barrier as Lake Onega, the flora contains a considerable number of Siberian species, while many West European species do not appear. M. Kuznetsoff's remarks on the extension of the *Abies sibirica*, which is slowly advancing towards the West, and the lime-tree, which seems to be, on the contrary, disappearing, are very interesting. Both the *Ulmus campestris* and the *U. effusa* were found as far north as the sixty-third degree of latitude. The presence of the *Lymnanthemum nymphoides*, which is found in Lithuania and South-Eastern Russia, but not in Central Russia, save Kursk, is especially remarkable, and M. Kuznetsoff explains the extension of this aquatic plant over Archangelsk by its having spread along the canal which connects the Volga with the Dvina. No fewer than twelve other South Russian plants, which must have migrated along the same canal, are named by the author. As to the *Mulgedium tataricum*, C. A. Mey., which is characteristic of the salt steppes of Astrakhan, it has been found on a shoal of the Dvina, under 64° N. lat. The paper is accompanied by a map, showing the western limits of extension of the *Abies sibirica*.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 14.—"On the Organization of the Fossil Plants of the Coal-Measures. Part xvi." By W. C. Williamson, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester.

In this memoir the author first calls attention to detached observations, made in his earlier memoirs, relating to the manner in which a medullary axis is developed in the interior of each of the primary vascular bundles of the Carboniferous Lycopodiaceæ. He then traces the changes undergone during the development of a small branch-bundle in *Lepidodendron Harcourtii*.

This is followed by a description of a small new species of Lepidodendron.

In a second new species, named *Lepidodendron intermedium*, an apparently early form of exogenous zone is shown to exist. When describing, in his previous memoir, Part xi. (see Plate 49, Fig. 11), the stem now designated *Lepidodendron fuliginosum*, he showed that, in it, we have an example of the most rudimentary and least perfectly developed form of an exogenous xylem yet seen amongst these Carboniferous Cryptogams. In this example, but a few radiating laminæ of vascular tissues make their appearance in the innermost cortex. In the plant now described, though these few laminæ are represented by a continuous cylindrical zone of tracheids, and though the laminæ are arranged in radial order, they are still embedded in a mass of cellular tissue, much in excess of what constitutes the medullary rays in the higher types of Lepidodendroid organization.

A fourth new species of Lepidodendron is described, under the name *L. Spenceri*, in young states of which no medulla is visible, but, in its place, a number of vertically elongated cells and imperfectly lignified scalariform tracheids are seen, inclosed within an outer series of perfectly lignified tracheids.

A fifth new species, *Lepidodendron parvulum*, is also described; after which the author points out the differences between the mode of development of the cellular medulla of these exogenous Cryptogams, and that of the representative organ among the Dicotyledonous Exogens. Amongst the ordinary Exogens, the growing tip of a stem or branch is a mere aggregation of cells, which mass is soon separated into two zones by the development within it of a ring of vascular bundles. The cells inclosed within this ring become the medulla, and those external to it constitute the cortex. Such a pith subsequently undergoes but a very limited enlargement. In most cases a time arrives when it grows less with age, and ultimately disappears; but in the Lepidodendra, though the tip of each growing stem was, in the first instance, also a cellular mass, an axial solid bundle of vessels was developed in the centre of the new growth almost at its very commencement. But it was only after this growth had made some progress, and the twig had become clothed externally with numerous leaves, that the first traces of a medulla began to appear in the centre of the bundle. It is thus clear that the medulla of these Carboniferous Lycopods is not genetically homologous with that of an ordinary exogenous flowering plant.

The axial vascular medullary bundle expanded into a hollow cylinder under the internal pressure of the growing medulla, which latter not only attained to considerable dimensions, but was a persistent organ. The ring inclosing the medulla supplied all the vascular bundles going to leaves and in part to branches. The author demonstrates that the branches are supplied with such bundles in two ways; when the growing stem divides dichotomously, which it does as amongst living Lycopods, the medullary vascular cylinder splits into two equal halves. But, besides this mode, the author shows that very frequently comparatively small segments are cut completely out of the vascular cylinder, in which a gap is thus left where the bark and the medulla meet. The angular segment thus detached develops into a solid cylindrical bundle, in which, in time, a medulla forms as before. The author is inclined to believe that all these latter forms of bundles only supply short abortive lateral branches, which most probably supported Lepidostrobous fruits.

March 21.—"On the Velocity of Transmission through Sea-water of Disturbances of Large Amplitude caused by Explosions." By Richard Threlfall, M.A., Professor of Physics, and John Frederick Adair, M.A., Demonstrator of Physics, University of Sydney. Communicated by Prof. J. J. Thomson, F.R.S.

This paper contains an account of a large number of experiments made with the object of determining the velocity of waves of compression caused by explosions under water.

The method adopted depended on the use of a certain "gauge" devised for the occasion, whereby the arrival of the disturbance at a given point was transmitted to a chronograph.

The disturbances themselves were caused by submarine explosions of dynamite and gun-cotton in quantities varying from nine ounces to four pounds.

The distance over which the velocity was measured was about 200 yards.

The water was that of the Pacific Ocean in the harbour of Port Jackson, New South Wales.

The chronograph was of the falling pendulum description, and fired the charge automatically.

The absolute time was obtained by comparing the chronograph tuning-fork with an astronomical clock.

The distance was obtained in terms of the standard yard of New South Wales by means of trigonometrical survey. The chief results for the range quoted are as follows:—

Class.	Description of explosive.	Number of experiments (each experiment involving two explosions and time measurements).	Velocity found in metres per second.	Temperature C.	Velocity of sound calculated in metres per second.	Excess of velocity as compared with velocity of sound.
						per cent.
A	9 oz. dry gun-cotton	11	1732 ± 22	17°·8	1523	13·75
B	10 oz. No. 1 dynamite	24	1775 ± 27	14·5	1508	17·7
C	18 oz. dry gun-cotton	5	1942 ± 8	18·3	1525	27·3
D	64 oz. dry gun-cotton	3	2013 ± 35	19·0	1528	31·7

The chief portion of the paper is occupied in the description of the details of the precautions taken to make the measurements as accurate as possible. This involves some chronographic improvements. An explanation of the large observed velocity is attempted.

"On an Effect of Light upon Magnetism." By Shelford Bidwell, M.A., F.R.S.

Several experimenters in the early part of the present century tried to magnetize iron and steel by the action of light,[1] but I do not know of any recent attempts in this direction, and of late years the thing has been generally regarded as impossible. Under ordinary circumstances there can be little doubt that this is the case, but, if a certain condition is fulfilled, we might, I think, expect to find some evidence of the action of radiation upon the magnetism of iron.

The condition is that the susceptibility of the bar AB to be operated upon shall be greater (or less) for a magnetic force in the direction AB than for an equal one in the direction BA. This paper contains a short preliminary account of a series of experiments which have been undertaken with iron bars having this property. Much yet remains to be done, which will require a considerable amount of time, and for which special apparatus must be constructed. In the meantime, the results already obtained appear to possess sufficient interest to justify their publication.

The method of preparing the bars is as follows. A piece of soft iron rod, which may conveniently be 10 or 12 cm. long and from 0·5 to 1 cm. in diameter, is raised to a bright yellow heat and slowly cooled. When cold, it is placed inside a solenoid, through which is passed a battery current of sufficient strength to produce a field of about 350 or 400 C.G.S. units. The iron when removed from the coil is found to be permanently magnetized, and its north pole is marked for the sake of distinction with red sealing-wax varnish. The bar is then supported horizontally and in an east and west direction behind a small reflecting magnetometer, and over it is slipped a coil, which is shunted with a rheostat, the resistance of which can be gradually increased from 0 to 26 ohms. The coil can be connected by a key with a single battery cell, which is so arranged as to produce a demagnetizing force inside the coil. The resistance of the rheostat is slowly raised, so that more and more current passes through the coil, the battery key being alternately lifted and depressed until the magnetometer indicates that the iron bar as a whole is perfectly demagnetized. The strength of the demagnetizing force required varies according to circumstances: it is generally about one-thirtieth or one-twenty-fifth of the original magnetizing force.

After this treatment the iron rod does not differ, so far as ordinary tests will show, from one which has never been submitted to magnetic influences. Nevertheless, as is well known, it possesses certain properties which distinguish it from a piece of really virgin iron. In the first place, the magnetization induced by a force acting in such a direction as to make the

[1] Chrystal, "Encyclopædia Britannica," vol. xv. p. 274, mentions the following names: Morichini, Mrs. Somerville, Christie, Riess, and Moser.

marked end a north pole is greater than that caused by an equal force in an opposite direction. Again, if such a bar be held horizontally east and west (to avoid terrestrial influences), and tapped with a mallet, the marked end at once becomes a north pole. A similar effect follows if the rod be warmed in the flame of a spirit-lamp. Lastly, if it be placed inside a coil and subjected to the action of a series of rather feeble magnetic forces, of equal strength but alternating in direction, the marked end will generally become a north pole, even though the last of the alternate forces may have tended to induce the opposite polarity.

A rod treated as above described appears to be remarkably sensitive to the action of light. When such a rod is placed behind the magnetometer, and illuminated by an oxyhydrogen lamp about 70 cm. distant, there occurs an immediate deflection of from 10 to 200 scale-divisions,[1] the magnitude of the effect varying in different specimens of iron. As the action of the light is continued, the deflection slowly increases. When the light is shut off, the magnetometer instantly goes back over a range equal to that of the first sudden deflection, then continues to move slowly in the backward direction towards zero.

The first quick movement I believe may be due to the direct action of radiation, and the subsequent slow movement to the gradually rising temperature of the bar. With a thick rod (1 cm. in diameter) the slow movement is barely perceptible, extending over only one or two scale-divisions in the course of a minute, the spot of light becoming almost stationary after the first sudden jump. With a thin rod the sudden effect is generally smaller, while the slow after-effect is greater, and may continue until the spot of light passes off the scale.

As a general rule the magnetic effect is such as to render the marked end of the rod a north pole: occasionally, however, it becomes a south pole, but in such cases I have always found that the polarity is comparatively feeble. It may even happen[2] that the marked end becomes north when certain portions of the rod are illuminated, and south when the light acts upon other portions. This is probably due to irregular annealing and a consequent local reversal of the direction of maximum susceptibility: it indicates that the light effect is local, and is confined to the illuminated surface. In one remarkable specimen, which happens not to have been annealed at all, the sudden effect and the slow effect are in opposite directions. When the light is turned upon this rod, there is at first a sudden deflection of twenty magnetometer-scale-divisions to the left, the spot afterwards moving slowly and steadily towards the right. When the light is shut off there occurs at once a jump of twenty divisions further towards the right before the spot begins to move back in the zero direction.

Some attempts have been made to repeat the experiments with light polarized by means of a Nicol's prism; but, either because the largest prism at my disposal was too small (its aperture being barely 2 cm.), or because too much of the radiant energy was absorbed by the spar, I failed to get any magnetic effects whatever with the prism in either position.

[Prof. Silvanus Thompson has quite recently been kind enough to lend me a very large and excellent Nicol's prism. From a few experiments already made with this instrument it appears that the action of the light is quite independent of the plane of polarization.]

There can be no doubt whatever of the reality of the effects here described: they are perfectly distinct, and are at any time reproducible with certainty. The only question is how much of them is primarily caused by the action of light, and how much by mere incidental change of temperature. But, taking all the circumstances into consideration, I think the evidence is in favour of the conclusion that the *instantaneous* magnetic change, which occurs when a prepared iron bar is illuminated, is purely and directly an effect of radiation.

[1] The magnetometer mirror was 1 metre distant from the scale, and each division = 0·64 mm. ($\frac{1}{40}$ of an inch).
[2] This has been observed in two specimens.

Physical Society, March 23.—Prof. Reinold, President, in the chair.—Prof. J. V. Jones read notes on the use of Lissajous's figures to determine a rate of rotation, and of a Morse receiver to measure the periodic time of a reed or tuning-fork. In determining resistance absolutely by the B.A. or by Lorenz's method, it is important to know the speed of rotation at the instant when the deflection of the galvanometer needle is observed. To determine this, an arm carrying a mirror is caused to oscillate by a pin placed eccentrically in the end of the spindle, and a Lissajous's figure is obtained by using another mirror mounted on a vibrating reed driven electrically. Equality of period is obtained when the resulting elliptical figure is permanent, and the frequency of the reed is determined subsequently. In making the experiments it is found convenient to control the speed of the disk by braking it either with the finger or a piece of wood. The reed consists of a rectangular steel rod, 100 centimetres long, and section 1·51 × 0·60 centimetres, and the length of the vibrating segment can be altered by sliding it through a clamp. To permit of this change without altering the electrical contacts, the latter are formed by two independent springs, one of which is always in contact with the rod. For determining the frequency of the reed a second pair of contacts are operated by the vibrator and the currents recorded by a Morse receiver, whilst simultaneously the paper receives marks from a pen controlled by a standard clock. The limit of accuracy of this part of the experiment is found to be $\frac{1}{10}$ per cent., due to changes in the reed's frequency. This is a serious defect, and the author of the paper asked for advice as to the precautions necessary to obtain reeds of constant pitch. Prof. Ayrton, whilst recognizing the extreme importance of determining speed accurately, suggested that the inconstancy of the reed may be due to the impulse being given at the end instead of the middle of its swing, and recalled an experiment, shown before the Society by Prof. Perry and himself, in which the pitch of an electrically driven fork was varied greatly by altering the adjustment of the contact screw. Referring to Dr. Thompson's modification, where two tuning-forks drive each other, it was pointed out that the method requires the synchronism of the two forks to be very exact. Mr. Blaikley inquired whether any doors were opened or closed during the experiments, as the pitch of a reed is affected by the size of its resonance chamber. He also stated that the pitch of reeds driven pneumatically could be maintained constant to 1 part in 10,000, and mentioned that two forks nearly in unison influence each other's period when near together. Referring to the two forks mentioned by Prof. Ayrton, Prof. S. P. Thompson said it was advantageous to mount such forks on sounding boxes, for when placed at a suitable distance apart they then exert considerable mutual control. The sketch put on the board by Prof. Jones led Dr. Thompson to suppose that a perfect method of driving forks had been devised, for two springs were shown touching opposite sides of the bar, and such an arrangement might be used to complete the circuit, only when the reed is in the middle position. He also believed that forks give greater constancy than single reeds, and mentioned some recent improvements, in which one prong of an electrically driven fork is made of phosphor bronze. Mr. T. H. Blakesley, reasoning from ideas suggested by Mr. Stroh's experiments on vibrating membranes, concluded that the periods of forks, placed at $\frac{1}{4}$ wave-length apart, would not influence each other. In reply to a question from Mr. F. J. Smith, Prof. Jones thought there could be no "creeping" of the reed through the clamp. He also stated that he had been led to use a reed from the results obtained in Delaney's system of telegraphy, and the fact that Lord Rayleigh considered electrically driven forks satisfactory.—Dr. Hofford read extracts from the following papers:—On the Clark cell as a source of standard currents, by Prof. R. Threlfall and Mr. A. Pollock. The authors find, contrary to ordinary ideas, that Clark cells can be used to send currents of considerable magnitude without the E.M.F. being appreciably changed, and have constructed cells which give 0·001 amperes steadily for half an hour. This result has been obtained by increasing the size of the cell so that each electrode is about 5 square inches in area, and the internal resistance is about 6 ohms. For the ordinary small test-tube cell, the resistance of which may be about 1500 ohms, the current ought not to exceed 0·00001. On closing the circuit the P.D. (potential difference) drops almost instantaneously to its steady value, and when the circuit is opened rises equally rapidly to very nearly the original E.M.F. The cells completely recover in time. If the current sent be too large, the P.D. falls for a time, and afterwards rises and tends towards a fixed value. In this respect Clark's cells are greatly superior to large Daniell's, sending currents through the same resistance. The paper contains several tables and curves, as well as valuable results respecting the close agreement between the E.M.F.'s of a great number of different cells.—On the application of Clark's cell to the construction of a standard galvanometer, by Prof. R. Threlfall. A large cell, as above mentioned, together with a known platinoid resistance, are used to standardize a reflecting galvanometer, constructed with a single

movable coil, sliding in guides so as to vary the constant in known proportions. Two controlling magnets are carried on opposite sides of a sleeve sliding on a central tube, inclosing the long fibre, and a small hollow copper cylinder in oil acts as damper. The tangent law was found by calculation to be practically correct up to 15°, and the scale is curved so as to read tangents on a scale of equal divisions. To give this result the approximate polar equation to the curve is shown to be $r = f(1 + 0{\cdot}207\theta^2 + 0{\cdot}0269\theta^4)$. In standardizing the instrument a known current is sent through it from the Clark cell, and the controlling magnets adjusted till the spot comes to a fiducial mark on the scale. By varying the position of the coil, currents ranging from 0·001 to 0·4 ampere, can be measured.—On the measurement of high specific resistances, by Prof. Threlfall. The chief points dealt with are, the means for producing thin plates of material of known dimensions, the form and methods of magnetizing the needles, and the arrangement of suspension found necessary when great sensibility is required. For producing the plates, two plane platinized brass slabs are used, and kept at a known distance apart by micrometer screws, whilst the material is melted between them. The screws are then withdrawn, and the resistance determined by comparison with a megohm, using different E.M.F.'s in the two cases so as to obtain about equal deflections. As regards the galvanometer, after many unsatisfactory attempts to use one made according to the Messrs. Gray's pattern, the coils of that instrument were mounted in the ordinary way, and used with an astatic combination of magnetized steel disks. Extreme care was taken to obtain disks exactly similar, and a pair of electro-magnets were made to magnetize them *in situ*, so as to obtain great astaticism. For suspending the magnets, quartz fibres were found greatly superior to silk, the zero of the instrument being very indefinite, even with a silk fibre 30 inches long. Considerable difficulty was experienced in attaching the mirror to the needle on account of the distortion produced by ordinary cements; slow drying white paint was ultimately used and found satisfactory.—On the measurement of the resistance of imperfectly purified sulphur, by Prof. Threlfall and Mr. A. Pollock. The apparatus used is described in the previous paper, the galvanometer of 16,000 ohms being arranged to give one division for 2×10^{-12} amperes. A mean of the results obtained gives about 11×10^{13} ohms per cubic centimetre as the specific resistance. In performing the experiments many precautions were required to prevent air currents and magnetic disturbances, and paraffin keys were found to give much better insulation than ebonite ones. In conclusion, the authors enumerate the chief points to which attention should be paid in designing and using very sensitive galvanometers. They consider it desirable to use quartz fibres at least 6 feet long, to provide very fine adjustment for the controlling magnets, whose supports should be independent of the suspension arrangement, and believe that the whole should be placed in a thick soft iron cylinder. Mr. Boys, speaking of attaching mirrors to wires, said he found a very small speck of shellac varnish to be very satisfactory. As regards quartz fibres, Prof. Threlfall's method of producing them differed materially from his, and the fibres were much thicker. He considered it quite unnecessary to use fibres anything like 6 feet long, and thought 15 inches quite sufficient where the weight to be supported was not large. Prof. Ayrton regarded the results obtained from Clark's cells to be of extreme scientific and commercial importance, for they showed that a very convenient standard of E.M.F. could also be used as a standard for current. Prof. Thompson, after commenting on the convenience of good E.M.F. standards, expressed a hope that Prof. Threlfall would be able to suggest a convenient method for producing a standard ampere.

Royal Meteorological Society, March 20.—Dr. W. Marcet, F.R.S., President, in the chair.—Dr. Marcet delivered an address on "The Sun: its Heat and Light," in which he said that the source or origin of all meteorological phenomena is the sun, which sends or radiates its heat to the earth through the molecular vibration of the invisible matter connecting earth with space. If there were no air and moisture we should feel the sun's heat to a much greater extent. After the sun's rays have reached the earth, a portion of the heat they bring with them is absorbed by the earth and terrestrial objects, another is converted into motion, and a third is reflected into space. After describing the various actinometers and other instruments used for determining the amount of solar radiation, Dr. Marcet showed that the temperature of the solar rays falls rapidly when tested at increasing altitudes in a balloon. He then gave an account of some observations made by Mr. Glaisher in the celebrated balloon ascent which he and Mr. Coxwell made at Wolverhampton on September 5, 1862, when they reached a height of about seven miles above the earth's surface. Regarding the transformation of solar heat into motion, a very interesting illustration is afforded by the radiometer, in which the direct influence of the sun's rays causes a light vane to rotate; while another illustration of the mechanical effects of heat upon fluids is their conversion into what is known as the spheroidal condition. In connection with this Dr. Marcet explained how by wetting his hand he could pass it through molten lead without injury. He also related the case of Henry Hall, one of the keepers of the Eddystone Lighthouse, who, on the occasion of the fire which destroyed the lighthouse on December 4, 1755, accidentally swallowed some molten lead, but did not die till several days afterwards, when a piece of lead was taken out of his stomach. Dr. Marcet then briefly drew attention to the sun's light, stating that the passage of the sun's light through our atmosphere alters it in kind to a great and remarkable extent. Light can be decomposed into its elementary colours. In connection with this branch of the subject Dr. Marcet performed a number of experiments, including, among others, that of passing a ray of light through a glass freed from dust, when the ray disappears within the vessel, but reappears on the other side, showing that the power to form light was there, though not the material for this power to act upon. Dr. Marcet concluded his very interesting address by describing the various forms of sunshine recorders.—The meeting was then adjourned in order to afford the Fellows and their friends an opportunity of inspecting the Exhibition of Instruments opened on the previous evening. A full account of this Exhibition has already been given on p. 523.

Geological Society, March 20.—W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—Supplementary note to a paper on the rocks of the Atlantic coast of Canada, by Sir J. W. Dawson, F.R.S.—The occurrence of colloid silica in the Lower Chalk of Berkshire and Wiltshire, by Mr. W. Hill and Mr. A. J. Jukes-Browne.—Note on the pelvis of *Ornithopsis*, by Prof. H. G. Seeley, F.R.S. The remains preserved in Mr. Leed's collection at Eyebury, and described by Mr. Hulke, are the largest and most perfect pelvic bones of a Saurischian known in this country. An examination showed that the bones of the right and left sides were united in the median line almost throughout their length by a median suture, and that they formed a saddle-shaped surface internally from front to back. After giving a detailed description of the pubis and ischium, the author stated that he was not aware that this type of pelvis had been previously observed. He noted that the antero-posterior concavity between the anterior symphysis of the pubic bones and the posterior symphysis of the ischia was a well-marked characteristic of Saurischian reptiles, but that it remained to be determined to what extent the median union of the pubic bones was developed in the group. It was impossible to judge of the form of the ilium from the imperfect fragment preserved, but it did not make any recognizable approximation to the bone in those American genera which offered the closest resemblance of form to the pubis and ischium. There were several minor differences of proportion between the bones from the Oxford Clay and those from the Wealden of the Isle of Wight, and the former differed in ways pointed out from *Morosaurus*, *Diplodocus*, and *Brontosaurus*, though there were resemblances.

Zoological Society, March 19.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a list of the fishes collected at Constantinople and sent to the Society by Dr. E. D. Dickson. The species were sixty-six in number, and had been determined by Mr. G. A. Boulenger.—Mr. Tegetmeier exhibited a female Gold Pheasant in male plumage, and a curiously distorted pair of horns of the Ibex of Cashmir.—The Rev. A. H. Cooke read a paper on the position of the land-shells of Australia and the adjacent islands, commonly referred to the genus *Physa*, which it was shown (mainly from an examination of the *radula*) were really more nearly allied to the genus *Limnæa*. Mr. Cooke proposed to refer these species to the genus *Bulinus*, established by Adanson in 1757.—Mr. G. A. Boulenger read notes on some specimens of Lizards belonging to the Zoological Museum of Halle, which had been sent to him for examination. To these notes were appended revised descriptions of two Lizards from the Argentine Republic, *Gymnodactylus horridus* and *Urostrophus scapulatus*.—A communication was read from Prof. W. N. Parker,

containing an account of the occasional persistence of the left posterior cardinal vein in the Frog. This condition, abnormal in the Frog, was shown to be essentially normal in *Protopterus*.—A communication was read from Mr. J. Douglas Ogilby, containing notes on some fishes new to the Australian fauna.—Mr. Oldfield Thomas read a paper giving the description of a new Bornean Monkey belonging to the genus *Semnopithecus*, obtained by Mr. Charles Hose on the north-west coast of Borneo. The author proposed to name it *Semnopithecus hosei*, after its discoverer.

April 2.—Prof. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of March 1889, and called attention to a specimen of the Manatee (*Manatus australis*), purchased March 2, being the second example of this Sirenian obtained alive by the Society; to an Oriental Phalanger (*Phalanger orientalis*, var. *breviceps*), presented by Mr. C. M. Woodford, of Sydney; and to a specimen of Owen's Apteryx (*Apteryx oweni*), presented by Captain C. A. Findlay.—Mr. Smith-Woodward exhibited and made remarks on a maxilla of the early Mesozoic Ganoid *Saurichthys*, from the Rhætic formation of Aust Cliff, near Bristol.—A communication was read from Mr. W. K. Parker, on the osteology of *Steatornis caripensis*. The conclusion arrived at as regards the affinities of this isolated form of birds was that *Steatornis* is a waif of an ancient avifauna, of which all the near allies are extinct, and that *Podargus* of Australia is its nearest surviving relative.—Mr. Oldfield Thomas read some preliminary notes on the characters and synonymy of the different species of Otter. The author gave a revised synonymy of the four species of *Lutra* recognized as belonging to the Palæarctic and Indian Regions, and of the two found in the Æthiopian Region. The American Otters, for want of a larger series of specimens, could not at present be satisfactorily worked out.—Mr. E. T. Newton read a paper, entitled "A Contribution to the History of Eocene Siluroid Fishes." Mr. Newton observed that spines of Siluroid fishes from the Bracklesham Beds were described by Dixon in his "Fossils of Sussex" (1850), and referred to the genus *Silurus*. Mr. A. Smith-Woodward had recently shown good reason for referring these specimens, and certain cephalic plates from the same horizon, to the tropical genus *Arius*. The greater part of a skull, from the Eocene Beds of Barton, in the Museum of the Geological Survey, confirmed the latter generic reference. Its close resemblance to a skull of *Arius gagorides* in the British Museum left no room for questioning their generic relationship, while at the same time the fossil differed from any known species of *Arius*. The fortunate discovery of one of the otoliths within the fossil skull, and its resemblance in important points to that of *A. gagorides*, still further confirmed this determination. Some other otoliths from Barton, and one from Madagascar, were also referred to the genus *Arius*.—Mr. A. Smith-Woodward read a note on *Bucklandium diluvii*, a fossil from the London Clay of Sheppey, noticed by König, and hitherto not satisfactorily determined. It was shown that this fossil was a portion of the skull of a Siluroid Fish allied to the existing genus *Auchenoglanis*.—A communication was read from Mr. H. W. Bates, F.R.S., containing descriptions of new species of the Coleopterous family Carabidæ, collected by Mr. J. H. Leech in Kashmir and Baltistan.—A second communication from Mr. Bates gave descriptions of some new species of the Coleopterous families Cicindelidæ and Carabidæ, taken by Mr. Pratt at Chang Yang, near Ichang, in China.

PARIS.

Academy of Sciences, April 1.—M. Des Cloizeaux, President, in the chair.—On regulating the velocity of a dynamo-electric machine serving as a receiver in the transmission of force by electricity, by M. Marcel Deprez. The case is first considered of the magnetic field and of the electromotive force of each machine, which are shown to be functions of the intensity of the general current only when the inducting electros are disposed in a simple series in the principal circuit. Cases are then discussed in which the receiver has a constant magnetic field, and in which the field of the generator is constant.—On the biorthogonal reduction of a lineo-linear form to its canonical form, by Prof. Sylvester. Taking F as a lineo-linear function of two series of letters—

$$x_1, x_2, \ldots, x_n; \xi_1, \xi_2, \ldots, \xi_n;$$

then F will contain n^2 terms. By subjecting the letters x and ξ respectively to two independent orthogonal substitutions, arbitrary quantities are introduced into the transformed form $n^2 - n$, so that by giving them suitable values we should be able to get rid of this number of terms while retaining the n pairs alone, whose arguments will be, for instance,

$$x_1\xi_1, x_2\xi_2, \ldots x_n\xi_n.$$

Prof. Sylvester here calls the multiples of these arguments the *canonical multiplicators*; he gives the rule for determining them, and at the same time for finding the two simultaneous orthogonal substitutions which lead to the canonical form.—On the progress of the Suez Canal, and its state in the year 1888, by M. de Lesseps. Of the works undertaken to widen the Canal from 22 to 65, 75, and in some places even 80 metres, some have already been completed, and for a distance of about 15 kilometres from Port Said vessels have now ample space for passing each other. The depth will be increased to 8·50, and ultimately to 9 metres, and navigation by night is facilitated by luminous buoys and tow-paths, the light being obtained by means of compressed gas. All the sidings have been widened to 100 metres between Suez and Port Said, thus allowing six large vessels to be shunted at the same time in all of them. Ships using the electric light are now able to traverse the Canal in about twenty hours, the time hitherto required being from thirty-five to forty hours. In 1888 the Canal was traversed by 3440 vessels of 6,640,832 tons (2625 of 5,223,254 tons, British), yielding to the Company 65,102,000 francs in tolls, &c.—On an aperiodic balance with direct readings of the last fractional weights, by M. P. Curie. The instrument here described, and illustrated with a vertical section and a general view, has been constructed for the purpose of increasing the rapidity and accuracy of weighing operations in scientific and industrial laboratories.—On transformation and equilibrium in thermodynamics, by M. P. Duhem. It is pointed out that the "new function" spoken of by M. Gouy in a recent communication (*Comptes rendus*, cviii. p. 507), had already been anticipated by Mr. J. W. Gibbs, in 1873, whose views were later explained by M. Duhem in his work on the thermo-dynamic potential.—On the difference of potential at contact of a metal and of a salt of the same metal, by M. H. Pellat. The author's researches lead to a general law thus expressed: The difference of normal potential between a metal and the solution of a salt of the same metal is nil. It also follows that the difference of potential of two salts of the same acid and of different metal at contact, increased by the difference of potential of these metals placed in contact, is proportional to the quantity of heat liberated by the substitution of one of the metals for the other in the salt of the acid in question.—On telephonography, by M. E. Mercadier. The problem of telephonography—that is, the transmission of sound to distances by telegraph lines, for instance—is here studied in connection with the recent improvements of the phonograph by its inventor, Mr. Edison, and by Mr. S. Tainter (graphophone). M. Mercadier's process, which shows good results even with inferior instruments, appears to be much simpler than that by which Mr. Edison has lately succeeded in communicating along the wires between New York and Philadelphia, using for the purpose his new improved phonograph.—Papers were contributed by M. F. Beaulard, on the double elliptical refraction of quartz; by M. Woukoloff, on the law of the solubility of gases; by M. René Drouin, on succinamic nitrile; by M. Albert Colson, on the artificial and natural alkaloids; by MM. P. Langlois and Ch. Richet, on the strength of the respiratory function as affected by anæsthetics; by M. Abel Dutartre, on the action of the poison of the land salamander (*Salamandra maculosa*); and by M. Haug, on the Lias formations in the sub-Alpine ranges between Digne and Gap.

BERLIN.

Meteorological Society, March 12.—Prof. von Bezold, President, in the chair.—Dr. Wagner spoke on the connection between cosmic and meteorological phenomena. After a short review of the earlier attempts to connect meteorological phenomena with the rotation of the sun or with the phases of the moon, he passed on to a consideration of Prof. von Bezold's recently published work, in which it is shown that the storms in Bavaria and Würtemberg have a distinctly recurrent periodicity of 26 days, corresponding to the period (25·84 days) of the sun's rotation. In opposition to this, Köppen had found from his own calculations, based on more extensive data, that the storms are recurrent with a periodicity of 29 and not 26 days, thus corresponding to the synodical period of the moon. The speaker had himself investigated the periodicity of storms, not only in Bavaria

and Würtemberg, but also in Baden, keeping the observations of lightning separate from those of mere rain-storms : this seemed to be necessary, not only inasmuch as von Bezold did not take any account of lightning, but also because the occurrence of lightning at the time of new moon, or during the last quarter of the moon, might give rise to apparent maxima resulting from purely optical causes. He found that the storms possess a periodicity of 29 days, which include three maxima, the chief of these being in the last half-quarter, the next at new moon, and the least at full moon. No physical explanation, or even any idea of any connection between the storms and the phases of the moon, can at present be given.—Dr. Assmann gave an account of a phenomenon which had been observed on the trees in the Thiergarten as a result of the recent heavy snow-fall. The masses of snow which were piled up high on the branches of the trees had slipped down round their sides and hung down like curtains; they possessed a not inconsiderable consistency and glacier-like structure. The superficial thawing which occurred daily about midday had contributed largely to bring about the modification which the snow had undergone.

Physiological Society, March 15.—Prof. du Bois-Reymond, President, in the chair.—Dr. Benda spoke on multinuclear mammalian spermatozoa, and refuted a number of objections which had been raised to his views on spermatogenesis.—Prof. Gad gave an account of experiments which Dr. Piotrowski had made under his direction, on the difference between the conducting power of nerves and their irritability. It was known that under certain conditions, as, for instance, when a nerve is dying or is surrounded by an atmosphere of carbonic acid gas, its power of conducting impulses shows no change, while at the same time the irritability of that part which is surrounded by the gas has disappeared. After confirming the above by renewed experiments, Dr. Piotrowski found that when he surrounded a small stretch of the sciatic nerve with alcohol vapour he obtained a result exactly the reverse to that observed with carbonic acid gas : the nerve was irritable, but could no longer convey impulses coming from its central end. Irritability and conducting power were tested, not only by muscular contractions, but also by the negative variation at the peripheral end of the nerve. Three distinct causes might be assumed for the difference between irritability and conducting power which had been experimentally proved as above. In the first place, irritability and conducting power might be two totally distinct properties of a nerve. But this view must be dismissed, inasmuch as the only possible way of conceiving the propagation of an impulse is to suppose that the impulse is transmitted from one transverse section of the nerve to another, so that the stimulation of one section acts as a stimulus to the rest. In the second place, it might be supposed that the electrical resistance of the nerve-sheath and medullary-sheath had been increased, so that the stimulus, which was applied from the exterior, could not overcome this increased resistance, while at the same time the conducting power of the axis-cylinder remained unchanged. But this view is untenable in face of the fact that alcohol vapour increases the irritability of a nerve but lessens its conducting power. And it is still further opposed by an experiment on the olfactory nerve of the pike. This nerve possesses scarcely any sheath, or at most an extremely thin one, and still it behaved exactly as does a sciatic nerve when surrounded by carbonic acid gas. Finally, mechanical stimuli were just as efficient as electrical, and in this case the resistance of the sheath does not affect the question. A third possible explanation was that nerves possess not only a longitudinal, but also a transverse irritability, and that the latter is diminished by the CO_2, and increased by the alcohol vapour. This last explanation was also rendered probable by an experiment in which the heightened irritability under exposure to alcohol vapour was still further increased when the current used for stimulation was led through the nerve at right angles to its length by means of wide electrodes instead of by means of the wire electrodes usually employed, in which latter case a small longitudinal stretch of the nerve is included between the points of the electrodes. The speaker therefore regards it as proved that the irritability of a nerve can be diminished by the action of CO_2 without its conducting power being simultaneously affected. Further, that by means of alcohol vapour the irritability may be increased, while the conducting power is at the same time considerably diminished, and that nerves possess a distinct transverse irritability. The speaker also regarded it as extremely probable that the effect of CO_2 and alcohol vapour is different upon the transverse and longitudinal conducting powers of a nerve.

Vienna.

Imperial Academy of Sciences, February 7.—The Secretary read a letter by Dr. Ludolf Griesebach on his travels in Turkistan, describing the geology of the environs of Ghazni.—The following papers were read :—On the retinal image of the insect's eye, by Prof. S. Exner.—On the orbit of Winnecke's comet in the years 1858–86, by E. von Haerdtl.—On the relation of atmospheric pressure to electricity (sealed), by T. Altschul.

February 17.—The following papers were read :—On some derivatives of cyanamide, by A. Smolka and A. Friedrich.—On morphine, by Zd. H. Skraup and Dr. Wiegmann.—On the definitive determination of the plane of polarization, by the late L. Kudelka.—On an anomaly of Mendeleeff's periodic system (sealed), by B. Brauner.—On marine Hydrachnida, with some remarks on Midea (Bruz), by R. von Schaub.—On the passage of electricity through bad conductors, by H. Koller.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Egeson's Weather System of Sunspot Causality : Charles Egeson (Sydney, Turner and Henderson).—The Chemistry of Photography : R. Meldola (Macmillan).—First and Fundamental Truths : J. McCosh (Macmillan).—British Dogs, No. 30 : H. Dalziel (U. Gill).—The Dentists' Register, 1889 (Spottiswoode).—The Medical Register, 1889 (Spottiswoode).—A Treatise on Manures : A. B. Griffiths (Whittaker).—Argentine Ornithology, vol. ii. : P. L. Sclater and W. H. Hudson (Porter).—Encyclopædia Britannica, ninth edition, index (Edinburgh, Black).—By Leafy Ways : F. A. Knight (Stock).—An Elementary Text-book of Applied Mechanics : D. A. Low (Blackie).—Journal of the Scottish Meteorological Society, third series, No. 5 (Blackwood).—Deutsche Ueberseeische Meteorologische Beobachtungen, Gesammelt und Herausgegeben von der Deutschen Seewarte, Heft 2 (Berlin).—Journal of the Chemical Society, April (Gurney and Jackson).—Geological Magazine, April (Trübner).—Mind, April (Williams and Norgate).—Himmel und Erde, April (Berlin).

CONTENTS.

THURSDAY, APRIL 18, 1889.

A CHEMICAL "WRECKER."

Chemical Lecture Notes. By Peter T. Austen, Ph.D., F.C.S., Professor of General and Applied Chemistry, Rutgers College, and the New Jersey State Scientific School. (New York: John Wiley and Sons, 1888.)

THE work of a teacher of chemistry is becoming more difficult and more perplexing every day. The mass of facts of primary importance both to the science and to technology is now so great that the amount of time that can reasonably be devoted to the business of lecturing during an ordinary College course is wholly inadequate to overtake them. The chemical student of to-day is naturally expected to have a wider range of knowledge, and a far higher standard of acquirement, than his brother of five-and-twenty years ago. The wonder is, that one small head can carry all that he is required to know. One inevitable result of this mass of material is seen in the specialization, both in work and in teaching, which is becoming increasingly apparent. There are chemists to whom the chemistry of the carbon compounds is rapidly becoming a sort of "Dark Continent," and who begin to regard the intricacies of a structural formula with much the same feelings as they would look upon the tangled vegetation of a jungle; and, on the other hand, there are men to whom the sesquipedalian names of organic chemistry are as familiar as household words, but who are oblivious to the most ordinary facts of mineral or physical chemistry. Specialism is of course inevitable. The field is far too big to be ranged over by one man if he means to do his fair share of the work of cultivating it. But the question remains, What to teach, and how to teach it? The truth is, that as chemistry is too frequently taught to-day, the facts obscure the view of the principles. We pile up the deck-load when we ought to jettison half the cargo. What we want is, a stricter subordination of facts to principles. We need to import the methods of the statistician into our procedure. Could anything be more deadly dull, or intellectually more depressing, than the courses of so-called "advanced chemistry" professed in some of our Colleges, in which the only stimulus to mental exertion on the part of the teacher and the taught comes from the spur of the inevitable examination at the end? Not one teacher in ten seems to recognize that his first duty is to be interesting. His first duty, he will tell you, is *to pass* his men; and as our systems of examination are at present ordered, the passing is more a question of the facts than of the principles. And yet no one who has listened to the lectures of such men as Liebig or Hofmann or Victor Meyer can doubt for a moment that the teaching of even the most "advanced" chemistry is capable of affording a high intellectual enjoyment. But then, such men are not the slaves of a Syllabus; they are not held in bondage in Burlington Gardens. They are free to develop their own methods and to stamp their own individuality on their work. The revolt in the *Nineteenth Century*, the other day, might have been more successful if it had been more judiciously fought. The chemist who knows, can afford to smile at Mr. Frederic Harrison's sneer at the value of the knowledge of the number of the isomeric amyl alcohols. Of course, the bare fact of the number is not of cardinal importance, but it is evidently not given to Mr. Harrison to know all that is implied by that fact. In this respect at least, Mr. Harrison is a degenerate disciple. The Master's knowledge of chemistry was not bounded by the limits of a volume in the "International Scientific Series": Comte had dabbled sufficiently deep in the science to have appreciated the real worth even of the fact, could he have lived to acquire it. But, although Mr. Harrison may shoot badly, the circumstance that he should have gone to the barricades at all is significant; and every teacher who has a soul above that of a crammer must share in his growing impatience with the present condition of things.

Now there is no doubt, if we may judge from his book, that Prof. Austen would also gladly range himself behind the barricade if Mr. Knowles would only enlist him; but whether his shooting would be of any use, is, as we proceed to show, very questionable.

Dr. Austen writes, as he tells us, for those students who study, "not merely to pass, but to know." His book is not intended to be a text-book: it is simply a collection of notes and observations on topics which his experience as a teacher has shown "often give the student more or less trouble." No attempt has been made "to include all the rocks and shoals on which the chemical student may get wrecked." In short, the book is an attempt to deal with the philosophy of chemistry rather than with the facts; and as such it seemed to us, in view of the ideas to which we have attempted to give utterance at the beginning of this notice, to merit careful examination.

We have, first, a short introduction, in the style of certain well-known lectures, "adapted to a juvenile auditory," which we associate with the classic shades of Albemarle Street. Dr. Austen's sentiments concerning the functions and duties of a teacher are admirable, but they have just as much originality as his attempts to write in the manner of Dr. Tyndall. The introductory paragraphs are, indeed, characteristic of the rest of the book. We have much loose statement, and many faults of expression and of taste, with, now and again, a shrewd practical remark, which Mr. Harrison's clever young examinee, with the "marvellous *flair*," would certainly "spot," like the vulture he is said to resemble. But there is not a single original observation in the work, nor a single new experiment. The iron-filings and the sulphur do their time-honoured duty in illustrating the nature of chemical change, and Hofmann's well-known examples of the volumetric relations of the simple and compound gases are duly set forth, but with nothing of the *verve* of that great expositor. We admit that it is difficult to find anything better in illustration of these particular points; but then, what is the *raison d'être* of Dr. Austen's book? In dealing with such matters as Molecules and Atoms, Valency and Atomicity, Dr. Austen is clearly out of his depth, or rather he is a sufficiently good swimmer to get over the surface indifferently well—but with neither the skill nor the hardihood to get down to the pearls below. He is befogged by the smoke-rings, and muddled among the vortex-atoms. At times he seeks to be interesting, but with what success the following extracts may serve to show.

In the section on Substitution or Replacement we read: —

"It takes but a slight imagination to see in many reactions analogies with the diplomatic methods of human affairs. Thus the reduction of potassium dichromate to chromic chloride in the presence of alcohol and hydrochloric acid, is particularly suggestive of what in politics is called a 'deal,' or in diplomacy an 'understanding.' ... So again, in the case of bleaching by moist chlorine, the reaction $H_2O + Cl_2 = 2HCl + O$ becomes possible if there is some substance to take up the O. Otherwise there is no reaction, as O refuses to be put out and allow H to dissolve their partnership, and unite with Cl, unless provided for. But if an easily-decomposed organic substance can be procured, the reaction can be consummated. The colouring matter is attacked by the O, and the H and Cl unite, all participants being apparently satisfied."

Now if truth be the test of real humour, it follows that this humour, if humour at all, is false. At best it is but a tawdry imitation of the style of "Die Verlobung in der Bleikammer," which Dr. Austen seems to admire.

In a lecture on the Nature of Affinity we are told that "no word has been more abused than 'affinity.' Used in the sense to denote the tendency or desire to combine, the word is proper, and in its right place." And this is how Dr. Austen illustrates the "proper" use of the term:—

"$HBr + Cl = HCl + Br$.

"Assume that H represents a young lady from the High School, Br a bore, and Cl a College student, and the mechanism of the reaction becomes at once apparent." The parable is probably intended to indicate a "rock" or a "shoal" on which the chemical student may get wrecked if he does not take care. Indeed, the too-susceptible Dr. Austen cannot keep the "fair sex" out of his pages. The following burst of eloquence is culled from a lecture "On the Chemical Factor in Human Progress" (p. 62):—"To what an extent is the fair sex indebted to the humble chemist! We have given them the wonderful aniline colours, which would put Solomon in all his glory to shame, and compel the peacock to hide both his head and his tail in bedazzled desuetude." "Bedazzled desuetude" is not bad, and Dr. Austen has prudently copyrighted it, with the rest of the work. We take breath and read on:—"We can make artificial blushes, heart palpitations, and alabaster complexions, ... we can change raven locks to tresses of glinting gold, or *vice versâ;* we can supply eyes of any shade of colour, as well as any number or kind of teeth. In fact, the chemist is to the girl of the epoch what the lamp was to Aladdin—only more so." The "girl of the epoch" is doubtless properly grateful, and we hope "the man of the time" is equally so, when he knows that his "overcoat may have in it the remains of ball dresses and prison shirts. It may have laid on luxurious beds, or in the gutter, or both."

Dr. Austen proceeds to inquire, "what is the effect of the increasing accuracy in chemical analysis upon our civilization. Chemical analysis is the balance-sheet of trade. It establishes responsibility. It says to the cheat, honoured and respected though he may be in the community, and there are countless thousands of them, 'Thou art the man!' It compels accuracy of statement—rigid truth" After this astonishing example of "accuracy of statement," the reader will be prepared for anything, and we might give him a dozen instances of the same kind of "rigid truth" if it were worth while to spend more time on this silly production. We are only too thankful that this uncertificated chemical pilot has not attempted to include more of the rocks and shoals on which the student may get wrecked.

The whole book is a jumble of feeble sentiments, false statements, and mischievous reasoning, thrown together with no attempt at order or connection. The spirit of prophecy was surely on him when Dr. Austen wrote this brilliant passage in "The Chemical Factor," &c. (p. 56): "Tangle your cord ever so intricately, the chemist will cut it at a single stroke, and will prick the bubble of your silly babble." The metaphors are mixed, but the meaning is clear.

In concluding this notice of a book about which it is impossible to say a single good word, we are sufficiently mindful of the Laureate's saying about the satire which has no pity in it to offer Dr. Austen the grain of comfort which is contained in his own statement (p. 48): "The more the world abuses you, the more reason you have to suppose that you have evolved an idea that has some claim to be original."

THE BEST FORAGE CROPS.

The Best Forage Crops. By Drs. Stebler and Schröter. Translated by A. N. McAlpine, B.Sc. Lond. (London: David Nutt, 1889.)

DR. STEBLER'S well-known work, "Die besten Futterpflanzen," has found a translator into English. A French translation is also before us, so that it is now available to everyone who speaks any one of the three great languages of the civilized world. It may be described as a complete account of the herbage which constitutes permanent pasturage and rotation grasses. The precise technical value of the word *Futterpflanzen* we do not attempt to give, although forage or fodder crop appears to be the correct English equivalent. An English agriculturist would, however, be somewhat surprised to find in a work dealing with the best forage crops no mention made of what are looked upon in this country as the best fodder crops. We are accustomed to rank such crops as vetches, rape, winter rye, trifolium, kale, and cabbage, as among the forage crops; and even swedes, mangel, carrots, and parsnips, although separately classed as root crops, would not be improperly included in the same designation. By fodder crops we usually mean plants cultivated for their leafy herbage for forage, and "grass" and "hay" would come under the designation. Dr. Stebler's work deals exclusively with these last sections of the class fodder crops. We therefore consider the title of this work, as rendered into English, too ambitious, as it is scarcely a "complete account" of the best-known forage plants, but an exhaustive treatise upon some of the less-known ones, most of which are included by English farmers under the term pasture or meadow grasses.

These remarks apply to the title only, and are not to be considered as detracting from the value of the book, which is unquestionably very great. The number of facts

and the variety of authorities quoted are extraordinary, and we welcome this addition to the literature of grasses as most opportune. The work is practical as well as theoretical. It deals with hay-making and ensilage, impurities and adulteration, preparation of the soil, time for sowing, quantities of seed, and judging the quality of seed. It also supplies accurate botanical descriptions, describes varieties, and treats of geographical distribution, growth, and development. So far as the list of plants described is concerned, the work is complete; so far as it purports to treat of the "best forage plants," it is incomplete, and ought either to have been preceded, or be followed, by another. As a botanical account of various members of the Gramineæ and Leguminosæ, the book is exceedingly useful. It must, however, be borne in mind that a large proportion of the grasses described are unimportant economically, and are therefore not to be ranked as among the best fodder crops. Take, for example Yorkshire fog (*Holcus lanatus*), false oat grass (*Arrhenatherum elateus*), yellow oat grass (*Avena flavescens*), sweet vernal grass (*Anthoxanthum oderatum*), reed canary grass (*Phalaris arundinaceæ*), upright brome grass (*Bromus erectus*), awnless brome grass (*Bromus inermis*), kidney vetch (*Anthillis vulneraria*), and goat's rue (*Galega officinalis*). These plants are not of any agricultural importance, and yet they occupy about one-third of the attention of the reader.

Dr. Stebler's book is, in fact, a study of pastoral plants rather than of agricultural crops, and its chief students will be found among seedsmen and those who are abandoning agriculture for the simpler processes of pastoral life.

As a manual of grasses it is exhaustive, so far as the plants selected for description are concerned, but the number described is limited, and the order in which they are introduced is curiously unscientific. Why, for instance, is sainfoin interpolated between *Festuca ovina* and *Festuca heterophylla?* and with what object is Alsike clover pitched in between rough-stalked meadow grass and sheep's fescue, and separated from red and white clovers by a number of true grasses?

Somewhat significantly, the first position in this standard work is allotted to perennial rye-grass, and in the light of the present controversy upon the value of this grass it is interesting to know what so high an authority as Dr. Stebler has to say upon its value and permanence. Speaking of it, he says:—"At times over-estimated, at times depreciated, it is yet one of the most valuable grasses. It is more a 'bottom' than a 'top' grass. For pastures on heavy soils it cannot be surpassed. In marshy districts where the soil is good, it forms a large proportion of the herbage, so much so that in such cases experienced agriculturists use only perennial rye-grass and a little white clover. Its duration depends very much on the nature of the soil and the climate: on dry, light soils it disappears after the second year, whilst in moist climates and on good heavy soils, it will persist for seven years or even longer." The word perennial must always be used in a qualified sense, as no grass, unless it be gifted with immortality, can actually be so described. The individuals perish, but their place is taken by fresh generations, and in is sense rye-grass may be considered as a lasting ingredient in pastures. Its occurrence on village greens, where it cannot seed, meadows, and town moors, as well as in almost all pastures, and especially those which are closely grazed, are evidences that rye-grass can hold its own for an indefinite length of time. Our own climate is especially suited to it, especially when the soil is of a heavy nature and of good quality. Dr. Stebler recommends 5 per cent. as the maximum amount in a mixture required for permanent pasture, but on this point we must differ from him. He, in fact, is not quite consistent, as he tells us that 80 per cent., with white clover to complete the mixture, is used for forming the excellent pastures on the alluvial flats of Northern Germany.

In the discussion now occupying the attention of agriculturists as to the value of perennial rye-grass, both sides may adduce arguments based on Dr. Stebler's opinion, but a perusal of the work will show that the relative permanence of the various grasses described is not pointed out with clearness. The subject is, indeed, beset with many difficulties, and the chief argument in favour of the true perennial character of rye-grass is its continual presence in closely-grazed pastures.

The illustrations of seeds and the botanical descriptions are among the most useful features of this excellent work. The analyses and chemistry of the grasses are also instructive. The book is evidently written from actual personal knowledge, and the positions of Dr. Stebler as Director of the Seed Control Station at Zürich, and of Dr. C. Schröter, the Professor of Botany, also at Zürich, are guarantees that the work is thorough and trustworthy.

Prof. McAlpine, who has undertaken the arduous work of translation, is the Botanist to the Highland and Agricultural Society. The work has been printed abroad, and the typography is somewhat cramped and the quality of the paper and binding scarcely worthy of the text. A glaring advertisement on the outside of the cover might well have appeared somewhere else than on a work of such standard merit as that of Drs. Stebler and Schröter.

JOHN WRIGHTSON.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873–76, *under the command of Captain George S. Nares, R.N., F.R.S., and the late Captain Frank T. Thomson, R.N.* Prepared under the superintendence of the late Sir C. Wyville Thomson, Knt., F.R.S., &c., Director of the Civilian Staff on board, and now of John Murray, LL.D., Ph.D., &c., one of the Naturalists of the Expedition. Zoology—Vol. XXIX. Published by Order of Her Majesty's Government. (London: Printed for Her Majesty's Stationery Office, and sold by Eyre and Spottiswoode, 1888.)

VOLUME XXIX. contains a Report, by the Rev. Thomas R. R. Stebbing, on the Amphipoda collected during the cruise of the *Challenger;* it forms one of the longest volumes yet issued, consisting of 1774 pages of letterpress, and is illustrated with an atlas of 212 plates.

Of this immense volume, which will always remain a memorial of the patient research and labour of the author, just 640 pages are devoted to the bibliography. It is, perhaps, the most elaborate ever written as a preface to any form of a Report, and meeting one on the first opening of the pages, seems to challenge a few words of comment.

The importance of a bibliography to a student no one will refuse to acknowledge. A list of the works written on the subject of a Report not only shortens the references in the necessary quotations under the genera and species, but, when not required for this purpose, it helps to furnish material for further workers. Whether the list of works published should be arranged under the dates of publication or under the authors' names, alphabetically placed, has been a matter on which differences of treatment have occurred; in the present case they are arranged chronologically. The bibliography begins with the fourth century before Christ, when Aristotle wrote his "De Animalibus Historiæ," and is carried down to the present time, and is illustrated with woodcuts.

Under the date of publication, the author's name, and particulars of his birth and death, are given; these are followed by the full titles of his work, and then comes an analysis of the contents of the work, so far as these relate to Amphipod Crustacea. All of these analyses are of interest; all, or at least most, of them involved great skill and patience in their compilation; and many of them will be of service to future students. But still, it may very legitimately be doubted whether all this enormous mass of details as to the writings of the Milne Edwardses, Spence Bate, Boeck, &c., was necessary in a Report of the nature of the one before us, or whether it be in keeping with the regulations laid down for the guidance of those intrusted with the preparation of these Reports. In the printed suggestions made by the first Superintendent of these Reports, Sir Wyville Thomson, and, to the best of our belief, also approved of by Dr. Murray, was one "that no printing shall be unnecessarily repeated of matter which has already been printed elsewhere." This, it was added, does not apply to the preliminary Reports, leave to print which in various journals had been given.

If the important bibliographical list of Axel Boeck had been brought up to date, the student would have been benefited, and space would have been enormously saved; but the aim of the author seems rather to have been "to produce a record, after the annalistic method, of the progress of knowledge in this branch of natural history"—a very worthy aim, no doubt, and, as far as we can judge, well accomplished, but one outside a Report on the species taken during the *Challenger* cruise.

One hundred and eighty species are described as new, and thirty-one new genera are defined. The author feels sure that our knowledge of the group is still very imperfect, and calls attention to the fact that a few weeks' stay at Kerguelen yielded forty-eight species from this small region, previously supposed to be barren in Amphipods; and still none of the shore-frequenting species, mentioned by Dr. von Willemoes Suhm as "everywhere under stones," were apparently collected, so that still further additions to the Amphipod fauna may be expected.

As to the bathymetrical distribution of the Amphipods, there must be a good deal of uncertainty, as with other groups of animals taken in the trawls and tow-nets; some were undoubtedly taken at or near the bottom, while others were as certainly taken in the surface and subsurface waters: still, there seems some significance in the fact that, of twenty-eight species of the group of the Gammarina, said to be from depths of from 300 to 2300 fathoms, twenty-five genera are represented, of which ten are new, and twenty-six of the twenty-eight species are also new.

Many of the species described as new are as interesting as novel, but the details relating to them do not admit of being particularly referred to. In the introductory remarks, the author states that he believes the two well-known species of Gammarus, *G. pulex* and *G. locusta*, to be the representatives of an ancestral form of Amphipod.

"Far more than any other Amphipod, *Gammarus pulex* appears to have spread itself over the fresh-water streams of the world, and *Gammarus pulex* is connected by the very closest ties with *Gammarus locusta*. It is clear, from the general distribution of the Gammarina, that the chief nurseries whence they issue are the weeds of the coast. From these, the rivers are accessible, as well as the ocean; yet in the rivers the species of Amphipoda are few, while in the ocean they are multitudinous. This admits of a simple explanation, if we accept *Gammarus locusta* as representing the ancestral form which at one time occupied the world without the competition of other species of Amphipoda.

"In order to enable the family to extend its range over the fresh waters of the world, no further change was needed than such as would enable some of the progeny to pass from salt water to brackish, and from brackish to fresh. But, the section of this genus having once obtained command of the rivers, by the capacity of living vigorously in the river water, would have an immense advantage over all rivals attempting in the future to make a lodgment in the stream while their capacity for life therein was in its initial stages and only feebly developed."

There is an atlas of 212 plates, the figures on which have all been drawn by the author in a most satisfactory way. It is to be regretted that there are no detailed descriptions of these plates; perhaps with the 640 pages of bibliography, and sixty of index, this was too much to expect.

OUR BOOK SHELF.

Magnetism and Electricity. By Edward Aveling, D.Sc. (London: Chapman and Hall, 1889.)

As most intending candidates for London Matriculation will be aware, the new regulations affecting the science subjects have been in force since last June. Chemistry and natural philosophy have been replaced by mechanics, which is compulsory, and chemistry, light and heat, or magnetism and electricity, at the option of the student. To meet these new requirements Dr. Aveling has prepared a series of text-books on the specified subjects, of which the book before us is one. The book is of necessity planned on examination lines; and, although the author hopes "that the matter and method may be of service in the introduction of students generally to the subjects considered," we could hardly recommend it to those who do not require it for examination purposes. However, it completely covers the syllabus, and gives accurate, though often scanty, information. In the chapter on "Terrestrial Magnetism," for example, some very useful data are given

but the explanations of the methods of determining them are very meagre. Thus, on p. 22 the declination compass is described, and the explanation given for its use is simply that "the telescope is set in the plane of the geographical meridian, and, as the needle sets in the plane of the magnetic meridian, the angle between the telescope, which always lies over 0° to 180° on the circle, and the needle, is the declination." Again, referring to the determination of dip, it is simply stated "that when the instrument is arranged with the circle in the plane of the magnetic meridian, the angle of inclination can be read off on the circle" (p. 26). We fear that very few students would succeed in getting even approximate values with only these brief statements to guide them if the instruments were put into their hands.

One chapter is devoted to "Examples on Formulæ," which will, no doubt, be of great service to students, although the title is rather suggestive of cramming. The examples given are not less numerous than useful, no less than 52 out of 144 pages being devoted to them. Several of the papers set at previous Matriculation, Science and Art, and other examinations are given.

The book is well illustrated throughout, and although it is more of an epitome of the chief laws and experiments than a text-book, it will, no doubt, be of great service to those for whom it is primarily intended.

Heat and Light. By Edward Aveling, D.Sc. (London: Chapman and Hall, 1889.)

THIS is another text-book of the series referred to in the preceding notice, and follows on the same lines. It is characterized by the same bare outline, the explanations of the methods of determining the various data generally including no suggestion whatever as to difficulties and corrections. This is especially noticeable in the account of Joule's classical experiment (p. 26), in which no mention whatever is made of the corrections for loss of heat due to radiation or for the velocity of the weight on falling. The diagram, too, is seriously wrong, since it simply shows a set of vanes revolving in a vessel of water; without the pierced partitions necessary to prevent the rotation of the water, the experiment is, of course, useless.

The chapter on the composition of white light and the spectrum is perhaps the least satisfactory in the book. The merest outline of the subject is given, and there are two or three very obvious slips. On p. 165, for instance, the electric light and the lime-light are quoted as examples of monochromatic light. and again on p. 166 it is stated that "glowing gases yield spectra with *dark* lines." The idea that the actinic rays are confined to the violet part of the spectrum is rather old-fashioned, and is scarcely likely to be credited by a student who may have happened to experiment in the direction of orthochromatic photography.

Like its predecessor, the book contains numerous examples and illustrations.

The Encyclopædia Britannica. Ninth Edition. Index. (Edinburgh: Adam and Charles Black, 1889.)

THE publication of this volume (of 500 pages) marks the completion of one of the greatest literary undertakings of the present age. As to the necessity for an index there can be no doubt, since, as the editor explains, the plan of the "Encyclopædia Britannica" was that subjects rather than words should be dealt with, and that large subjects should be discussed in a connected way, under general headings, so that the book might be used not only for occasional reference, but for systematic study. This plan was adhered to, and the result is that "many things which a reader may wish to understand are explained, not under their own names, but in the course of a larger discussion." In such cases reference must be made to the index; and this is so full and so accurate that no one who may have occasion to consult it will ever have the slightest difficulty in at once finding what he wants. The index has been compiled by Mr. William Cairns, and arranged and revised by the Rev. George M'Arthur, with the assistance of Miss Emily Stevenson and Mr. J. T. Bealby. The volume contains also a complete list of contributors, with a key to the initial letters affixed to the longer articles. A glance over this list, which includes almost all the foremost writers of the day, suffices to explain the high character of the work as a whole.

Blackie's Modern Cyclopædia of Universal Information. Edited by Charles Annandale, M.A., LL.D. Vol. I. (London: Blackie and Son, 1889.)

IT is intended that the work of which this is the opening volume shall serve as "a convenient work of reference for readers of all classes—comprehensive in scope, handy in size, moderate in price, and generally adapted to the needs of the day." Of course no one who may want to obtain a thorough knowledge of any subject will think of seeking for it in such a work as this; but the editor does not place before himself too high an object of ambition when he expresses a hope that the new Cyclopædia may prove useful to persons who have little time for acquiring information from books in general, though they take an interest in many topics lying outside their own pursuits. The present volume deals with words beginning with the letter A, and with many of those beginning with B. The articles are short but clear, and, so far as they go, accurate. Especial attention has been given to matters which are of living interest in our own day, and we are glad to see that many scientific articles have been written or revised by specialists. The volume contains some good maps and many interesting pictorial illustrations.

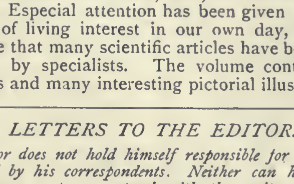
LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Spherical Eggs.

A BIOLOGICAL friend lately asked me for a solution of the problem, How many spherical eggs, 0·03 millimetre in diameter, can be contained in a cubic millimetre?, the whole space occupied by the eggs being large compared with a millimetre. Assuming the eggs as closely packed as possible in a horizontal stratum, their centres will lie at the angular points of a series of equilateral triangles whose sides are equal to a diameter. The number of spheres in this stratum corresponding to a unit of area will thus, on an average, be $\frac{2}{d^2\sqrt{3}}$, d being the diameter.

The next stratum above will have the centres of the spheres placed so that each is at the vertex of a regular tetrahedron,



having alternate equilateral triangles of the lower series as base. Evidently, in a volume of the slice between the two planes of centres, having a unit of area for its base, there will be, on an average, $\frac{2}{d^2\sqrt{3}}$ spheres. But the thickness of the slice is $d\sqrt{\frac{2}{3}}$. Hence, in a unit volume, on an average, there will be $\frac{\sqrt{2}}{d^3}$ spheres, *i.e.* $\sqrt{2}$ times, or about $\frac{7}{5}$, as many as would be contained, assuming their centres at the corners of cubes.

Possibly some of your readers may have considered this problem, and may be able to suggest some other method of packing the spheres. I do not remember to have met with any discussion of it. W. STEADMAN ALDIS.
University College, Auckland, New Zealand, February 4

Temperatures in Lake Huron.

SOME very interesting results observed by Commander Boulton, R.N., on the temperature of the waters of the Georgian Bay, the eastern basin of Lake Huron, have been placed by him in my hands. They appear to establish that the waters of the bottom of the bay are colder than the even deeper waters of the rest of the lake.

Lake Huron in its profound depths forms three great basins—the Georgian Bay, defined along its western outline by the bold cliffs of the Niagara limestones, and the central and southern basins, separated by the subaqueous corniferous escarpment which diagonally crosses the lake in a south-eastern direction from the outline of Lake Michigan. Whilst the southern basin has generally a sandy bottom, and is in many parts comparatively shallow, the central basin has a floor chiefly of clay, and includes the deepest portions of the lake.

The surface temperature necessarily varies with the seasons, and with the continuous or fitful nature of the weather for days preceding the observations. Thus on May 11, 1888, when the ice had but recently broken up, the surface water of the Georgian Bay near Owen Sound registered 34° F., whilst at 15½ fathoms the minimum was 34½° F.

Observations will during the coming summer be continued in this and other lakes, but in the meantime the records given hereunder may be taken as preliminary illustrations of the temperature of the waters of the bay. For comparison, some published observations taken in 1860 by the United States engineers in the central and southern basins are also given.

GEORGIAN BAY.

Lat. 45° 6′, *long.* 81° 7′.
July 27, 1888, 8.30 *a.m.*

	° F.
Surface	60¼
10 fathoms	45¾
20 ,, ...	41½
35 ,, ...	41
66 ,, (bottom)	39½

Lat. 45° 35′, *long.* 80° 49′.
August 20, 1886, 8 *a.m.*

	° F.
Surface	59¾
31 fathoms (bottom)	39½

Lat. 45°, *long.* 80° 52′.
August 20, 1886, 12.38 *p.m.*

	° F.
Surface	65
42 fathoms (bottom)	37¾

CENTRAL BASIN.

Lat. 45° 18′, *long.* 82° 23′.
July 30, 8 *a.m.*

	° F.
Surface	52
65 fathoms (bottom)	42

SOUTHERN BASIN.

Lat. 44° 33′, *long.* 82° 54′.
August 5, 10 *a.m.*

	° F.
Surface	58
38 fathoms (bottom)	52

Lat. 43° 46′, *long.* 82° 1′.
June 20, 9 *a.m.*

	° F.
Surface	55
45 fathoms (bottom)	52

On August 20, 1886, the temperature of the surface rose from 59¾° F. at 8 a.m., to 62° at 9 a.m., 63¾° at 11.34 a.m., and 65° at 12.38 p.m.

The suggestive explanation of the lower temperature of the Georgian Bay depths is that whilst the more southern and warmer waters of Lake Michigan in their course from the inlet to the outlet do not reach the bay, a considerable portion of the colder waters of Lake Superior find their way into it by the channel north of the Manitoulin Islands. Further, the subaqueous cliffs which block the western side of the bay preclude a free circulation between the deeper waters of the bay and the profound depths of the lake beyond. A. T. DRUMMOND.

Will Fluctuations in the Volume of the Sea account for Horizontal Marine Beds at High Levels?

IN the interesting article "On the Gradual Rise of the Land in Sweden" (NATURE, March 21, pp. 488-92), Nordenskiöld arrives at the conclusion that the small alterations of the relative level of sea and land which observation proves have taken place in Sweden, are due to movements of the land, not to fluctuations of the sea-level. On the other hand, he contends that the extensive horizontal stretches of marine strata found in many places on the earth's surface at heights measured by thousands of feet above the sea-level indicate fluctuations of level in the sea itself. This is certainly reversing the order of things as believed in by most geologists. It is also suggested that the fluctuations of sea-level are due to alternate increase and decrease of the volume of the sea, arising from gaseous and fluid additions from outer space or loss thereto, the alternate gains and losses balancing one another over long periods.

It is not my object in this communication to discuss the physical possibility of such alterations of the volume of the sea having taken place in this way, but to point out that, even if granted, such rising and falling of the sea-level fails to explain the geological phenomena for which it is invoked. Formations horizontal in one place are disturbed in another. They cannot be divided into two hard and fast stratigraphically dissimilar kinds of marine deposits, the *horizontal* and the *folded*, as is attempted by Nordenskiöld. Even the example quoted by him of the Tertiary strata of Spitzbergen shows this, as it is stated, "Near the west coast they are much disturbed, but further inland they form horizontal strata of sand and clay, &c."

The plains of Russia are, as was shown long ago by Murchison, largely occupied by nearly horizontal strata of undisturbed Silurians, while in the Ural Mountains the same formation is thrown up on end. I venture to pronounce this continuity of horizontal with disturbed deposits an almost universal phenomenon, for where plateaus are capped by horizontal strata, as often happens, these cappings are only the remnants left by denudation.

It is a well-known geological fact that as strata recede from a mountain range they become less and less disturbed and more horizontal. Again, there are no horizontal strata of any extent or thickness that are not riven with faults showing that they have been subjected to upthrow or downthrow as the case may be, and these have to be accounted for as well as the level at which the strata occur. Except in the very newest deposits, strata bear very little relation to the levels at which they are now found. Because strata are often horizontal at high levels it is no indication that they have not been upheaved. The Colorado plateaus may be cited as an instance, and such instances may be multiplied to any extent.

There is, however, another difficulty appertaining to the explanation offered by Nordenskiöld. It is this, the general rise of the sea-level over the whole globe to the extent even of 1000 feet would obliterate an enormous area of land. Where, then, would the sediment come from to form the beds appealed to in proof of the rise of the sea-level? Formations are not arranged concentrically at varying levels or, in other words, stratigraphically contoured, as would be the case were they due to this cause. But there is a final and still greater difficulty to be met. Denudation is year by year reducing the height of the land, and if no compensatory elevation excepting over disturbed areas took place, continents instead of growing as they are supposed by some to do, would long ago have been obliterated, and the earth planed down to a uniform level, so that when periods of "high water" recurred all terrestrial life would be destroyed. This contingency no doubt to some minds will be plain demonstration of the truth of Nordenskiöld's theory.

T. MELLARD READE.
Park Corner, Blundellsands, near Liverpool, March 25.

The Meteorological Conditions of the Aruwhimi Forest Tract.

I CANNOT but think that the true explanation of the rank exuberance of the Aruwhimi forests, so graphically described by Mr. Stanley, or rather of the humid climate that fosters them, is different from that suggested either by the great traveller himself or the writer of the notice in last week's NATURE. The source of the winds that feed the rainfall of this region seems to me a question of secondary importance, but since in equatorial regions, as a rule, easterly winds predominate, I am inclined to think, with the writer of the article, that this source is most probably the Indian Ocean.

If, however, this be so, since in the interval between the coast and the Aruwhimi basin they have to pass over some of the highest mountains in the continent, and reach the latter on a descending slope, they would be comparatively dry winds, more or less analogous to the Alpine *föhn*, were there not other conditions present which more than counterbalance the desiccating influence of the eastern mountains. The first and most

important of these conditions I take to be the equatorial position of the Aruwhimi basin; the second that it is situated in the heart of the continent. Both of these, but more particularly the former, determine it as the seat of ascending air-currents, and therefore of their dynamic cooling on a gigantic scale, and it is to this dynamic cooling that the high rainfall of the region is to be ascribed.

Very probably a considerable portion of the precipitated moisture is locally re-evaporated, so that, as suggested long ago by Sir John Herschel in the case of the Brazilian forest rainfall, the same water is precipitated again and again. There are not, I believe, in the lower atmosphere, any steady winds blowing outwards to carry away the evaporation of the damp forest tract, and the main loss of water to be supplied by easterly or other winds is that carried off by the river drainage, probably less than half of the rainfall. The air which has ascended to the higher regions of the atmosphere as a part of the main circulation of the globe, parts with nearly the whole of its vapour in the act of ascending.

We have a case in some respects analogous to that of the Upper Aruwhimi in the very damp and equally forest-clad province of Upper Assam. This too is characterized by a very calm atmosphere, being girt with lofty mountains on the north and east, and also shut off on the south and south-west from the Bay of Bengal by hills of considerable elevation. Such gentle winds as blow in the valley are chiefly from the east or down valley. Yet the rainfall is over 100 inches in the year, and the whole tract is one of marsh and dense forest. It is indeed not situated under the equator, and herein it is less favourably conditioned as a region of excessive rainfall than the basin of the Aruwhimi.

As the result of a long study of the rainfall of India, and perhaps no country affords greater advantages for the purpose, I have become convinced that dynamic cooling, if not the sole cause of rain, is at all events the only cause of any importance, and that all the other causes so frequently appealed to in popular literature on the subject, such as the intermingling of warm and cold air, contact with cold mountain slopes, &c., are either inoperative or relatively insignificant.

Folkestone, April 11. HENRY F. BLANFORD.

"Les Tremblements de Terre."

M. FOUQUÉ'S letter (NATURE, March 28, p. 510) does not meet the main points of my criticism of his book. He thinks that a pendulum swinging in synchronism with the ground's motion is the right thing to use as an absolute seismometer. M. Poincaré's mathematical note, to which he refers as supporting his view, does not support it, but shows why such a pendulum is unsuitable. It is necessary to emphasize this, for it relates to a fundamental matter in the dynamics of earthquake measurement—a matter on which the work done of late years in Japan seems to me to be so intimately based that a misunderstanding about it must be fatal to a proper appreciation of that work. And, in point of fact, I did not find that M. Fouqué gave an appreciative account of what any of the Japanese observers had done. As to his mention of Prof. Ewing's seismograph, in particular, I criticized it not so much because it was meagre as because it was incorrect,—so incorrect as to justify the inference that the author was not acquainted with that instrument.

THE REVIEWER.

Hertz's Equations.

MR. WATSON'S criticism, that Hertz's equations are only true for places at some distance from the oscillator, is no doubt perfectly valid. [There is, by the way, an insignificant and obvious misprint of λ for ρ about the middle of his letter.] But this was entirely recognized by Hertz himself; he treated the oscillator as infinitesimal, knowing that it was nothing of the kind when you got near it, and refrained from drawing his diagram-curves into its neighbourhood, for this very reason.

The fact is surely that, to work out completely the case of electric oscillators in a compound body formed of a couple of spheres joined by a cylinder, would tax the resources of a strong mathematician; and it is impossible that the vibration can be, in any sense, a pure one; all manner of sub-vibrations must be superposed upon the main.

From the physical point of view, some general notion of what was happening at a distance of a wave-length or more from the oscillator was desirable, and this Hertz satisfactorily obtained.

But, to work out what is happening in the immediate neighbourhood of a dumb-bell oscillator must be left, I imagine, to the time when some pure mathematician may devote his attention to this particular shape of conductor, if the case appears to him of sufficient interest. At present I see no special reason why it should be so regarded, but of that Mr. Watson is a better judge. I hope he may see fit to attack the problem.

Grasmere, April 13. OLIVER J. LODGE.

THE COMPRESSIBILITY OF HYDROGEN.

AS stated in the obituary notice that appeared in NATURE (vol. xxxviii. p. 598) at the time of the melancholy accident which caused his death, Wroblewski was engaged in an investigation of the behaviour of hydrogen on compression. The results of this investigation, as far as it had then advanced, have now been made public (*Monatsh. für Chem.*, 1888, p. 1067 *et seq.*). They are of a most important and interesting nature, and form a fitting memorial of the patience and skill of the observer, who most unhappily was not spared to bring this, the last and most complete of a long series of similar investigations, to a close.

Hydrogen has long occupied an exceptional and isolated position among gases. This is due to the fact that, as Regnault first pointed out, hydrogen forms the sole exception to the law that the product of the pressure into the volume, pv, of any gas decreases with increasing pressure,—the exact converse being true in the case of hydrogen, this product showing a regular increase. It is true that, as since shown by Amagat and others, this behaviour of hydrogen becomes general for all gases when the pressure is increased beyond a certain limit, but before reaching this limit the product pv invariably decreases until a minimum is reached for all gases with the exception of hydrogen. For hydrogen neither the decrease nor the minimum have yet been observed, the gas as hitherto examined showing an invariable increase of pv with increasing pressure. The natural inference was, however, that the exception was only apparent, and that the minimum above noted would be found to occur also with hydrogen if the gas were examined at lower pressures than those hitherto investigated—that is to say, at pressures below one atmosphere. But a difficulty in the way of this hypothesis arises from the fact that the critical pressures of all gases are found to be *below* the pressure at which the minimum value for the product of pressure into volume occurs, and therefore on the above reasoning the critical pressure of hydrogen would have to be phenomenally low and considerably beneath one atmosphere.

To gain a further insight into the relation of volume to pressure in the case of hydrogen, Wroblewski decided to investigate this relation through a wide range of temperature. For this purpose he selected as temperatures sufficiently apart, the boiling-point of water, 100° C., the melting-point of ice, 0° C., the boiling-point of liquid ethylene, $-103°.5$ C., and the boiling-point of liquid oxygen, $-183°$ C. The pressures employed varied from one to seventy atmospheres.

The method of experimenting was exceedingly simple. The gas at a known pressure was forced into a bulb of known capacity having a capillary neck, and kept at one of the above four temperatures. A sufficient length of time was allowed for the gas to attain the fixed temperature; it was then transferred to a eudiometer, and its volume measured. It is needless to add that every precaution was taken both in purifying the gas and in applying the necessary corrections.

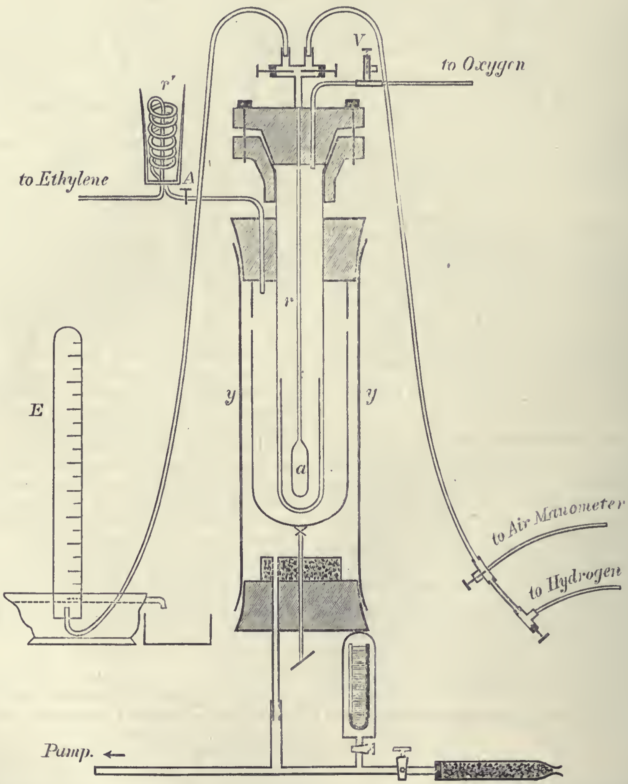
The results with the three first of the above temperatures agree with the behaviour of hydrogen already observed, the product of volume into pressure constantly increasing with the pressure. It was found that for the range of pressures under investigation (one to seventy

atmospheres) the relation between these two quantities may be expressed in all three cases by the general equation $pv = a + bp + cp^2$. The curves which this equation represents have their concave side towards the axis of p. Between the temperatures $-103^\circ\cdot5$ and 100°, then, nothing is observed that contradicts former experience with regard to hydrogen on compression.

But at the temperature of boiling oxygen, -183°, the behaviour of hydrogen is very different. Up to a pressure of about fourteen atmospheres the product pv decreases with rising pressure, and the above equation no longer holds good. At about fourteen atmospheres, however, this decrease has reached a minimum, and from this point the gas behaves in the ordinary way, and the product pv increases with the pressure in accordance with the equation given above. Not the least sign of liquefaction occurs at this temperature at any of the pressures employed.

This is an observation of great importance. It shows that at sufficiently low temperatures hydrogen behaves on compression like all other gases, and has a minimum value of pv occurring, not at some exceptionally low pressure, but at a pressure of fourteen atmospheres.

For the general representation of the four isothermals thus obtained, Wroblewski employs an empirical formula that differs but slightly from one given by Clausius—



In the sketch given of the apparatus used when the gas was cooled by means of liquid oxygen, a is the bulb in which the compression of the hydrogen takes place. The vessel containing this is filled with oxygen under pressure and surrounded by ethylene under evaporation, liquid ethylene being obtained by passing the gas already cooled by a mixture of ice and salt through a worm tube immersed in r' in a mixture of solid carbon dioxide and ether. By suddenly diminishing the pressure on the oxygen by opening the tap V communicating with the outside air, the gas is partially liquefied and the desired temperature is reached. The volume of the gas in a is afterwards measured by allowing it to escape into the eudiometer E.

$$p = \frac{RT}{v - a} - \frac{K}{\epsilon^T v^2},$$

the values of the constants being in the present case R $= \frac{1}{273}$, $a = 0\cdot00111665$, K $= 0\cdot00051017$, and $\epsilon = 1\cdot003892$. This equation serves for the calculation of the critical temperature, pressure, and volume of hydrogen, for which the values are found to be—

$$\theta\epsilon^\theta = \frac{8K}{27Ra}, \ \pi = \frac{RT}{8a}, \text{ and } \phi = 3a,$$

or, substituting the values for the constants, we get—

Critical temperature $\theta = 32\cdot6$ or $-240^\circ\cdot4$ C.,
,, pressure $\pi = 13\cdot3$ atmospheres,
,, volume $\phi = 0\cdot00335$,

from which

Critical density $= 0\cdot027$.

Similar results were obtained by employing other formulæ than the one given above, but this was retained as being in best agreement with the experimental results.

These numbers afford at once an explanation of the ordinary behaviour of hydrogen on compression and the

absence of the minimum of pv at ordinary temperatures, this being due to the fact that hydrogen possesses a very low critical temperature combined with a low critical pressure. For if, instead of taking temperature and pressure in the ordinary units, we take temperature in terms of the critical temperature, and pressure in terms of the critical pressure as units, and then with temperatures as abscissæ and pressures as ordinates construct the curve for the observed minimum points of pv, this curve will be found to be one and the same for all gases. As Wroblewski has shown, it is a continuation of the curve for the vapour pressures at different temperatures of the liquefied gases, pressure and temperature being expressed in terms of the critical. Drawing this curve by the aid of the observations that have already been made with ethylene, carbon dioxide, methane, and other gases, it is found that the pressure for the minimum point rapidly rises with rising temperature, and reaches a maximum of about 3π for a temperature of $1{\cdot}4\theta$. From this point, however, the pressures decrease as the temperature rises, so that, when the temperature is about 3θ, the pressure at which the minimum pv occurs is π, or the critical pressure; and if the temperature be further increased beyond this point the pressure of minimum pv is reduced below the critical, and continues still further to fall as the temperature rises.

Applying this to hydrogen at a temperature of about 3θ, the pressure of minimum pv should be π, the critical pressure, and this as a matter of fact agrees with the result of the above experiments. For 3θ is approximately $-176°$ C., at which point the required pressure should be the critical or 13·3 atmospheres, and observation shows that at $-183°$ C. this pressure is fourteen atmospheres, and as, the temperature being lower, we should expect the pressure to be somewhat higher, this is a very close approximation indeed. At higher temperatures the pressure falls below the critical, and this evidently takes place so rapidly that at $-103°$ C., or for about 5θ, the pressure of minimum pv is so small a fraction of the critical as to be removed outside the range of observation. The critical pressure itself being low of course assists this process.

This, then, explains the behaviour of hydrogen on compression, and why this behaviour differs from that of the other gases. For a gas must be raised to a temperature of over 3θ before it will act like hydrogen, whereas gases have up to the present only been examined at temperatures not far removed from the critical.

The very low critical temperature of hydrogen is remarkable. It confirms the saying of Regnault, that hydrogen at ordinary temperatures is a gas *plus que parfait.* At the same time it shows that no reliance can be put on Pictet's statement that hydrogen was liquefied at about $-140°$ C. under a pressure of 360 atmospheres; and whether the temperature attained in Cailletet's experiments was low enough to actually liquefy the gas must be looked upon as extremely doubtful. At the time of his death Wroblewski was planning experiments for the liquefaction of hydrogen, the only thing necessary to make his work complete.

By cooling hydrogen under a pressure of 110 atmospheres to $-213°{\cdot}8$ C. by the evaporation of liquid nitrogen, and then suddenly diminishing the pressure, as low a temperature as $-223°$ C. was obtained, but the hydrogen still remained in the gaseous state and refused to liquefy.

H. CROMPTON.

THE MANATEE.

THE Zoological Society have added to their living collection in the Regent's Park a young specimen of the Manatee (*Manatus americanus*), which those who wish to have an opportunity of inspecting an extremely curious form of Mammalian life should take an early opportunity of visiting. The Manatees belong to the order *Sirenia* of naturalists, and are sometimes called "herbivorous Cetaceans," although it is, to say the least, very doubtful whether they have any near relationship whatever to the true Whales or order Cetacea. These creatures were abundant in former geological epochs, but since the extermination of the *Rhytina*, or Steller's Sea-cow, at the latter part of the last century, have only two representatives still living on the earth's surface, viz. the Manatee of America and Africa, and the Dugong of the Indian Ocean.

As will be seen by our outline sketch, taken from the Zoological Society's Proceedings, the Manatee is shaped more like a fish than an ordinary mammal. It is formed exclusively for aquatic life, and inhabits the estuaries and rivers of the American and African continents, where it passes its time browsing on the plants beneath the surface and adjacent to the banks. It remains mostly under the water, and only raises its head above the surface to breathe.

The present specimen, which arrived at the Gardens from Liverpool on March 2, is the second example of this singular form that has been received alive by the Society. The first specimen was acquired in August 1875, and lived about a month in the Regent's Park, where it attracted many visitors. When dead, however, it was by no means wasted, as it formed the subject of an excellent article on its anatomy read before the Zoological Society by the late Prof. Garrod, and subsequently published in the tenth volume of the Society's Transactions.[1]

In 1879, a pair of Manatees, received from the Island of Trinidad, lived for several months in the Brighton Aqua-

[1] A complete account of the anatomy of the Manatee was prepared by Dr. Murie in 1872, from the examination of a specimen of this animal imported from Surinam for the Zoological Society, which unfortunately only lived just long enough to reach England. This will be found in the eighth volume of the Zoological Transactions. The attention of residents on the Amazons should be called to the existence in that river of a second species of Manatee (*Manatus inunguis*), discovered by the great Austrian naturalist, Johann Natterer, in 1830, but as yet little known in Europe.

rium, and were the subject of some very interesting notes on their habits, prepared by Miss Agnes Crane, and published in the Zoological Society's Proceedings, from which the following particulars are extracted:—

"Lettuces and endives formed the favourite food of this pair of Manatees; six dozen of these vegetables, weighing 30 pounds, being their average daily allowance. The male would devour at a pinch leaves of the cabbage, turnip, and carrot. Both relished those of the dandelion and the sow-thistle (*Sonchus oleraceus*). Some varieties of a common river-weed were also taken; but this food was abandoned on account of the leeches with which it was found to be infested Sometimes the animals swim gently about, and pursue the leaves floating on the water. At others, the plants are seized in their mouths, drawn down, and eaten under water, the hand-like fore-fins being employed in separating the leaves The food is invariably swallowed below the surface. The masticatory actions of the animal have been so fully and accurately described by Prof. A. H. Garrod, F.R.S. (Trans. Zool. Soc., vol. x. p. 137), that further remark on that subject is unnecessary. The habits of the animals in captivity, while affording occasional evidence of the ease and rapidity with which they move in the water, do not furnish much support to the views of their capability of habitual active progression on land. Yet it must be admitted that, supplied with a sufficiency of nicely-varied food, they have no inducement to leave the water, and that the construction of their straight-walled tank precludes such efforts as a rule. The male, however, has recently been observed to make some slight attempts at terrestrial movement, turning himself round and progressing a few inches when his tank was empty. With jaws and tail-fin pressed closely to the ground, the body of the animal becomes arched, and is moved by a violent lateral effort, aided, and slightly supported, by the fore-paddles, which are stretched out in a line with the mouth. But the effect of these very laboured efforts was not commensurate with their violence; in fact, their relation to active locomotion may be compared to those of a man lying prone with fettered feet and elbows tied to side. Nor does the Manatee seem at all at ease out of water, as he lies apparently oppressed with his own bulk, while he invariably makes off to the deepest corner of his tank directly the water is re-admitted. One point may be regarded as definitely settled. Notwithstanding the predilection they have evinced for land vegetables, they never feed out of water. Food has been repeatedly offered them, but it always remains untouched, although readily devoured when the influx of water set the leaves floating on the surface. Although it is possible that the animals can get out of water and remain so for a short period, as they progress so slowly and do not feed out of water it seems as though they must be acquitted of the garden depredations and prolonged wanderings from their native element with which they have been credited."

The Manatee now in the Zoological Society's Gardens has been placed in one of the large warm-water tanks in the Reptile House, the corresponding tank on the opposite side being occupied by a very fine specimen of the Snapping Turtle (*Macroclemmys temmincki*).

After inspecting the Manatee, those who wish for more information on the subject of the Sirenians should visit the Natural History Museum at South Kensington, and examine the splendid mounted skeleton of the *Rhytina*, or Steller's Sea-cow—a recently extinct gigantic representative of the same order of mammals—in the Palæontological Gallery.

THE ROYAL SOCIETY SELECTED CANDIDATES.

THE following fifteen candidates were selected on Thursday last by the Council of the Royal Society to be recommended for election into the Society. We print with the name of each candidate the statement of his qualifications:—

JOHN AITKEN.

He is an accurate, successful, and highly inventive investigator in Experimental Physics, and for the purpose of his investigations has designed and constructed many ingenious and valuable pieces of apparatus. Has carried out a valuable and instructive research regarding the relations between fog and dust in air, and in connection therewith has devised and used methods for counting the number of dust particles in air. The results of his works were given in numerous papers read by him before the Royal Society of Edinburgh, and published in the Proc. and Trans. Roy. Soc. Edin., during the years from 1875 until the present date. Received from the Roy. Soc. Edin. the Keith Prize for 1883–85, for a paper on "The Formation of Small Clear Spaces in Dusty Air" (1884), being a continuation of the subject of a former paper "On Dust, Fogs, and Clouds" (1880), and for contributions on atmospheric phenomena, the more important of these being a series of papers on "Thermometric Screens" (1884–87).

EDWARD BALLARD, M.D. (Lond.),

Physician in H.M. Civil Service. Eminently distinguished as an Investigator of Causes of Disease, and as a promoter of scientific sanitary administration. Has published, *inter alia*, as follows:—On the Influence of Weather and Season on the Public Health, based on the Statistical Study of 272,000 Cases of Sickness (1857–68); On Vaccination and its Alleged Dangers (1868); A Local Outbreak of Enteric Fever traced to a Local Distribution of Milk (1871); Reports to the Local Government Board, year by year, on particular inquiries, local, or more or less general (1871–88). Of the latter the following may be named:—The Effluvium Nuisances which arise in various manufacturing and other branches of industry, studied as to their Effects on the Public Health, and as to their Preventability (1876–78); various Studies of Outbreaks of Disease referable to Articles of Food (1873, 1880–88); Survey of England as to the Local Preparations against Chlorea (1885-86): Studies of the Causation of Epidemic Infantine Diarrhœa in England and Wales (1882-88).

ALFRED BARNARD BASSET, M.A.,

Barrister-at-Law. Author of a treatise on Hydrodynamics, in two volumes (1888); also papers in the *Quart. Journ. Math.*, *Mess. of Math.*, *Phil. Mag*, Proc. Lond. Math. Soc., Proc. Camb. Phil. Soc., Phil. Trans. Roy. Soc. (1888), and *Amer. Journ. Math.* These papers treat of the Motion of Liquids about Elliptic Cylinders, of Associated Functions and Spherical Harmonics, of Electric Currents of a Sheet rotating in a Field of Magnetic Force, the Potential of a Spherical Bowl, Motion of a Liquid Ellipsoid and Stability of this Motion, Motion of two Spheres in a Liquid, Potentials of Circular Disks, Motion of a Ring in a Liquid, Motion of a Sphere in a Viscous Liquid, and the Steady Motion of an Annular Mass of Rotating Liquid.

HORACE T. BROWN,

F.C.S., F.I.C., F.G.S., Brewer. Distinguished as an investigator of the Carbohydrates, and of the Phenomena of Fermentation; also on account of the services he has rendered in introducing scientific methods into the brewing industry. Author of the following and other papers:—"On the Estimation of Ammonia in Atmospheric Air" (Proc. Roy. Soc., 1870); "On the Influence of Pressure on Fermentation" (Journ. Chem. Soc., Part I., 1872, Part II., 1873); "On the Electrolysis of Sugar Solutions" (*ibid.*, 1872). In conjunction with Mr. Heron:—"Contributions to the History of Starch and its Transformations" (*ibid.*, 1879); "Some Observations on the Hydrolytic Ferments of the Pancreas and Small Intestine" (Proc. Roy. Soc., 1880). In conjunction with Dr. Morris:—"On the Non-crystallizable Products of the Action of Diastase upon Starch" (Journ. Chem. Soc., 1885); "Determination of the Molecular Weights of the Carbohydrates" (*ibid.*, 1888).

LATIMER CLARK,

C.E., F.R.A.S., F.R.G.S., Electrical Engineer. Past President of the Institution of Electrical Engineers. Author of:—"Description of the Britannia and Conway Tubular Bridges;" chapter on "Tides of the Menai Straits," in E. Clark's book on

the Britannia and Conway Bridges; "Elementary Treatise on Electrical Measurement;" and (jointly with R. Sabine) "Electrical Tables and Formulæ." Author of various papers on Electric Measurement, and on various branches of Engineering Science, published in Report of the Brit. Assoc., the Government Report on Submarine Cables, and the Engineering Journals. Introduced a standard voltaic cell of great importance and value for promoting accurate measurement of electric potentials, and presented a paper on this subject to the Royal Society, which was read June 19, 1873. From 1848–51 was Resident Assistant Engineer at the Britannia Tubular Bridge, under the late Robert Stephenson. In 1851 became Engineer to the Electric and International Telegraph Company, and remained in this service for 20 years, part of the time as Engineer-in-Chief. Made important observations on the passage of electricity through long underground lines, of which the results were the subject of Faraday's Bakerian Lecture of 1854, and are given in his "Experimental Researches," with Faraday's own experiments and theory. In 1854 introduced the system of transmitting messages through "pneumatic-despatch tubes" in the Electric Telegraph Company's service. The system is continued in the Postal Telegraph system of the Government, having been found admirably successful and useful.

DAVID DOUGLAS CUNNINGHAM, M.B., C.M. (Edin.).

F.L.S., Surgeon-Major, Beng. Med. Service, Honorary Surgeon to the Viceroy of India. Professor of Physiology, Medical College, Calcutta. Fellow of the University of Calcutta. Distinguished as the author of numerous original scientific memoirs in connection with Animal and Vegetable Physiology and Pathology, among which may be noted:—"On certain Effects of Starvation on Vegetable and Animal Tissues;" "On the Development of certain Microscopic Organisms occurring in the Intestinal Canal;" "On the relation of Cholera to Schizomycete Organisms;" and (in conjunction with the late Dr. T. R. Lewis, F.R.S., elect.) of the following papers relating to the Etiology of Cholera and other Diseases:—"A Report of Microscopical and Physiological Researches into the Nature of the Agent or Agents producing Cholera;" "The Soil in its Relation to Disease;" "Cholera in Relation to certain Physical Phenomena;" "The Fungus Disease of India;" Leprosy in India;" &c., &c. Distinguished as an eminent Indian Physiologist and Pathologist.

LAZARUS FLETCHER, M.A. (Oxon),

F.G.S., F.C.S., Memb. Phys. Soc. President of the Mineralogical Society. Late Scholar of Balliol College, and Fellow of University College, Oxford. Late Millard Lecturer on Physics, Trinity College, and Junior Demonstrator at the Clarendon Laboratory at Oxford. Senior University Mathematical Scholar, 1876. Late Examiner in the Natural Science Schools in Oxford and Cambridge. Keeper of the Mineralogical Department, British Museum. Conducted the re-arrangement of the Minerals in the new Museum at South Kensington, and by his descriptions of these in the "Guides" published by the Trustees of the British Museum, has contributed valuable aid to the students of Crystallography and Mineralogy. Is the Author of many memoirs in the Journal of the Crystallological Society, and recently that of the Mineralogical Society (with which the former Journal has been united) on various minerals, including Copper, Silver, Gold, Bismuth, Sulphur, Nagyagite, Realgar, Zircon, Skutterudite, and Copper Pyrites, and of two important mathematical memoirs on the Dilatation of Crystals on Change of Temperature, in *Phil. Mag.* 1880 and 1885.

WILLIAM BOTTING HEMSLEY,

A.L.S. Assistant for India in the Herbarium of the Royal Gardens, Kew. Entered the Kew Herbarium in 1863; assistance acknowledged by G. Bentham, F.R.S., in preface to Flora Australiensis, 1863; A.L.S., 1875; Lecturer on Botany at St. Mary's Hospital, 1876; author of numerous papers on Systematic Botany, which, together with his larger works, have given him an authoritative position in this branch of science; author of the Botany (5 vols, 4to) of the "Biologia Centrali-Americana," 1879-88; joint author with Brigade-Surgeon Aitchison, F.R.S., of memoirs on the botanical collections of the several Afghan expeditions (Journ. Linn. Soc., xviii. pp. 29-113, 1880; xix. pp. 148-200, 1883; Trans. Linn. Soc, 2nd Ser. III. pp. 1-139, 1888); engaged by sub-committee of the Government Grant Committee to prepare the "Index Floræ Sinensis," an enumeration of all known Chinese plants (in course of publication, Journ. Linn. Soc., xxiii. 1886–88, *et seq.*).

CHARLES THOMAS HUDSON, M.A.,

LL.D. (Cantab.). President of the Royal Microscopical Society (1888). Was 15th Wrangler, 1852. Joint author of Hudson and Gosse's "Rotifera." Discoverer of *Pedalion mirum*, and of numerous new genera and species of Rotifera, described in papers published in the Journ. Roy. Micros. Soc., *Quart. Journ. Micros. Sci.*, and the *Ann. and Mag. Nat. Hist.* from 1869 to the present year. Specially distinguished for his knowledge of the Rotifera, concerning which he is the chief living authority. ["The genus Pedalion discovered and described by Dr. Hudson is one of the most remarkable and important contributions to animal morphology of the past twenty years."—E. R. L.]

THOMAS MCKENNY HUGHES, M.A.

F.G.S., F.S.A. Professorial Fellow of Clare Coll. Camb. Chev. Ord. SS^{rum} Maur. et Lazar. Ital. President Brit. Committee, Internat. Geol. Congress. President Chester Nat. Hist. Soc. Hon. Memb. Soc. Géol. de Belg. Memb. Soc. Géol. de France. Memb. Soc. Geol. d'Italia, &c., &c. Woodwardian Professor of Geology, Cambridge. Author of the following papers:—"On the Junction of the Thanet Sand and the Chalk," &c. (Quart. Journ. Geol. Soc. 1866, vol. xxii. p. 402); "Geology of Parts of Westmoreland and Yorkshire" (Proc. Geol. and Pol. Soc. W. Riding, Yorks. 1867); "Break between Upper and Lower Silurian Rocks, Lake District" (*Geol. Mag.* 1867, p. 346); "The Two Plains of Hertfordshire and their Gravels" (Quart. Journ. Geol. Soc. 1868, vol. xxiv. p. 283); "Part of the Geology of the London Basin" (Mem. Geol. Surv. iv. 1872). Memoirs Geol. Surv.—Explanation of Quarter Sheet 98, N.E., and of ditto S.E. (1872). "Man in the Crag" (*Geol. Mag.* 1872, vol. ix. p. 247); "Exploration of Cave Ha, Yorkshire" (Journ. Anthrop. Inst. 1874); "Felstone Implements in Pontnewydd Cave" (*ibid.*); "Classification of the Sedimentary Rocks" (Brit. Assoc. Rept. 1875); "Geological Measures of Time" (Royal Inst. March, 1876); "Silurian Grits, Corwen, N. Wales" (Quart. Journ. Geol. Soc. 1877, vol. xxxiii. p. 207); "Evidence afforded by Gravels and Brick-earth" (as to remains of Man) (Journ. Anthrop. Inst. 1877); "Pre-Cambrian Rocks of Bangor" (Quart. Journ. Geol. Soc. 1878, vol xxxiv. p. 137); "Relation and Duration of Forms of Life on the Earth to the Breaks in the Sedimentary Rocks" (Proc. Camb. Phil. Soc. 1879); "Silurian Rocks of the Vale of Clwyd" (Quart. Journ. Geol. Soc. vol. xxxv. p. 694); "The Pre-Cambrian Rocks of Caernarvon" (*ibid.*, p. 682; "Transport of Fine Mud, &c., by Conferva" (Camb. Phil. Soc., Feb. 1880); "The Altered Rocks of Anglesea" (*ibid.*); "Evidence of Later Movements of Elevation and Depression in British Isles" (Vict. Inst., March, 1880); "On the Geology of Anglesea" (Quart. Journ. Geol. Soc. 1880, vol. xxxvi. p. 237); "Geology of the Vale of Clwyd" (Chester Soc. Nat. Sci., Nov. 1880); Second Paper on "Geology of Anglesea" (Quart. Journ. Geol. Soc., 1882, vol. xxxviii. p. 16); "On the Brecciated Bed in the Dimetian at St. David's" (*Geol. Mag.* 1883, p. 306); "Report of Excursion of the Geol. Assoc. to Bangor, Snowdon, Holyhead, &c." (Proc. Geol. Assoc. vol. viii., July 1883); "Fossils in Pleistocene Gravels, Barnwell, near Cambridge" (*Geol. Mag.*, 1883, p. 454); "Tracts of Terrestrial and Freshwater Animals" (Quart. Journ. Geol. Soc., 1884); "On so-called '*Spongia paradoxica*' from the Red and White Chalk, Hunstanton" (*ibid.*); Report of the Excursion of the Geol. Assoc. to Cambridge (Proc. Geol. Assoc. 1884); "On some Perched Blocks" (Quart. Journ. Geol. Soc., 1886); "On Caves" (Vict. Inst., 1887); "Drifts of the Vale of Clwyd, in relation to the Caves" (Quart. Journ. Geol. Soc., 1887); "Some Brecciated Rocks in the Archæan of Malvern" (*Geol. Mag.* 1887); "On Bursting Rock Surfaces" (*Geol. Mag.*, Nov. 1887).

EDWARD B. POULTON, M.A. (Oxon.),

F.L.S., F.Z.S., F.G.S. Tutor of Keble College. Lecturer in Natural Science, Jesus College, Oxford. Distinguished as a zoologist, and especially for investigations upon the colours of insects. Author of the following, among other papers:—"The Tongue of *Perameles nasuta*" (*Quart. Journ. Micros. Science*, January 1883); "The Tongue of *Ornithorhynchus paradoxus*" (*Quart. Journ. Micros. Science*, July, 1883); "On the Tongues of Marsupialia" (Proc. Zool. Soc., December, 1883). Papers relating to the subject of colour and marking in insects, in

Trans. Entom. Soc., April 1884, August 1885, and June 1886; Proc. Roy. Soc., No. 237, 1885, and No. 243, 1886; "The Experimental Proof of the Protective Value of Colour and Markings in Insects in reference to their Vertebrate Enemies" (Proc. Zool. Soc., 1887); "An Inquiry into the Cause and Extent of a special colour-relation between certain exposed Lepidopterous Pupæ and the surfaces which immediately surround them" (Phil. Trans., 1887); "Notes in 1886 on Lepidopterous Larvæ" (Trans. Entom. Soc., 1887).

WILLIAM JOHNSON SOLLAS, D.Sc. (Cantab.), Hon. LL.D. (Dubl.)

F.R.S.E., F.G.S. Late Fellow of St. John's College, Cambridge. Professor of Geology in the University of Dublin. Author of numerous papers on Geology, Palæontology, and the Natural History of the Sponges, among which the following may be specially enumerated:—"On the Silurian District of Rhymney, &c." (Quart. Journ. Geol. Soc., vol. xxxv. p. 475); "On a New Species of Plesiosaurus, &c." (*ibid.*, vol. xxxvii. p. 440); "On the Structure and Affinities of the Genus *Siphonia* (*ibid.*, vol. xxxiii. p. 242); "On *Stauronema*, a New Genus of Fossil Hexactinellid Sponges" (*Ann. and Mag. Nat. Hist.*, Ser. 4, vol. xix. p. i.); "On the Flint Nodules of the Trimmingham Chalk" (*ibid.*, Ser. 5, vol. vi. p. 384); "On the Sponge Fauna of Norway" (*ibid.*, Ser. 5, vol. v. p. 130, 5 parts).

CHARLES TODD, M.A. (Camb.), C.M.G.,

F.R.A.S. Postmaster-General, Superintendent of Telegraphs, and Government Astronomer. He has executed important astronomical observations extending over thirty-eight years, including Transit of Venus, Jupiter's Satellites, Determination of Australian Longitudes, &c. He has conducted Meteorological Observations in South Australia extending over thirty years. He has written a Treatise on the Meteorology of South Australia, and other works. He has contributed papers to the Royal Society of South Australia, and was responsible for the erection of the telegraph line across the interior of Australia from Adelaide to Port Darwin, 2000 miles in length, and to Western Australia, 1000 miles in length.

HERBERT TOMLINSON, B.A. (Oxford),

Formerly Junior Student of Christ Church, Oxford. Whitworth Exhibitioner, 1870. Demonstrator of Natural Philosophy in King's College, London. Author of numerous papers on physical subjects published in the Phil. Trans., Proc. Roy. Soc., *Phil. Mag.*, &c., the most important of which relate to the influence of stress and strain on the Physical Properties of Matter. The following may be enumerated:—(1) "Effect of Magnetization on the Electrical Conductivity of Iron" (Proc. Roy. Soc., 1875); (2) "Increase in Resistance to the passage of an Electrical Current produced in certain wires by Stretching" (*ibid.*, 1877); (3) "Alteration of Thermal Conductivity of Iron and Steel caused by Magnetism" (*ibid.*, 1878). The following papers relate to the influence of Stress and Strain, &c.:—(4) "Moduli of Elasticity" (Phil. Trans., 1883); (5) "Electrical Conductivity" (*ibid.*); (6) "Relations between Moduli of Elasticity, Thermal Capacity, and other Physical Constants" (Proc. Roy. Soc., 1885); (7) "Alteration of the Electrical Conductivity of Cobalt, &c., by Longitudinal Traction" (Proc. Roy. Soc., 1885); (8) "Internal Friction of Metals" (Phil. Trans., 1886); (9) "Co-efficient of Viscosity of Air" (*ibid.*); (10) "On Certain Sources of Error in Connection with Experiments on Torsional Vibrations" (*Phil. Mag.*, 1885); (11) "Temporary and Permanent Effects on some of the Physical Properties of Iron produced by raising the Temperature to 100° C." (*ibid.*, 1886); (12) "Effect of Change of Temperature on the Internal Friction and Torsional Elasticity of Metals" (abstr. in Proc. Roy. Soc., 1886); (13) "Effect on Magnetization on the Elasticity and the Internal Friction of Metals" (Phil. Trans., vol. clxxix. p. 1); and other papers.

GERALD F. YEO, M.D. (Dubl.),

F.R.C.S. Professor of Physiology, King's College, London. Researches:—"On the Physiology of the Central Nervous System" (with Prof. Ferrier) (Proc. Roy. Soc., 1881; Phil. Trans., 1884); "On the Physiology of Muscle and Nerve" (with Dr. Cash) (Proc. Royal Soc., 1882 and 1883; Journal of Physiol., 1884); (with Mr. Herroun) (*ibid.*, 1884); "On the Composition of Human Bile" (with Mr. Herroun) (Journ. of Physiol., 1884); "On the Cause of the First Sound of the Heart" (with Dr. Barrett) (*ibid.*, 1884). On Pathological Subjects:—"Diseases of the Kidney" (Dubl. Path. Soc., 1865); "Lymph Glands" (*Med. Jährb. d. Aertze*, in Wien, 1871); "Pleuropneumonia in Cattle" (Report for Roy. Agric. Soc., 1878); and of numerous other papers (Proc. Dubl. Path. Soc.; *Irish Hospital Gazette*; *Dubl. Journ. Med. Sci.*, 1872 to 1875). Author of "Manual of Physiology."

THE SHOOTING-STARS OF APRIL.

In recent years this meteor group has not developed exceptional activity, nor have its annual returns attracted such general observation as the *Perseids* of August; but it is nevertheless a stream that is entitled to a considerable amount of interest, as some of its displays appear to have been noticed in ancient times, and it is identified with the comet described by Thatcher on April 4, 1861. The modern displays of this shower have not justified the anticipations formed of it in regard to its richness because of its periodic character. Of late years the special region of the orbit where the meteorites are clustered in the richest profusion has probably been far removed from the earth. The apparent feebleness of the shower may therefore be regarded as merely temporary. The *Leonids* of November have during the last fifteen years similarly offered a poor spectacle to those who have encouraged the hope that they might attain a prominent degree of activity. But with the parent comet (I. 1866), in distant parts of its path, it is not surprising that comparatively very few of these meteors have been seen. The same remark equally applies to the April meteors. They are chiefly condensed near the comet of 1861, which is now traversing a section of its orbit sufficiently remote from the earth to have withdrawn all the richer parts of the stream from our cognizance. The meteorites lately encountered by the earth upon crossing the node of this comet on about April 20 are simply the outlying and more scattered remnants of the system. It is highly probable, however, that the distribution of the particles is to some extent irregular, and that in certain years the shower attains a more pronounced aspect than the conditions would indicate. Thus in 1884 there was a rather conspicuous display, the number of meteors visible being about 22 per hour for one observer; but this, though representing a striking degree of productiveness relatively to the minor showers, yet falls much below the character of a meteor-stream of first-class importance.

In the present year, the Lyrid showers, if visible, will be most favourably witnessed in the early part of the night, as moonlight will interfere in the morning hours. On April 19 the moon rises at 11h. 53m., on the 20th at 13h., and on the 21st at 13h. 55m. The north-eastern sky should be watched before our satellite emerges from the horizon. The most essential features to be noticed during the progress of the display will be the following:—

(1) The position of the radiant-point on each night of observation. It is very important to note whether this point becomes rapidly displaced to the eastwards, as in 1885 (NATURE, vol. xxxii. p. 5).

(2) The horary number of meteors appearing to one observer, and the proportion radiating from Lyra.

(3) The paths and visible peculiarities of the largest meteors. It is necessary that such data be gathered and utilized in computations of the real paths of those meteors which may be recorded at more than one station.

(4) The duration of the individual meteor-flights. This is an element extremely difficult to estimate with tolerable precision, especially in respect of swift-moving meteors like the Lyrids.

(5) The positions of radiants of the minor streams which furnish meteors at this epoch. Subjoined are the

places of some of the principal of these, which have been ascertained during the last fifteen years:—

No.	R.A.	Decl.	No.	R.A.	Decl.
1	213°	+ 53°	5	286°	+ 24°
2	227	− 1	6	293	+ 43
3	231	+ 17	7	296	± 0
4	272	+ 21	8	302	+ 23

The centre of emanation of the Lyrids is at $270° + 32\frac{1}{2}°$, which lies between the constellations of Hercules and Lyra. It will be very interesting to secure additional observations this year as to the strength and character of this stream, and of the many lesser contemporary displays which manifest themselves at this period. Fortunately the weather is often propitious in the vernal season, and enables researches of this nature to be successfully prosecuted.

W. F. DENNING.

NOTES.

OUR readers may remember that, last autumn, *apropos* of a great patent case of colossal dimensions which was then before the Courts, we published an article urging that, in the interests of speedy justice, no less than for the dignity of science and its professors, it was most desirable that advantage should be taken of the provisions which already exist in our law, and especially in the Judicature Act of 1873 and its amending statutes, and in the rules of the Supreme Court framed under them, for the employment of scientific assessors or experts to aid the judge in strictly scientific cases. It may be remembered that, even in the very case on which we then commented, the tardy employment of Prof. Stokes to aid Mr. Justice Kay was productive of most satisfactory results. We are glad, therefore, to notice that, in a case of some difficulty which came before Lord Coleridge last week, the same eminent man was again called in, and again with the result of relieving the Court from the task of hearing a mass of expert evidence with which no judge and jury are competent to deal satisfactorily. The whole question at issue was whether a certain anemometer, of which one of the parties was patentee and the other the purchaser, came up to the description of its qualities given by the vendor. A considerable array of counsel appeared on both sides, and it was arranged that the services of Prof. Stokes should be called in to the aid of the Court. Seven of the anemometers were submitted to him, and, after an investigation by him, his report was read, and upon it judgment was given. The result is, that the report of the case occupies less than a third of a column of the *Times*. Without the services of Prof. Stokes, or some similar sworn expert, we should have had half-a-dozen or more expert witnesses on one side contradicted by half-a-dozen expert witnesses on the other side; a case which would have lasted three or four days before a wearied judge, conscientiously striving to understand purely technical details, and a perplexed and confused jury; great loss to both parties; an unsatisfactory result; and, as we think, no little scandal to science and scientific men. All this has been prevented by the very simple expedient of calling in an eminent man of science to make a sworn report on the purely technical details, and leaving the rest to the ordinary administration of our Courts. Herein, we are persuaded, lie the proper functions of scientific men in the administration of public justice.

TWO years ago the Dutch Congress of Science and Medicine was founded, and it was decided that it should meet every two years. The first meeting was held at Amsterdam in September 1887. The second meeting will take place at Leyden from the 25th to the 27th of April. The President of the Congress is Prof. Suringar (Leyden), who will deliver the opening address. A large attendance is expected.

THE meetings of the Institution of Naval Architects, last week, were in every way most successful, and the Institution is to be congratulated on the importance and the wide range of the subjects discussed. At the first meeting, on Wednesday, April 11, a remarkable paper on the designs for the new first-class battle-ships was read by Mr. W. H. White, the Director of Naval Construction. The principal object of this paper was to describe the main features of the approved designs for these battle-ships, and to contrast their protection, armament, speed, and coal-endurance with the corresponding features in other battle-ships designed during the last twenty years. Incidentally, Mr. White sought to show that there are good reasons why these ships surpass in size any previously constructed vessels of the Royal Navy. The reading of the paper was followed by a discussion, in which Sir E. J. Reed, Lord Charles Beresford, and others took part. On Thursday, Sir N. Barnaby, late Director of Naval Construction, read a paper on the protection of buoyancy and stability in ships. The next paper was by Captain Penrose Fitzgerald, on the protection of merchant steamers in time of war. The cruiser, *Piemonte*, built for the Italian Government, at Elswick, was described by Mr. P. Watts, of Elswick, her designer; and Mr. J. I. Thorneycroft read a paper on water-tube boilers for war-ships. On Thursday evening, technical papers were read by Mr. John Scott, Mr. J. Macfarlane Gray, and Mr. V. B. Lewes. On Friday, the first paper read was by Mr. Beauchamp Tower, describing an apparatus for providing a steady platform for quick-firing or machine-guns, or a telescope, or a search-light, on board ships at sea. The second paper was by Prof. V. B. Lewes, on the corrosion and fouling of steel and iron ships. Two papers by Mr. R. E. Froude followed, one on the part played in the operations of propulsion by differences in fluid pressure, and the other on Prof. Greenhill's theory of the screw propeller. Technical papers were likewise read by Mr. W. Rundell and Mr. A. F. Hill.

THE general meeting of the Institution of Mechanical Engineers will be held on Wednesday evening, May 1, Thursday evening, May 2, and Friday afternoon, May 3, at 25 Great George Street, Wastminster, by permission of the Council of the Institution of Civil Engineers. The chair will be taken by the President, Mr. Charles Cochrane, at half-past seven p.m., on Wednesday and Thursday evenings, and at half-past two p.m. on Friday afternoon. He will deliver his inaugural address on Wednesday evening. The following papers will be read and discussed as far as time permits:—"Research Committee on Marine-Engine Trials: Report upon Trials of the s.s. *Meteor*," by Prof. Alexander B. W. Kennedy, F.R.S., Chairman; and "Description of an Apparatus for Drying in Vacuum," by Mr. Emil Passburg, of Breslau (Friday afternoon). The anniversary dinner will take place on Friday evening, May 3.

THE public funeral of M. Chevreul, which took place in Paris, on Saturday last, was one of great splendour. This was due in part, no doubt, to the interest excited by M. Chevreul's extraordinary age; but it must also be taken as a striking indication of the respect felt in France for men who achieve eminence in science. In front of the house in which M. Chevreul died, beside the Jardin des Plantes, a tent was fitted up as a chapel; and here the body was placed in state. The procession to the Cathedral of Notre Dame was headed by a detachment of police, who were followed by a platoon of cuirassiers, the 103rd Infantry Regiment, with flags, and a band of ushers, carrying wreaths presented by the Stearine-makers of France, the Stearine-makers of Lyons, the Friendly Society of Natives of Anjou, living in Paris, and a large number of other public and private bodies. Last of all came a wreath sent by the Gobelin Works, surrounded by a woollen fringe dyed by M. Chevreul himself. The pall-bearers were MM. Fallières,

Minister of Public Instruction, Louis Passy, President of the Society of Agriculture, Chaumeton, President of the Students' Association, Des Cloizeaux, of the Academy of Sciences, Quatrefages, of the Academy of Sciences, Chautemps, President of the Municipal Council of Paris, and Roy, Manager of the Society of Arts and Manufactures. Next came the members of M. Chevreul's family, grandchildren and great-grandchildren; and they were followed by the representatives of the President of the Republic, by several of the Ministers, the Presidents of the Senate and the Chamber, and representatives of all the great educational and scientific bodies and administrative departments. At Notre Dame there was an impressive religious service. The interior of the church was hung with black, and over the porch, which was also hung with black, was a scroll bearing the dates "1786–1889." In the centre of the choir was a catafalque resting on silver columns, and surmounted by a canopy with bands of ermine. After the religious ceremony, the body was removed to L'Hay, and interred in the family vault. In compliance with M. Chevreul's last wishes, no speech was made over his grave.

MR. FRANCIS ARTHUR HERON, B.A., of New College, Oxford, has been appointed, after competitive examination, to the Assistantship in the Geological Department of the British Museum, vacant by the resignation of Mr. Arthur Dendy, now Demonstrator of Biology in the University of Melbourne.

WE are glad to see that female art students have now the opportunity of studying the scientific basis of their profession, as Bedford College for Ladies has instituted a well-arranged course of lectures to be given this spring by the Professor of Physics, Mr. Wornack, on light and colour. That an artist should have, at least, an elementary knowledge of the physics of light and colour there can be no doubt.

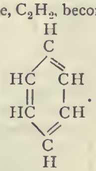
THE Liverpool Marine Biology Committee have arranged to have a four days' dredging expedition at Easter in the Liverpool Salvage Association's steamer *Hyæna*. The ground expected to be covered on this cruise is from the south end of the Isle of Man southwards to Anglesey, and along the north coast of Anglesey to Puffin Island. The submarine electric light will again be used as an attraction in tow-nets let down to the bottom, and this method of capturing some of the more active Crustacea which appear to escape the dredge, will be tried in considerably deeper water than in last year's *Hyæna* Expedition. Mr. Hoyle's new tow-net, which can be opened and closed at any required depth, and Mr. W. S. McMillan's large bottom net, will also be used.

THE extraordinary meeting of the Société Géologique de France, which will be held this year in Paris, beginning on August 18, promises to be one of great interest. During the week devoted to the meeting, the collections in Paris will be visited, and there will be a series of excursions to places of interest within easy reach of that city. In the week following the meeting, excursions will be made to more distant localities—among others to the Auvergne and Brittany, that to the former district under the guidance of M. Michel-Lévy, and that to Brittany conducted by M. C. Barrois. Arrangements will be made with the railway authorities for a reduction of 50 per cent. upon the fares; but in order to secure this advantage the names of persons intending to attend must be sent to the secretaries of the Society before July 1. British geologists, and especially Fellows of the Geological Society of London, are cordially invited to be present.

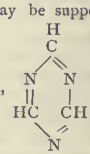
THE April number of *Himmel und Erde*, the magazine of the Gesellschaft Urania (Berlin), contains an interesting article on the Norwegian North Sea Expedition, by Prof. Mohn, Director of the Norwegian Meteorological Institute, Christiania. There is also a good article on the famous hot springs of the Yellowstone Park, by Prof. Zittel, which is illustrated by a map of the neighbourhood and a beautiful photogravure of the terraces. The Copernican theory is discoursed upon by Dr. William Meyer, and two drawings comparing the supposed paths of Mars on the old and new theories very forcibly illustrate its beauty and simplicity. Dr. Ernst Wagner contributes an article on the eruption of Krakatão, particularly referring to the work of the Krakatão Committee of the Royal Society. Besides these, there are also particulars of the various astronomical phenomena for the month.

PROF. KIKUCHI, of Tokio, Japan, has completed his work (in Japanese) entitled "Elements of Plane Geometry" by a second volume, which contains "Book iv., Ratio and Proportion; Book v., Geometrical Applications."

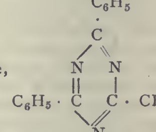
A SERIES of derivatives of the unknown tri-hydrocyanic acid, $H_3C_3N_3$, have been prepared by Prof. Krafft and Dr. von Hansen, of Heidelberg. Tri-cyanogen chloride, $C_3N_3Cl_3$, and the corresponding bromide have long been known, and the radicle C_3N_3 is supposed to exist in the ferro- and ferri-cyanides. Hence it has been expected that some day the hydride itself would be obtained, and although this has not yet been accomplished, a very close approach has been effected by the Heidelberg chemists, who have succeeded in preparing derivatives containing organic radicles instead of hydrogen. Tri-cyanogen hydride may be regarded as bearing the same relation to prussic acid as benzene does to acetylene. On passing a current of acetylene gas through a considerable length of platinum tubing heated to redness, condensation occurs, and benzene, together with other heavier hydrocarbons, is produced; three molecules of acetylene, C_2H_2, become locked up together in the closed chain of C_6H_6,



. In the same way three molecules of prussic acid, HCN, may be supposed to condense into the closed chain of $H_3C_3N_3$,



. The first compound of the new series is methyl-diphenyl tri-cyanide,



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Two parts of aluminium chloride, Al_2Cl_6, were added to a mixture of five parts of benzonitrile, $C_6H_5 . CN$, and two parts of acetyl chloride, $CH_3 . COCl$, keeping the temperature down to 0° C. The mixture was warmed upon a water-bath, when the aluminium chloride gradually passed into solution, and hydrochloric acid gas was copiously evolved. The yellow liquid thus obtained was poured into iced water, upon which a yellowish waxy substance separated endowed with a powerful tear-producing odour. The dried wax was next dissolved in ether, and after filtration of the solution and subsequent evaporation of the ether, was subjected to fractional distillation under reduced pressure. A quantity of benzonitrile passed over first; then benzoic acid, and finally, at a pressure of 15 millimetres and temperature of 220°–230°, corresponding to 370° under

ordinary pressure, methyl diphenyl tricyanide, which solidified in the receiver to a white mass of crystals. By recrystallization from alcohol, it was obtained in long needles About 50 grammes were obtained for every 100 grammes of acetyl chloride used. The crystals melted at 110°. When hydrochloric acid gas was passed through a solution of the needles in benzene, crystals of the hydrochloride separated. Upon similarly treating a warm alcoholic solution, and adding a warm solution of platinum chloride in alcohol, ruby-red crystals of the platinochloride were obtained on cooling, analyses of which confirmed the above formula, which had been independently established empirically by analyses of the base itself. A vapour-density at 444° also pointed to the corresponding molecular weight. The proof of its constitution was afforded by the products of saponification, which were found to be simply acetic and benzoic acids and ammonia. When propionyl chloride was substituted for acetyl chloride, the corresponding ethyl compound was obtained, and likewise the propyl compound by use of normal butyryl chloride.

SEVERE oscillations of the ground were noticed at Athens during the evening of April 3.

IN the *Meteorologische Zeitschrift* for March, Dr. J. Hann summarizes the results of the meteorological observations made during the French International Polar Expedition to Cape Horn in 1882-83. These observations are specially interesting, both on account of the locality and of their fulness, as they embrace several subjects not generally included in the other expeditions. The principal features of the climate are a relatively mild temperature, a high degree of humidity, precipitation in the form of rain, snow, and hail at all seasons, an almost continually cloudy sky, and sudden and very violent storms, especially in summertime. Storms occur in summer every four or five days, and decrease in number and intensity as winter advances. Nine times out of ten they approach from between north-west and south-south-west. Storms from the north-east are very rare. Thunderstorms rarely occur; distant thunder was only heard five times during summer, and lightning was seen only twice. No observations were made at Orange Bay during the month of September, but Dr. Hann has interpolated values from observations taken at Ushuaia, about a degree further northward.

THE New England Meteorological Society, following the custom of the Royal Meteorological Society of London, held an Exhibition of Instruments at Boston, in January last. Among the more interesting articles exhibited we may mention: (1) A registering actinometer, by Richard Brothers, of Paris, consisting of a bright and black globe, each containing a thermometer which registers on a drum. (2) A Watkin aneroid for mountain use, in which the hand travels three times round the dial in registering from 23 to 31 inches, so that the open scale is not sacrificed to the size of the instrument. (3) A portable anemometer, as designed by Mr. F. Galton for the Meteorological Office. In it, the Robinson cups are geared to a dial, but can be disconnected by inverting a sand-glass after a run of two minutes, and the wind's velocity in miles per hour can be read off at leisure. (4) A form of the Jordan sunshine-recorder, modified by Prof. Pickering. It consists of two half-cylinders, each with its axis parallel to the earth's axis. The sun shines through holes, the latter being shifted slightly each day, so that one sheet of sensitized paper lasts a week. (5) The Chief Signal Office exhibited a very delicate anemometer, with conical cups made of aluminium, used to determine the constants of the anemometers of the service. Among the curiosities of the Exhibition were a bottle and a saucer fused together by lightning, and a piece of window-glass which had been ground translucent by the sand-bearing winds of Cape Cod.

MR. C. CARUS-WILSON writes to us that he has devised a simple and effective dry method by which the denser minerals—zircon, rutile, tourmaline, &c.—may be separated from sand. A piece of cardboard about 2 feet long is bent in the form of a shoot or trough (it must not be allowed to break), and held in this form by elastic bands at either end; this must then be held, or fixed, at an angle sufficiently inclined to allow the sand to travel slowly down the shoot on being gently tapped. A small quantity of the sand to be treated is now placed at the head of the trough, which is then tapped with the finger. When the trough is tapped, the sand travels slowly down, and in doing so, the denser grains lag behind, forming a dark mass in the rear of the stream; this dark mass increases as the sand flows on, and must be collected and placed in a receptacle just the moment before the last tap would cause it to fall off the trough. When a sufficient quantity of this denser sand has been thus collected, it should be placed in the lid of a cardboard box (about 12 inches by 6), and gently shaken to and fro at a slightly inclined angle, the mass being at the same time gently blown upon with the breath. The finer quartz grains will thus be blown away, and hardly any but the denser grains will remain.

AT a recent meeting of the Northern Antiquarian Society of Copenhagen, Dr. L. Zinck drew attention to the remarkable graves from the Stone Age found in the northern part of Seeland. In one grave 52 bodies were found, and upwards of 175 ornaments. From the number of graves in one locality he came to the conclusion that the occupants had dwelt there. Certain bone implements showed that they had reared sheep, whilst their cooking-pots were exactly like those now in use by the peasants, called "Jutland" pots.

DURING last year the archæological researches that have been carried out in Norway were extended as far north as 70° 15′ lat. N. The results appear to show that the islands and the coast were well populated in prehistoric times, but that the cultivation of the soil did not begin until a late date. Numerous burial-places were found, and among the weapons and implements discovered were schist arrow-heads, knives of three kinds, and chisels. No stone axes like those found in the south were discovered. From the fact that no bronze objects have ever been found in the north of Norway, it is concluded that the inhabitants of the Stone Age, on coming in contact with those of the early Iron Age, adopted the use of iron, and never learnt the use of bronze. It is worthy of note that all the implements from the Stone Age are of schist, none being of flint, as in the south.

ATTENTION has lately been called by an American physician (Dr. Lindsey) to the therapeutic value of regions below the sea level, for asthmatical or consumptive patients, who there have continuously higher atmospheric pressure than at the sea-level. Excellent effects have been thus obtained in the valley of Conchilla, near Los Angeles in California, about 273 feet under the sea (barometric pressure only about 7 mm. higher). The most noteworthy place of the kind on the earth's surface is probably the Dead Sea district (–1289 feet), and the following are some others: Lake Asal in East Africa (–639 feet), the oasis of Araj in the desert of Lybia (–270 feet), the Arroyo del Muerto in California (–230 feet), the oasis of Siwah in Lybia (–123 feet), the borders of the Caspian (–86 feet).

IN the Report of the Acting Administrator of British Bechuanaland for the past year, presented to the Houses of Parliament, it is said that the forests of that region are of considerable extent, but they are being rapidly destroyed for the timber and firewood required at Kimberley. Both natives and Europeans are engaged in denuding one of the finest forest tracts in the world, which might be protected by a small yearly expenditure. The system at present employed is that any person on payment of a small fee is allowed to cut down timber without any check being put upon

him. The Surveyor-General suggests that a ranger be employed by the Crown to sell, on behalf of the Crown, fuel and timber that has reached maturity. With regard to the trigonometrical survey of the country, the piles built extend over 4000 square miles, and the final results deduced from observations made over 2600 square miles have been recorded. It was intended to commence a geological survey during the past year, with the object chiefly of examining the districts in which gold, coal, lead, and other minerals have been found, but considerations of economy have led to the abandonment of this scheme for the present.

THE present state and the history of the flora of the province of St. Petersburg were lately the subjects of a very interesting communication by Dr. R. Regel at a meeting of the St. Petersburg Society of Naturalists (*Mémoires*, vol. xix.). The influence of man in the introduction of new species is most marked in so populous a province. Several species have been unconsciously imported by man from South-Eastern Europe, the Mediterranean coast, and Asia; and many garden plants, such as *Bellis perennis*, *Impatiens parviflora*, and *Aster præcox*, have become regular members of the wild-growing flora. Some of them have spread with astonishing rapidity. The recently imported *Erigeron canadense* has now penetrated as far as the Altai Mountains; the *Matricaria discoidea*, imported from America thirty years ago, is found all over the region; while the *Sambucus racemosa* grows even in the wildest marshes of Schlusselburg. Plants imported by man are dispersed by birds, water, and wind over a wide space, and a great many species, such as *Elodea canadensis*, *Corydalis bracteata*, *Scilla cernua*, &c., have spread during the last ten years. Dr. Regel insists upon the necessity of such species being carefully mentioned, because a few years after their introduction the botanist may not be able to explain how they appeared in the region, and may suppose that they have been merely overlooked.

ANOTHER interesting feature of the St. Petersburg flora which has been pointed out by Dr. Regel, is the frequency of white colour in the case of such flowers as are coloured pink or blue in Central Europe. The prevalence of white in the north is thus confirmed. The *Polygala vulgaris*, *Lychnis viscaria*, *L. Flos-cuculi*, *Centaurea phrygia*, *Jasione montana*, *Campanula glomerata*, *C. Trachelium*, *C. latifolia*, *C. rotundifolia*, *C. patula*, *Calluna vulgaris*, *Arctostaphylos Uva-ursi*, *Thymus serpyllum*, *Brunella vulgaris*, *Gymnadenia conopsea*, *Orchis maculata*, *O. Traunsteineri*, and others were found with white flowers. In accordance with Dr. Masters's views, Dr. Regel sees in this fact a pathological phenomenon due to unfavourable climatic conditions. The colouring pigment does not disappear, but more intercellular spaces appear, and being filled with air, they permit the full reflection of light. In fact, several white flowers of *Campanula patula* became blue when dried, the pressure exerted upon them evidently having compressed and reduced the intercellular spaces.

THE following are the lecture arrangements at the Royal Institution after Easter, so far as they relate to science:—Prof. E. Ray Lankester, four lectures on some recent biological discoveries; Mr. Eadweard Muybridge, of Pennsylvania, two lectures on the science of animal locomotion in its relation to design in art (illustrated by the zoopraxiscope); Prof. Dewar, five experimental lectures on chemical affinity; Prof. W. Knight, of St. Andrews, three lectures on (1) the classification of the sciences, historical and critical; (2) idealism and experience, in philosophy and literature; (3) idealism and experience, in art and life (the Tyndall lectures). The Friday evening meetings will be resumed on May 3, when a discourse will be given by Sir Henry Roscoe, M.P., on aluminium; succeeding discourses will probably be given by Prof. Dewar, Prof. Silvanus P. Thompson, the Rev. S. J. Perry, Prof. D. Mendeleef, Mr. A. Geikie, Mr. C. V. Boys, and other gentlemen.

THE Registrar of the University of London desires us to call attention to the fact that the June examination for matriculation is, in the present and future years, to be held a week earlier than heretofore.

IN the list of English and American Corresponding Members of the French Academy of Sciences, printed in NATURE, January 24 (p. 312), Prof. A. Agassiz was inadvertently omitted from the list of names under "Anatomy and Zoology."

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus*) from Barbadoes, presented by the West Indian Natural History Exploration Committee; a Kinkajou (*Cercoleptes caudivolvulus*) from Demerara, presented by Mrs. Marian FitzSimons; a Grey Squirrel (*Sciurus griseus*) from North America, presented by Miss Vokes; a Shag (*Phalacrocorax gracilis*), British, presented by Mr. Henry Reynold's; a Black Tortoise (*Testudo carbonaria*) from Trinidad, a —— Cayman (*Jacare*, sp. inc.) from Demerara, presented by Colonel Fielden, F.Z.S.

OUR ASTRONOMICAL COLUMN.

MELBOURNE OBSERVATORY.—The twenty-third Annual Report of the Board of Visitors to this Observatory, together with the Annual Report of the Government Astronomer, have just reached us. The visitation took place on October 4, 1888, and the Astronomer's Report is for the year ending June 30, 1888. The principal points of interest in the Reports are those referring to the great reflector and to the new photographic telescope. The mirrors of the former instrument had become so dull that their repolishing was rendered imperative, and the work was to be done at the Observatory itself, the risk and cost of sending them to England being prohibitory. With a view to this important operation, a number of small mirrors have been repolished, in order that the necessary experience might be acquired before the great mirrors were taken in hand. The new telescope for the photographic survey was well advanced, and Mr. Ellery expected that Melbourne would be ready to enter on her share of the work as soon, if not sooner, than the other associated Observatories. The Government of Victoria, besides supplying the necessary funds to enable the Observatory to take part in the photographic survey, had granted £2300 for the erection of a dwelling-house for the Director within the Observatory grounds. The new transit-circle and the two equatorials had been in constant use, and were in good order; 2962 observations of right ascension, and 1434 of Polar distance, having been obtained with the former during the year. The photoheliograph had been subjected to a slight alteration, the front lens of the secondary magnifier having been previously too close to the primary focus, so that much trouble was caused from the magnified images of any particles of dust which might lodge upon it. The sun-pictures had, in consequence of the time employed over this alteration, not been so numerous as usual, a record of the sun's surface being only obtained on 129 days. With the great telescope, eighty-one nebulæ were observed or searched for.

Mr. Ellery reports that the meteorological department, especially with regard to the Inter-Colonial Weather Service, increases in importance and efficiency every year, and that he proposed to call a conference of the several Meteorological Directors of the Australian Colonies, with the view of assimilating their methods of reporting, and of still further improving meteorological work in Australia.

COMET 1889 *b* (BARNARD, MARCH 31).—Herr von Hepperger has computed the following elements and ephemeris for this object from observations of dates March 31, April 4 and 8:—

$$T = 1889 \text{ July } 27{\cdot}4812 \text{ Berlin M.T.}$$

$$\left.\begin{aligned} \omega &= 257^\circ\ 27'\ 28'' \\ \Omega &= 308\ \ 29\ \ 41 \\ \iota &= 162\ \ 46\ \ 20 \end{aligned}\right\} \text{Mean Eq. } 1889{\cdot}0.$$

$$\log q = 0{\cdot}29519$$

Error of middle place (O – C.).

$\Delta\lambda \cos\beta = -11''$. $\Delta\beta = 0''$.

Ephemeris for Berlin Midnight.

1889.	R.A. h. m. s.	Decl. ° ′	Brightness.
April 18	5 10 18	15 30·6 N.	0·92
22	5 8 57	15 22·9	0·91
26	5 7 53	15 15·2	0·91
30	5 7 4	15 7·5 N.	0·90

The brightness at discovery is taken as unity.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *APRIL* 21–27.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 21

Sun rises, 4h. 53m.; souths, 11h. 58m. 35·0s.; sets. 19h. 5m.: right asc. on meridian, 1h. 57·6m.; decl. 12° 1′ N. Sidereal Time at Sunset, 9h. 5m.

Moon (at Last Quarter on April 22, 14h.) rises, 1h. 0m.; souths. 5h. 0m.; sets, 9h. 0m.: right asc. on meridian, 18h. 58·1m.; decl. 22° 38′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	4 50	11 43	18 36	1 42·5	9 35 N.
Venus ...	4 37	12 48	20 59	2 47·7	22 45 N
Mars ...	5 25	12 59	20 33	2 57·7	17 4 N
Jupiter ...	0 42	4 38	8 34	18 35·9	22 55 S.
Saturn ...	11 25	19 5	2 45*	9 5·2	17 54 N
Uranus ...	17 44	23 12	4 40*	13 12·8	7 0 S.
Neptune..	6 12	13 57	21 42	3 56·3	18 46 N.

* Indicates that the setting is that of the following morning.

April.	h.	
24	23	Jupiter stationary.
25	7	Mercury in superior conjunction with the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Apr. 21,	2 53	*m*
			,, 26,	2 33	*m*
R Canis Majoris	7 14·5	16 11 S.	,, 22,	20 46	*m*
δ Libræ	14 55·1	8 5 S.	,, 23,	0 23	*m*
U Coronæ	15 13·7	32 3 N.	,, 24,	21 37	*m*
S Herculis	16 46·8	15 8 N.	,, 24,		*m*
U Ophiuchi	17 10·9	1 20 N.	,, 21,	20 19	*m*
		and at intervals of		20 8	
U Aquilæ	19 23·4	7 16 S.	Apr. 26,	23 0	*M*
R Vulpeculæ	20 59·5	23 23 N.	,, 26,		*M*
S Cephei	21 36·6	78 7 N.	,, 28,		*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ζ Ursæ Majoris	206	57 N.	Slow; bright.
,, β Libræ	228	5 S.	Swift.
,, β Serpentis	233	16 N.	Very swift.
,, π Herculis	256	37 N.	Swift.
	272	20 N.	Swift.

GEOGRAPHICAL NOTES.

To the current number of the Proceedings of the Royal Geographical Society, Mr. George Taylor contributes a valuable paper on Formosa and its aborigines. Mr. Taylor, while resident on the south coast of Formosa, had good opportunities of studying the natives, and the information which he has otherwise collected renders his paper the best summary of our knowledge of Formosa which we have at present. Mr. Taylor thus summarizes the geography of Formosa. On the western side, the land is composed of low level plains, extending from the sea-shore to some distance into the interior, the country appearing flat up to the more pronounced elevations which precede the steeper mountain slopes. The splendid watershed from the central mountains shows in the numerous rivulets which spread like a network over the plains, and renders them especially suitable for the cultivation of rice and sugar-cane. The western sea-board partakes of the nature of the land, the coast being lined with mud and sand-sanks intersected by channels, this formation extending some distance out to sea. Within, Formosa is comparatively hilly, but large areas are covered with tea plantations, which form the principal industry. The east coast is rugged, precipitous, and exposed to the full fury of the north-east monsoon, which blows hard throughout eight months of the year; therefore, except in the Pilam plain and a few small valleys, little attempt is made at cultivation. To the south, the land terminates in huge masses of coral limestone, and coral branches may be traced in peaks elevated 2000 feet above sea-level. The sea-shore is lined with a semi-vitrified conglomeration of clay, sand, and coral, which presents a serrated surface so sharp and ragged as to be impassable to all beasts; and the natives, when fishing, are obliged to protect their feet with sandals composed of many folds of boar-skin. Mr. Taylor's account of the aborigines is specially valuable. While the pure aborigines from the interior are of an essentially Malayan type, still there is extraordinary diversity of features, indicating a considerable mixture of types. Among the Paiwang, probably the earliest settlers, head-hunting prevails. The Tipuns, again, seem to be of northern origin. To the naturalist—whether geologist, botanist, or zoologist—the interior of Formosa offers an almost virgin paradise.

ACCOUNTS are to hand of M. W. Delcommune's recent exploration of the Lomami, one of the most important southern tributaries of the Congo. Both Cameron and Wissmann met with the Lomami far to the south, and the latter connected it with the Sankuru. M. Delcommune, however, navigated the river from its mouth in the Congo, about 100 miles below Stanley Falls, for a distance of 580 miles, to a point only three days' journey from Nyangwé in the Lualaba. The river is reported to traverse a magnificent country, to be free from all obstructions, and to all appearance it continues to be navigable for some distance beyond M. Delcommune's farthest point.

MESSRS. W. AND A. K. JOHNSTON have sent us a copy of the third edition of their small map of Central Africa, in which Mr. Stanley's recent route is laid down in red. The next edition ought to have the Lomami delineated in accordance with M. Delcommune's recent exploration.

AFRICA fills a large place in the new number of *Petermann's Mitteilungen*. Dr. K. W. Schmidt has an article which deserves serious attention on the surface or soil conditions of German East Africa. Dr. Schmidt writes from careful personal observation, and his estimate of the capabilities of the German sphere is not very encouraging. Freiherr von Steinächer contributes some useful notes on German South-West Africa, with a map of Herreo Land and neighbouring regions. Dr. R. Lüddecke describes at some length the features of the new map of Africa, in six sheets, which has been prepared for the new edition of Stieler's "Hand-Atlas."

IN an interesting account in *Les Missions Catholiques* of a missionary's journey through Ecuador, it is stated that of the many towns and village, as Archidona, Canelos, &c., the names of which appear on maps of the country, scarcely one exists. The natives do not live in villages, and even where there is a church, they live miles away in the forest in small solitary communities. Hitherto the missionaries have failed in inducing the natives to take to communal or social life.

M. ROGOZINSKI, who has been in Europe for some time, has returned to Fernando Po, and intends to resume his explorations in the Cameroons region, and especially to endeavour to ascertain the existence or non-existence of the Lake Liba, which still figures mysteriously in maps of Africa.

THE death is announced of M. V. A. Malte-Brun, son of the great geographer of that name, and who himself for the last forty years had been a student of and writer on geography.

AFFORESTATION IN CHINA.

THE question of afforestation in China is at the present time attracting a great deal of attention. China is a treeless country, and to this, perhaps, are due the devastating floods which work such ruin there, and the fearful seasons of drought, which are almost as destructive as the floods. The timber used

in various ways is all imported—chiefly from the United States of America, and from Hainan and Formosa. Till the overflow of the Yellow River some time ago, no one paid the least attention to this question; but now a proclamation of the liberal Viceroy, Li Hung Chang, to the people of his thickly-populated provinces, shows that the subject will receive the attention it deserves. His Excellency says that one of the first principles in governing a State is to watch over the agriculture of the State, so that it may benefit both the individuals who till it and the State. In one of the provinces over which he rules—namely, that of Chihli—arboriculture is rendered especially easy by the softness and fertility of its alluvial plains. If we omit the various species of fruit-trees, such as the apple, pear, and apricot, other kinds of trees are very rarely seen, and in consequence vast tracts of fertile plains are left barren. Some slight attempts have been made to plant these extensive tracts with forest trees; but the strong northerly winds which prevail soon uprooted trees which had not been planted to a sufficient depth nor in well-chosen places. Amongst the peasants, the Viceroy says, the principles of arboriculture are unknown, and therefore their previous efforts have only resulted in labour and money uselessly expended. In recent years the Viceroy has ordered the planting of willow-trees along the banks of the streams and rivers in Chihli, with the object of protecting and strengthening the embankments.

If successful methods, His Excellency asks, have been found for cultivating trees in salt lands, how much more easy ought they to be found in the rich level plains of Chihli? Accordingly, the authorities of the various prefectures and sub-prefectures of Chihli are instructed to procure the necessary seed trees, and to inform the people in their respective districts of the eight directions for tree-planting and the ten benefits to be derived from the same. Steps are to be taken by the authorities to encourage the people in their efforts at planting, but official agents, who might oppress the people, are not to be sent among them. At the end of each year a statement is to be submitted to the authorities, by every person who has tried planting, of the number of trees he has received, the number successful, the species which have thriven best, &c., so that the Government may reward those who are most successful in these experiments in arboriculture, as well as gather information to guide them in the future. Instructions are given to the local authorities to deal severely with any person who steals or cuts down the trees of others. The Viceroy says that his intentions in issuing this proclamation are to afford another source of livelihood to the peasants, to help in preventing droughts and checking floods, to regulate the rainfall, and to beautify the country.

The eight directions and the ten benefits are worth recording. The directions are as follow:—(1) To fortify the roots against injury from cold, which, on account of the loose nature of the soil near the surface, readily injures the roots, a fertilizer, made by burning a mixture of dung and grass, should be used when planting trees, and when the fertilizer is put in, the roots should be carefully covered. (2) When a tree has been securely planted, a small cumulus of earth should be placed around it, 6 or 7 inches high, and should be renewed before winter sets in every year till the close of the third year. By this means the wind and cold cannot reach the roots, nor will the necessary natural nourishment in the earth escape. (3) In places exposed to high winds the trees should be planted to a depth of at least 3½ feet; at this depth the rich part of the soil is reached. In case of willows and other such trees, the outspreading and dependent branches are to be carefully pruned. (4) Rich earth, with a suitable fertilizer, is to be added to poorer soils. (5) To prepare the ground for the reception of the seeds of such trees as the oak, elm, poplar, cypress, &c., which are shed every year, a trough is to be dug round each tree and filled with water to keep the soil moist. (6) Willow and mulberry trees should be planted in the spring, when there is rain. Before planting the young shoots, the soil should be well loosened and fertilized, and grafting should always take place after the rain, and the graft-trees should be well watered every alternate day. (7) In transplanting trees, the greatest care should be taken to preserve the three vertically-projecting roots, which every tree has, from the wind and sun. When there is rain, a small hole is to be dug by the side of the tree, cutting away one of these roots; this operation is to be repeated in a fortnight if there is rain; if not, a month must elapse before the second root is cut, and similarly in the case of the third root. When the roots are cut away, innumerable little roots will be thrown out. If there is no rain, the ground must be well watered before any transplanting is attempted. (8) In raising trees from the seeds of the oak, mulberry, &c., some fertile spot should be prepared just as it would be for a crop of grain, and the seeds are planted in the same way as grain is planted. Spring-time is the best, and while there is rain. When the young trees spring up and grow to the height of one or two feet, they can easily be transplanted as directed above.

The ten benefits of planting trees are thus enumerated by His Excellency:—(1) By planting trees at the river-banks the loose and sandy soil is strengthened by the roots, and the banks increase in height. (2) A large and profitable industry will spring up if pine, elm, willow, &c., are planted in the mountains on the borders. (3) The planting of trees around fields and farms will do away with the superfluous moisture and preserve a fair equilibrium of wind and fluid influences. (4) Where trees are in abundance, droughts will be unknown. (5) Abundance of trees also help to ward off epidemics, and in thickly-populated districts trees should be specially planted for this purpose. (6) Where there is abundance of trees, travellers and families can find rest and shelter in the summer. (7) The operations of highwaymen and banditti are hindered where trees and forests are plentiful. (8) The snows on the mountains of the border will be absorbed by forests. (9) The poorer peasants will have sufficient fuel from the branches, which are pruned every year. (10) Many of these trees, as the *Quercus mongolica*, afford food to the silk-worm, which, in the mountainous regions, weaves a cocoon which makes much cheaper and more durable silk than that of the mulberry silk-worm.

SUPERSTITION AND SORCERY IN NEW GUINEA.

IN the Report to the Colonial Office of the Special Commissioner for British New Guinea during the past year, there is a long and very interesting account of some of the superstitions of the natives of that country, written by Mr. H. H. Romilly. One of the most sacred obligations, he says, on the relatives of a deceased man is to place in his grave, and in his accustomed haunts, food and water for the spirit of the departed. It is thought that this spirit is all that remains of the deceased, and the human appetites take possession of it, or, rather, remain in existence, just as if the body had not died. If, however, he is killed in battle, there is not the same necessity of constantly feeding his spirit; the head of one of the tribe or race who killed him is sufficient. If the slayer is a white man, the angry spirit can be laid by a large payment of goods to the relatives of the deceased, and this constantly happens. Dreams are, to them, voices from the land of spirits, telling them what to do, for whom to work, from whom to steal, and what to plunder. White men are always attended by a familiar spirit, which is blamed for any mischief that befalls the natives in a locality where a white man happens to be. If the white man is a friend of theirs, they merely demand compensation, which he will pay, says Mr. Romilly, if he is a wise man; if he is unfriendly to them, the unfortunate white man may prepare for the worst. His attendant spirit will not help him, for it flies at the sound of a gun. On the death of a relative, there is a great drumming and burning of torches to send the spirit safely and pleasantly on its travels. In some parts of the country, certain trees have spirits, and on feast-days a portion of the food is set apart for these spirits. It is worthy of remark that all their spirits are malignant, and these have to be overcome by force of arms, by blessings, or by cursings. They cannot grasp the idea of a beneficent spirit, but regard them all as resembling Papuans generally—that is, vindictive, cruel, and revengeful. Consequently, these spirits are much feared; though they cannot be seen, yet they constantly use arrows and spears when they are vexed. The great opposer of spirits is fire, and hence, on every possible occasion, bonfires and torches are employed. Strange to say, though fire is thus all-powerful with them, they have no god or spirit of the fire. In this they are at least true to their belief, for no spirit can be, with them, beneficent. Sorcerers are implicitly believed in, and they generally do a good trade in the sale of charms, which are made, not on any fixed principle, but according to the freaks of fancy of the sorcerer or the purchaser. Sometimes it is a bit of bark, sometimes a crab's claw worked in the most fantastic way. These are protectors against all injuries or accidents that may happen to a

man. A sailor will wear one as a protection against shipwreck, another charm saves its wearer from wounds in battle, another from disease, and so on. Besides being a sorcerer, that personage is also a physician and surgeon, and usually the astrologer and weather prophet of his district. It can hardly be said that he is skilled in these professions. An unvarying mode of treatment of a patient who is suffering pain from any cause whatever is to make a long, and sometimes a deep, incision over the abdomen. As may be imagined, this is not a very safe remedy. In one instance Mr. Romilly mentions, a woman, who was suffering severely from several spear-wounds, was thus treated by the native sorcerer, who, in pursuit of his profession of surgeon, inflicted by far the most severe wound the poor woman received, thus destroying the chance of life which she had before he attended her. Many of the tribes are, through the influence of the missionaries, shaking off these superstitions. "But even these people," says Mr. Romilly, "the most civilized in New Guinea, and many of them professed Christians, in times of great excitement revert to their old habits. This was shown during the autumn of 1886. At that time a severe epidemic raged along the south coast. The people were dying, by hundreds, of pneumonia, and were beside themselves with fear. The usual remedies for driving away spirits at night were tried, remedies which had been in disuse for years; torches were burnt, horns were blown, and the hereditary sorcerers sat up all night cursing; but still the people died. Then it was decided that the land spirits were working this harm, and the whole population moved their canoes out in the bay and slept in them at night; but still the people died. Then they returned to their village, and fired arrows at every moving object they saw. . . In course of time the epidemic wore itself out; but while it lasted the civilized Motuans were as superstitious as any of their neighbours could have been."

THE MUSEUM OF COMPARATIVE ZOOLOGY, HARVARD COLLEGE.

THE Annual Report of Prof. A. Agassiz for 1887–88 has been issued. It gives the usual interesting account of the various courses of instruction which have been provided at the Museum during the academic year, and of the reports from the several officers about the collections under their care. Excellent progress has been made with the extensive addition to the Museum building, in which there will be ample accommodation for the geological and geographical departments. While numerous specimens have been sent to specialists, a number of applications have from necessity been refused, as the Museum staff is very far from being large enough to meet the demand on its time which attention to all such applications would require. For the future, the very reasonable rule has been laid down that only single specimens for special study can be sent out from the Museum, so that the larger collections must be studied at the Museum, where, we may add, they may be examined with every advantage. In an appendix, a list of the publications of the Museum during the past year will be found, and there is also a most important list of all its publications from the commencement: the Annual Report from 1859, the Bulletin from 1863, the Memoirs from 1864. In a footnote comment is made on some remarks appearing in the preface to the *Zoologischer Jahresbcricht* for 1886, on the irregular way in which the publications of the Museum appear. We only allude to this to express our hope that no criticisms will alter the present arrangement, which is one that allows of the prompt publication of the various new facts brought to light by the band of workers at Harvard. We can conceive that by a librarian, simply as such, the publication of a volume in parts is held in abhorrence, and the publication of parts of two or three volumes of a series, at the one time, fills him with dismay; but to the working student it is very different, and such owe a great deal of gratitude to the Curator of the Museum at Harvard, for the speedy publication of the Museum Memoirs as well as for the great liberality with which these are immediately posted to Europe on their issue from the press. The following paragraph we read with mingled feelings of regret and pleasure:—"In the past fifteen years I have been in the habit of supplying deficiencies for such expenditures as seemed to me essential for the rapid development of such an establishment. But it has now become evident that, while such a policy may have been useful in the early stages of the Museum, it has of late been rather a detriment to it than otherwise, as it was fast coming to be regarded as my personal establishment. The demands upon my time for the administration of the affairs have become so great, that I must retire from active duty to devote myself to scientific work, which I have too long neglected for the sake of bringing the Museum to the point it has reached. It is high time that I should withdraw, and that a younger man, more in sympathy with the prevailing tendency of science in this country, should endeavour to develop the Museum by increasing the interest of the friends of the University in its behalf." We fail to comprehend how any man living could be more in sympathy with modern science than Alexander Agassiz, but we recognize as a fact that he has original work to finish, while it is yet day, and it is universally acknowledged that he has established such a museum at Harvard as may employ the energies of many workers for years to come.

RESULTS OF EXPERIMENTS UPON THE GROWTH OF POTATOES AT ROTHAMSTED.

DR. GILBERT has, in the form of a lecture recently delivered at the Royal Agricultural College, given a *résumé* of twelve years of experimental work in connection with the growth of potatoes. The subject is in itself highly interesting, including, as it does, a large number of important questions relating to the propagation of new varieties, the proper cultivation of the ground, the potato-blight, as well as the best fertilizers for the crop. Dr. Gilbert at once disclaims all idea of entering upon the larger questions involved in potato-cultivation, and confines himself entirely to that of fertilizers, and in regard to this point he is not able to throw much fresh light upon the usual practices of growers. The old story of the value of a due apportionment of nitrogenous and mineral substances is clearly shown to be required for the growth of potatoes, as for all crops. The value of farmyard manure is also well indicated in a manner which, on the whole, supports the present practice of all good farmers. The meagre results obtained from mere mineral manures, unassisted by nitrogenous manures, are also well brought out. The practice of employing liberal dressings of dung, superphosphate, and potash salts, or of substituting nitrate of soda or sulphate of ammonia for farmyard dung, is simply indorsed by Dr. Gilbert's results, and, beyond this, no new light is shed upon the subject of fertilizers for potatoes.

The effect of liberal applications of nitrogenous and mineral manures in increasing the proportion of diseased tubers, in years in which the blight is prevalent, is too familiar to need further proof; and as a matter of fact, the wisest course appears to be to balance the advantages of a heavy crop against an increased liability to disease.

A point is made by proving very conclusively that the continuous growth of potatoes upon the same land does not render the crop more liable to disease, but rather the reverse. For example, the percentage of diseased tubers during the first four years of potato-growing ranged in the various plots from 5·14 to 12·82, the largest amount of disease occurring upon the land manured heavily with dung and nitrogenous dressings. In the second four years, the average amount of diseased potatoes ranged from 1·63 to 4·95 per cent., while in the third series of four years it was reduced to from 1·43 to 1·73 per cent. No fluctuations of season can overturn these figures. They have an important bearing upon the question of the propagation of the disease, and appear to detract from the value of suggestions that the blight continues to exist in the form of resting spores in the ground. If such was the case, the disease, when once established, would surely tend to greater virulence in the case of constantly repeated growths of diseased crops. Practical agriculturists would scarcely be induced, from these results, to take special measures for destroying diseased tubers, for carefully preventing their introduction into manure-heaps, or for gathering diseased haulm off the land—all of which precautions students of potato-disease have advised agriculturists to take.

The composition of the tubers, after manuring with the various fertilizers employed, is strikingly similar, with the exception that the heavier crops are rather more watery in character—a result which may always be looked for in luxuriant vegetation. The general result of these experiments is encouraging, in so far as they show that the methods in general use for manuring the potato crop are the best that can be devised for the growth of potatoes.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 28.—"On certain Ternary Alloys. I. Alloys of Lead, Tin, and Zinc." By C. R. Alder Wright, D.Sc., B.Sc., F.R.S., Lecturer on Chemistry and Physics, and C. Thompson, F.C.S., F.I.C., Demonstrator of Chemistry in St. Mary's Hospital Medical School.

It is well known, that quite apart from a tendency to separate more or less completely into different mixtures during solidification, certain mixtures of molten metals show a tendency to separate into two alloys of different densities on standing fused for some time. Lead and zinc and bismuth and zinc have been shown by Matthieson and V. Bose to form two such mixtures; the authors find that aluminium and zinc or aluminium and bismuth also behave in the same way; in each case two different alloys are formed, one consisting of the heavier metal with a little of the lighter one dissolved therein, the other of the lighter metal containing a small quantity of the heavier one.

On the other hand, *tin* will alloy indefinitely in all proportions with any of the four metals, lead, bismuth, zinc, or aluminium, the mixtures exhibiting no particular tendency to separate into two different alloys on simply remaining at rest in a fused condition, although in certain cases more or less separation is apt to occur *during solidification*, owing to partial formation of eutectic alloy. Various other metals, *e.g.* cadmium, antimony, silver, &c., appear to behave like tin in this respect.

It seemed to be of interest to examine the behaviour under similar conditions of ternary mixtures where two of the ingredients are not miscible together in all proportions (like aluminium and lead), whilst the third is miscible indefinitely with either of the other two (like tin). It might be expected that with certain proportions a single stable alloy would result, whilst with others the mass would divide into two different ternary mixtures. In point of fact this is precisely what occurs.

For a variety of reasons the authors selected the alloys of lead, tin, and zinc for their first experiments.

These led to the conclusion that the greater the proportion of tin present (provided it does not exceed the limiting amount beyond which no separation takes place) the more zinc is contained in the heavier alloy, and the more lead in the lighter one; but that the distribution of the tin throughout the entire mass is by no means uniform, the lighter alloy containing the greater percentage when the proportion of tin in the total mass is low, and *vice versâ* when it approaches towards the limiting amount; so that with a particular proportion of tin in the total mass uniform distribution as regards weight percentage occurs, but with no other proportion.

The authors next attempted to find out whether a moderately large variation in the temperature at which the mass kept molten had any great influence on the end result; for if not, obviously much laborious work would be saved. Two series of compound ingots (forty in all) were accordingly prepared, one at a temperature close to 565° C., the other at near to 689° C. From the analytical results obtained, three noteworthy curves are deducible—

(*a*) When the tin percentages in the heavier alloy are plotted as abscissæ and the zinc percentages as ordinates.

(*b*) When the tin percentages tin the lighter alloy are plotted as abscissæ and the lead percentages as ordinates.

(*c*) When the tin percentages in the heavier alloy are plotted as abscissæ and excesses of the percentage (+ or −) in the lighter alloy over those in the heavier one as ordinates.

These three curves respectively represent approximately the solubility of zinc in lead containing tin, that of lead in zinc containing tin, and the relative distribution of tin in the two alloys formed simultaneously. The three curves obtained from one series are practically identical with the corresponding curves from the other series, so that it may be fairly concluded that the effect of variation in temperature from 565° to 689° is negligible as compared with the experimental errors, more especially those due to imperfect separation by gravitation of the two alloys from one another.

The curves representing the tin distribution are remarkable. As long as the tin percentage in the total mass is less than about sixteen the lighter alloy contains more tin than the heavier one; at about this point (representing some 14 per cent. in the heavier and 18 per cent. in the lighter alloy) the difference becomes a maximum, after which the difference diminishes, until at about 28 per cent. the same percentage of tin is contained in both alloys. After this the heavier alloy contains more tin than the lighter, the difference continually increasing.

Certain irregularities were observed due to the existence of some cause interfering with the proper separation by gravitation of the heavier from the lighter alloy: this was ultimately traced to convection currents set up through unequal heating of the walls of the containing vessel at different levels, and it was found that the imperfect separation could be almost completely obviated by so heating the mass as to avoid this inequality of temperature. This was finally effected by employing crucibles very long in proportion to their diameter (large test-tubes moulded on a core from a plastic mixture of fireclay and syrupy silicate of soda, diluted with about three times its weight of water), heated by immersion in a bath of molten lead some 6 or 7 inches deep, contained in an iron cylindrical vessel (the lower two-thirds of a mercury bottle), surrounded by a concentric clay jacket and heated by a number of bunsen burners playing into the annular interspace. Several series of compound ingots were thus prepared, containing lead and zinc in ratios different for each series (2 to 1, 1 to 1, 1 to 2), some at a temperature near to 650°, others at about 750°. From the results of the analysis of upwards of 130 different alloys thus obtained, the following conclusions are drawn:—

When a mixture of lead, tin, and zinc in the molten condition is well stirred up by mechanical means and then left to itself for some hours at as nearly as possible a uniform temperature, a single homogeneous alloy results if the proportion of tin present is not less than three-eighths of the whole; but if materially less tin than this is present, the mass divides itself into two different ternary alloys, lead predominating in the heavier one and zinc in the lighter one. This phenomenon is entirely distinct from the segregation of alloys during solidification, in consequence of formation of eutectic or other differently fusible alloys.

If there is little or no inequality of temperature at different parts of the mass, separation by gravitation only is complete in a few hours, at any rate when tolerably pure metals are employed; but if the mode of heating is such that convection currents are set up, the separation is greatly interfered with, and in extreme cases almost entirely prevented.

The heavier alloy is a saturated solution of zinc in lead containing tin, and the lighter one a similar solution of lead in zinc containing tin. No matter what the relative proportions between lead and zinc in the original mass, the two alloys always correspond to two conjugate points on the solubility curves of zinc in lead-tin and of lead in zinc-tin.

But little, if any, difference in the way in which a given mass divides itself is noticeable, whether the temperature which the molten mass maintained is below 600° C. or above 700° C.

The tin contained in the mass does not distribute itself equally in the two alloys except when present in one particular proportion, which varies with the ratio of the zinc to the lead in the entire mass. With less tin than this the lighter alloy, and with more the heavier one, takes up the higher percentage of tin.

Curves drawn representing the tin present in the heavier alloy as abscissæ, and the (+ or −) excess of tin in the lighter alloy over that in the heavier one as ordinates, are found to differ with the ratio of zinc to lead in the entire mass. They always possess the same general features, viz. rising from the origin to a maximum elevation, then sinking down again to the base line, and crossing it so as to become negative; but the position and height of the maximum, the crossing point, and the general dimensions of the curve, vary with the ratio of zinc to lead in the mass.

As a result of this, whilst an indefinite number of different mixtures may be prepared, each one of which will give the same heavier alloy, the lighter alloy simultaneously formed will be different in each case; and conversely.

When no tin is present, lead dissolves zinc to such an extent as to form an alloy containing 1·24 per cent. of zinc, and zinc dissolves lead forming an alloy containing 1·14 per cent. of lead; the higher values found by previous observers being slightly incorrect through imperfect separation.

Nothing abnormal appears to characterize the solubility curves of zinc in lead-tin and of lead in zinc-tin; in each case the amount of one metal dissolved by the other increases as the quantity of tin present increases, in such a way that the curves are somewhat concave upwards.

Royal Society, April 4.—"On the Magnetic Inclination, Force, and Declination in the Caribee Islands, West Indies." By T. E. Thorpe, Ph.D., F.R.S.

The following determinations of the magnetic elements among the Caribees or Windward Islands were made in

August 1886, on the occasion of the Eclipse Expedition of that year to Grenada.

The instruments employed were magnetometer Elliott No. 61, and Dip Circle Dover 83, belonging to the Science and Art Department.

The method of observation was similar to that adopted in the Magnetic Survey of the British Isles for epoch January 1, 1886, for which these instruments were also employed.

The results may be thus summarized:—

Station: August 1886	Inclination	Force		Declination
		Horizontal	Total	
St. George, Grenada ...	40° 54'·7	3·1093	4·1144	0° 41'·5 E.
Hog Island, Grenada ...	41 14·1	3·1000	4·1223	0 51·5 E.
Island of Carriacou ...	—	3·0771	—	0 16·3 E.

Linnean Society, April 4.—Mr. Carruthers, F.R.S., President, in the chair.—Mr. D. Morris exhibited a specimen of the hymenopterous insect, *Eulema cayennensis*, concerned in the fertilization of *Coryanthes macrantha* (see Crüger, Journ. Linn. Soc., viii. 129), and obtained from Mr. Hart of Trinidad. Referring to the illustrations of the structure of the flowers, given in the *Gardener's Chronicle* (xvii., 1882, 593; and xxiii., 1885, 145), Mr. Morris explained the process carried out by the insects, chiefly bees, in removing the pollinia and subsequently attaching them on the stigma. The observations of Crüger had been verified by Mr. Hart in the Botanic Gardens, Trinidad.—Sir Edward Fry exhibited and made some instructive remarks on a copy of Grisley's "Viridarium Lusitanicum," 1661, presented by Linnæus to his pupil Loefling, the author of the "Iter Hispanicum."—Prof. R. J. Anderson exhibited some photographs of educational museum cases in Queen's College, Galway.—A paper was read by Mr. Lister on the Myxomycetes, or Mycetozoa, a group of organisms on the borderland between the animal and vegetable kingdoms, and formerly classed with Fungi. His remarks were illustrated by numerous coloured drawings of representative species, and the author also exhibited under the microscope the swarm cells from the spores of *Amaurochæte* and the streaming plasmodium of *Badhamia*. Attention was especially directed to the mode of feeding of the swarm cells and observations made on those of *Stemonitis*, where large bacilli were seen to be caught by pseudopodia projected from the posterior end of the organism, and drawn into its substance and digested. An interesting discussion followed, in which the President, Prof. Marshall Ward, Prof. Howes, and Mr. Breese took part.—A paper was then read by Mr. E. W. Hoyle, on the deep-water fauna of the Firth of Clyde, embodying the results of recent investigations. The explored area, which is shut off from the Irish Sea by a submarine plateau extending from the Mull of Cantyre to the Ayrshire coast, contains seven distinct deep-water basins, in which the depth exceeds 20 fathoms, and in some cases reaches 80 or 100 fathoms. An account was given of the dredging which had been carried on, with lists of the species obtained at various depths. A discussion followed in which Messrs. John Murray, W. P. Sladen, and G. B. Howes took part.

Chemical Society, March 21.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—The molecular weights of metals (preliminary notice), by Prof. W. Ramsay, F.R.S. The molecular weights of a number of metals have been determined by Raoult's vapour-pressure method, viz. by ascertaining the depression of the vapour-pressure of the solvent produced by a known weight of dissolved substance, the relation between molecular weight and depression being expressed by the equation, $W = \frac{W' \times P \times p}{100 \times d}$, where W is the molecular weight to be found; W' the molecular weight of the solvent; P/100 the percentage weight of the dissolved substance in solution; p the vapour-pressure of the solution; and d the depression in the vapour-pressure of the solvent produced by adding the substance dissolved. The solvent employed was liquid mercury; the temperatures 260° and 270° for a few substances, and the boiling-point of mercury for most. The results obtained are as follows:—

Element.	Atomic weight.	Molecular weight.		Molecular formula.
		Found.	Calculated	
Lithium . . .	7·02	7·00	7·02	Li_1.
Sodium . . .	23·04	12·1	11·52	$Na_{\frac{1}{2}}$.
Potassium . .	39·14	28·57	?	K_1 (?).
Silver	107·93	109·0	107·93	Ag_1.
Gold	197·22	204·3	197·22	Au_1.
Barium . . .	137·0	74·45	68·5	$Ba_{\frac{1}{2}}$.
Magnesium . .	24·3	20·33	24·3	Mg_1.
Zinc	65·34	60·85	65·43	Zn_1.
Cadmium . .	112·1	100·0	112·1	Cd_1.
Gallium . . .	69·9	68·1	69·9	Ga_1.
Thallium . . .	204·2	183·5	204·2	Tl_1 (?).
Tin	119·1	114·9	119·1	Sn_1.
Lead	206·93	204·1	206·9	Pb_1.
Antimony . .	120·2	134·2	120·3	Sb_1.
Bismuth . . .	208·1	209·2	208·1	Bi_1.

These numbers represent some of the actual results. Nevertheless, they must not be taken as absolute; although in many cases they are conclusive as to the molecular weight of the metal, still further experiments are needed. As an instance of what occurs, the metals thallium and antimony may be chosen. With thallium, for example, the following results were obtained:—

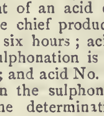
Percentage of thallium in amalgam.	Molecular weight.	
	Found.	Calculated.
0·8191	157·4	204·2
1·666	183·5	204·2
2·894	174·3	204·2
3·290	183·9	204·2

No appreciable change is produced on concentration. But with antimony, the molecular weight increases proportionately to the amount present, thus—

Percentage of antimony in amalgam.	Molecular weight.	
	Found.	Calculated.
1·117	134·2	120·3
1·526	155·4	120·3 to 240·6
2·257	193·4	120·3 to 240·6
3·289	294·6	240·6 to 481·2?

Here an association of atoms is evidently in progress. The results are based on the assumption that the molecular formula of mercury is Hg_1, in favour of which strong reasons can be adduced.—The application of Raoult's depression of melting-point method to alloys, by Messrs. C. T. Heycock and E. H. Neville. As a result of some preliminary experiments on the change in the solidifying point of tin caused by the addition of small quantities of other metals, the authors conclude that the dissolution of a metal in tin follows the same laws as that of compounds in other solvents, *i.e.* (1) that the fall in temperature of the solidifying point is directly proportional to the weight of metal added; and (2) that the fall of temperature is inversely as the atomic (molecular?) weight of the metal added. With tin, copper, silver, cadmium, lead, and mercury, the dissolution of one atomic proportion in 100 atomic proportions of tin caused a fall in temperature of the solidifying point varying from 2°·16 to 2°·67, with aluminium a fall of 1°·34, and with antimony a rise of 2°·0. In the discussion which followed the reading of these papers, Prof. Armstrong said that notwithstanding the apparent regularity and simplicity of the results, he was not prepared to accept them as in the least degree final. There was not sufficient evidence in his opinion that the effect observed was not in part at least the outcome of a change in the molecular composition of the solvent. The results obtained by Raoult's methods were, he thought, comparable with those obtained by determining the specific heats of the elements; in the latter case the observations were undoubtedly made with masses of molecules, which probably were of varying degrees of atomic complexity, and yet the results were found to be such as to justify conclusions being drawn as to the relative magnitudes of

their fundamental constituents—the atoms. In the same way it was possible that the results obtained by Raoult's method by means of observations on the behaviour of molecular complexes might afford the means of deducing the relative magnitudes of the fundamental molecules comprising the complexes, but not of the actual complexes operated with. Mr. Crompton drew attention to Beckmann's recent experiments on the lowering of the freezing-point; these show that the true molecular weight was only obtained when solutions were used the concentration of which was allowed to vary only within certain narrow limits; and that if the solutions were too dilute the molecular weight obtained from the lowering of the freezing-point was too low, while if the solutions were too concentrated, it was too high. In some cases the variation of the number obtained with the concentration was enormous. Prof. Carey Foster remarked that much depended on the definition given of a molecule, whether it is defined as that smallest quantity capable of existence *per se*, or as that quantity which produces a given effect in depressing vapour-pressure, or freezing-point, &c. The two magnitudes were not necessarily the same. The relation observed could hardly be accidental; yet he thought that the value obtained might be a quantity connected with the molecular weight but not necessarily identical with it. Prof. Ramsay, in replying, said that substances in dilute solutions must be regarded as in the gaseous state, their molecules being so far distant from each other as not to exert appreciable attraction on each other; and as occupying but a small portion of the space they inhabit. It has long been argued that the molecular complexity of the gases, hydrogen, oxygen, and nitrogen, must be the same, inasmuch as these elements have equal coefficients of expansion within the widest limits of temperature. A similar argument applies to substances in dilute solutions; it is much more probable that they have a simple and similar molecular structure than that the molecules, if complex, dissociate to an equal extent on equal rise of temperature, or on equal alteration of concentration. As regards the empirical nature of Raoult's laws, it is paralleled by the empirical nature of Boyle's and Gay-Lussac's laws—that is, such laws are merely approximations to truth, and depend on the fact that the molecules are sensibly beyond the sphere of each other's attraction, and themselves occupy no appreciable space. Hence their inapplicability at high concentrations.—Some compounds of tribenzylphosphine oxide, by Dr. Collie.—Contributions to our knowledge of the isothiocyanates, by Dr. A. E. Dixon.—The constitution of primuline and allied sulphur-compounds, by Mr. A. G. Green.—The determination of the constitution of the heteronucleal $\alpha\beta$- and $\beta\beta$-di-derivatives of naphthalene (second notice), by Prof. H. E. Armstrong and Mr. W. P. Wynne. Three heteronucleal α-chloro-β-naphthylaminesulphonic acids can be obtained when 1 : 2-α-chloro-β-naphthylamine hydrochloride is sulphonated with four times its weight of an acid containing 2 per cent. of SO_3. Acid No. I. is the chief product when the sulphonation is effected at 70° during six hours; acid No. II. is almost the sole product when the sulphonation is allowed to continue for a further six hours at 100°; and acid No. III. is obtained, together with acid No. II., when the sulphonation is effected at 160° during six hours. The determination of the constitution of the heteronucleal $\alpha\beta$- and $\beta\beta$-di-derivatives of naphthalene is arrived at from a study of these acids in the following way. Adopting the conventional symbol for naphthalene, with the α- and β-positions indicated, and numbering the positions 1, 2, 3, 4, 1′, 2′, 3′, 4′, as shown—



it is obviously possible to determine the relative positions of the three radicles, Cl, NH_2, and SO_3H, in a heteronucleal chloronaphthylaminesulphonic acid by determining the relative positions of the three pairs of radicles, Cl and NH_2, Cl and SO_3H, and NH_2 and SO_3H, and on this result to base the absolute orientation of the radicles, provided that it can be shown how the radicles are situated in any one of the two pairs of hetero-di-derivatives obtainable from the tri-derivative. No absolute method, free from reproach, has yet been devised for determining the constitution of any known hetero-di-derivative of naphthalene, but in the case of the hetero-$\alpha\alpha$-di-derivatives arguments may be advanced which are of such weight as to leave but little room for doubt that the $\alpha\alpha$-di-derivatives corresponding in constitution with ζ-dichloronaphthalene (m.p. = 83°) have the constitution 1 : 1′; the $\alpha\alpha$-di-derivatives corresponding in constitution with γ-dichloronaphthalene (m.p. = 107°) having then, by exclusion, the alternative formula 1 : 4′. The constitution of α-chloro-β-naphthylamine, and consequently the relative positions of the radicles Cl and NH_2 in the three sulphonic acids derived from it, was determined by the authors to be 1 : 2 (cf. NATURE, December 13, 1888, p. 166); the relative positions of the radicles Cl and SO_3H in the three acids were determined by replacing the NH_2 radicle by H by von Baeyer's hydrazine method, and converting the resulting heteronucleal chloronaphthalenesulphonic acids into the corresponding dichloronaphthalenes by treatment with phosphorus pentachloride, and the relative positions of the radicles NH_2 and SO_3H were ascertained by replacing the Cl by H by reduction with sodium amalgam, and converting the resulting heteronucleal naphthylaminesulphonic acids, first into the corresponding chloronaphthalenesulphonic acids by Sandmeyer's method, and finally into the corresponding dichloronaphthalenes. Acid No. I. yields, by displacing NH_2 by H, a chloronaphthalenesulphonic acid corresponding in constitution with γ-dichloronaphthalene, and therefore contains the radicles Cl and SO_3H in the positions 1 : 4′; it follows then, since the radicles Cl and NH_2 are in the positions 1 : 2, that the radicles NH_2 and SO_3H must be in the positions 2 : 4′. The naphthylaminesulphonic acid obtained from No. I. acid by reduction was found to be identical with Dahl's modification of β-naphthylamine-α-sulphonic acid, and to yield a dichloronaphthalene identical with so-called η-dichloronaphthalene (m.p. = 48°), so that these and all corresponding heteronucleal $\alpha\beta$-di-derivatives are proved to have the constitution 2 : 4′. Acid No. III., on displacing NH_2 by H, yields a chloronaphthalenesulphonic acid corresponding in constitution with the Badische modification of β-naphthylamine-α-sulphonic acid and with the heteronucleal $\alpha\beta$-dichloronaphthalene melting at 63°·5, and this must, by exclusion, be the 1 : 2′-compound, since the only alternative formula for a heteronucleal $\alpha\beta$-di-derivative, viz. 1 : 3′ or 2 : 4′, has been proved above to belong to $\alpha\beta$-di-derivatives corresponding in constitution with η-dichloronaphthalene (m.p. = 48°). Inasmuch, then, as acid No. III. contains the radicles Cl and SO_3H in the positions 1 : 2′, and the radicles Cl : NH_2 in the positions 1 : 2, it follows that the radicles NH_2 and SO_3H must be in the positions 2 : 2′. The naphthylaminesulphonic acid obtained from No. III. acid by reduction was found to be identical with Bayer and Duisberg's modification of β-naphthylamine-β-sulphonic acid, and to yield a dichloronaphthalene identical with δ-dichloronaphthalene (m.p. = 114°), so that these and all corresponding heteronucleal $\beta\beta$-di-derivatives are proved to have the constitution 2 : 2′. Acid No. II., on displacing NH_2 by H, yields a chloronaphthalenesulphonic acid convertible into η-dichloronaphthalene; it follows, then, that it contains the radicles Cl and SO_3H in the positions 1 : 3′, and since the radicles Cl and NH_2 are in the positions 1 : 2, the radicles NH_2 and SO_3H must be in the positions 2 : 3′. The authors could not, however, succeed in isolating this naphthylaminesulphonic acid owing to the peculiar behaviour of acid No. II. on reduction with sodium amalgam, but they have been able by other methods to place it beyond doubt that the acid when isolated would be found identical with Brönner's β-naphthylamine-β-sulphonic acid, and would yield a dichloronaphthalene identical with ϵ-dichloronaphthalene (m.p. = 135°), hence these and all corresponding heteronucleal $\beta\beta$-di-derivatives have the constitution 2 : 3′. These results not only render it possible to determine the constitution of the heteronucleal $\alpha\beta$- and $\beta\beta$-di-derivatives of naphthalene, but also afford a method of ascertaining the constitution of the two heteronucleal sulphonic acids obtained on sulphonating 1 : 2 dichloronaphthalene, and of the three trichloronaphthalenes which have been obtained from the three chloronaphthylaminesulphonic acids; the method is being extended by the authors to all the known chloronaphthylamines for the purpose of determining the constitution of the sulphonic acids obtained by them in characterizing the corresponding dichloronaphthalenes. The results, moreover, establish the correctness of the opinion, long held and frequently expressed by the authors, based on the higher melting-points of the "uniform" ϵ-di-derivatives (*i.e.* di-derivatives containing two similar radicles) in comparison with the isomeric δ-di-derivatives, that the ϵ-di-derivatives are symmetrically constituted; this conclusion, in

fact, is probably the most important outcome of the experiments, since it affords conclusive proof of the influence of symmetry as the determining cause of high melting-point, slight solubility, &c., in the case of naphthalene derivatives.—Contributions to the knowledge of citric and aconitic acids, by Mr. S. Skinner and Dr. S. Ruhemann.

Entomological Society, April 3.—Mr. F. Du Cane-Godman, F.R.S., Vice-President, in the chair.—Mr. Osbert Salvin, F.R.S., exhibited specimens of *Ornithoptera trojana*, Staud., and *O. plateni*, Staud., received from Dr. Staudinger, and obtained in Palawan, an island between Borneo and the Philippines. He remarked that *Ornithoptera trojana* was allied to *O. brookiana*, Wall.—Mr. R. McLachlan, F.R.S., exhibited, and made remarks on, seven examples of *Æschna borealis*, Zett., a little-known species of European Dragon-flies. He said that some of the specimens were captured by himself at Rannoch, in June 1865. The others were taken in Luleå, North Sweden, and the Upper Engadine (5000–6000 feet), in Switzerland.—Mr. W. H. B. Fletcher exhibited specimens of *Agrotis pyrophila* from various localities, including two from Portland, three from Forres of a smaller and darker form, and a melanic specimen from Stornoway at first supposed to belong to *A. lucernea*, but which, on closer examination, was seen to be referable to this species. He also exhibited series of *Triphæna orbona* from Stornoway and Forres, and *T. subsequa* from Forres and the New Forest. The specimens of *T. subsequa* from Forres were more distinctly and richly marked than those from the New Forest, and were also rather more variable in colour.—Dr. Sharp exhibited specimens of *Proculus goryi*, Kaup, found by Mr. Champion in Guatemala, prepared to show the rudimentary wings under the soldered elytra. Dr. Sharp called attention to the existence of a peculiar articulated papilla at the base of one of the mandibles; and he also showed sections of the head of *Neleus interruptus* displaying this papilla, as well as the articulated teeth on the mandibles.—The Rev. Canon Fowler exhibited specimens of *Agapanthia lineatocollis*, Don, and remarked that they were able to produce a distinct stridulation by the movement of the head against the prothorax, and of the hinder part of the prothorax against the mesothorax. He further remarked that Dr. Chapman had lately informed him that *Erirrhinus maculatus*, F., had the power of stridulating strongly developed.—Mr. Edward Saunders exhibited, on behalf of Mr. G. A. J. Rothney, in illustration of his paper on Indian Ants, specimens of the following:—*Camponotus compressus* and fragments of *Solenopsis geminatus* destroyed by it; *Camponotus* sp. (?), with a mimicking spider (*Salticus* sp.); *Pseudomyrma bicolor*, with its mimicking *Salticus*, and a new species of *Rhinopsis*, viz. *ruficornis*, Cameron, also found with it, and closely resembling its host; *Diacamma vagans; Holcomyrmex indicus*, with specimens of the grain which it stores and the chaff which it rejects; and a species of *Aphænogaster*, with the pieces of *Mimosa* with which it covers its nest.—Mr. G. A. J. Rothney communicated a paper entitled "Notes on Indian Ants."—Mr. Lionel de Nicéville communicated a paper entitled "Notes regarding *Delias sanaca*, Moore, a Western Himalayan Butterfly."—Captain H. J. Elwes communicated a note in support of the views expressed by Mr. de Nicéville in his paper.

Geological Society, April 3.—Mr. W. T. Blanford, F.R.S., President, in the chair.—The following communications were read:—The elvans and volcanic rocks of Dartmoor, by Mr. R. N. Worth.—The basals of Eugeniacrinidæ, by Mr. F. A. Bather. Although Profs. Beyrich and von Zittel had alluded to certain specimens of *Eugeniacrinus* as proving, by the course of the axial canals, that in this genus the basals had passed up into the radials, yet the two chief authorities who subsequently discussed the subject practically ignored this argument. M. de Loriol contented himself with denying any trace of basals, while Dr. P. H. Carpenter maintained that the top stem-joint represented a fused basal ring. In a previous paper the author had argued in favour of Prof. von Zittel's view without convincing Dr. Carpenter of its correctness. Such scepticism was, no doubt, warranted by the lack of detailed description and of figures. The object of the present note was to set the matter at rest by describing and figuring certain dorsal cups of *Eugeniacrinus caryophyllatus* kindly lent to the author by Prof. von Zittel. Owing to the mode of fossilization the canal system is plainly seen. The axial canal passes up into the radial circlet and gradually widens; at a short distance below the floor of the calycal cavity it gives off five interradial branches; these soon bifurcate, and the adjacent radial branches converge. Before they meet, each radial branch gives off a very short branch; this connects the radial branch with the ring-canal that contained the interradial and intraradial commissures. The evidence of all other crinoids that have these canals shows that the basals always contain the interradial branches. And in *Eugeniacrinus*, since the interradial branches have their origin in the middle of the radials, the basals must have passed up in between the radials. The President, Mr. P. Sladen, and Dr. Hinde took part in the discussion which followed the reading of this paper.—On some Polyzoa from the Inferior Oolite of Shipton Gorge, Dorset, by Mr. E. A. Walford.

Mathematical Society, April 11.—Mr. J. J. Walker, F.R.S., President, in the chair.—The Secretary read the following papers:—On the free vibrations of an infinite plate of homogeneous isotropic elastic matter, by Lord Rayleigh, F.R.S.—On the constant factors of the theta series in the general case $p = 3$, by Prof. F. Klein.—On the generalized equations of elasticity and their application to the theory of light, by Prof. K. Pearson.—On the reduction of a complex quadratic surd to a periodic continued fraction, by Prof. G. B. Mathews.—Construction du centre de courbure de la développée de contour apparent d'une surface que l'on projette orthogonalement sur un plan, by Prof. Mannheim.—Mr. Kempe and the President made short communications.

BERLIN.

Physical Society, March 22.—Prof. du Bois-Reymond, President, in the chair.—Dr. Assmann gave an account of the results he had obtained by a microscopic examination of the structure of rime, hoar-frost, and snow. In opposition to the view most usually held, that the solid condensations of aqueous vapour from the air are crystalline, he had observed some years ago, during a sojourn in winter on the Brocken, that hoar-frost consists of amorphous frozen drops, which, by their juxtaposition in rows, build up the long needles of which it is composed. He observed the same structure in some rime which he had collected from very various objects in December last during a cold which was not at all intense; in this case also the spicules of ice were composed of amorphous drops of ice frozen together in lines. In one case the little masses of ice which composed the rime were frozen together into a leaf-like structure. At the same time some small, scattered, and glittering ice-formations which had been formed in large numbers on the ground were crystalline in structure, consisting of thicker or thinner six-sided tablets or somewhat elongated prisms. On other occasions he found that the rime was itself composed of unequally developed crystalline structures, which branched at angles of 60°, and thus gave rise to a dendritic formation; at the same time the hoar-frost was also composed of crystalline structures. He had also succeeded in forming ice-flowers artificially on a pane of glass, and had satisfied himself by a microscopic examination of the same that they are always crystalline in structure. The structure of snow was investigated on the snow-garlands which had been described at a meeting of the Meteorological Society, and consisted of amorphous granules, such as compose the upper surface of a glacier. Dr. Assmann attributes the formation of rime and of hoar-frost to the existence of over-cooled drops of water, which suddenly solidify when driven by the wind against the solid substructure on which they are found. On the other hand, solid transparent ice is formed when water at 0°, or some temperature above zero, comes in contact with any solid object whose temperature is very low.—Prof. Liebreich exhibited a series of experiments intended to explain the occurrence of the inert layer in chemical reactions. Two years ago he had demonstrated to the Society the chief phenomena of its occurrence, as seen when a solution of sodium carbonate is mixed with chloralhydrate. When this is done the larger part of the mixed fluids very soon becomes milky, owing to the formation of innumerable small drops of chloroform, while at the same time a thin layer on the surface of the fluid remains clear: this clear portion is the inert layer, and is bounded above by the general meniscus of the mixture and below by a curved surface, whose convexity is turned upwards towards this meniscus. The speaker had, by means of a series of experiments, disposed of the view which had been put forward that the inert layer is only a portion of the mixed fluids from which the chloroform had evaporated. Of these experiments it may suffice to mention only one, in which the fluid was poured into a flat, open basin until it projected with a convex surface above the edges of the basin. Notwithstanding the larger

fluid-surface thus exposed no inert layer was to be seen. Similarly he had been able to show, by observations under the microscpe, that the phenomenon cannot be explained by any vortex movements in the fluid. Further, the assumption that it is due to a solution of alkali from the glass, which then prevents the precipitation of the chloroform, had been excluded by using a vessel made of quartz crystal. Prof. Liebreich inclined to the view, on the basis of his past experiments, which, however, must be further followed and extended, that the suppression or slowing of the chemical reaction at the surface of the fluid, which gives rise to the inert layer, is determined by the greater solidity and resistance of this part of the liquid.

Meteorological Society, April 2.—Prof. von Bezold, President, in the chair.—Prof. Börnstein spoke on the ebb and flow of the tide. After explaining the nature of the moon's action on the fluid part of the earth's surface, and showing that the flood is essentially due to a diminution of gravity and the ebb to its increase, he passed on to the consideration of the moon's attraction as it affects the atmosphere. Many experiments have been made with a view to proving the influence of the moon on the atmosphere, and at various places observers have succeeded in establishing a daily variation in the pressure of the air dependent upon the moon, and showing two maxima and two minima; these places are Singapore, St. Helena, Melbourne, and Batavia. The amplitude of the variation amounted to from 0·079 to 0·2 mm. But opposed to these are the observations of Laplace on the variations of the barometer in Paris, as also of Kreil in Prague, and further, Bessel's observations on atmospheric refraction. All these last-named observers found that the action of the moon on the earth's atmospheric envelope was either *nil* or else the reverse of that described above. Prof. Börnstein then discussed the question whether any ebb and flow of the atmosphere could possibly be detected with the means now at our disposal, and showed that the mercurial barometer can never be able to give indications of any such action, since it is itself affected by the alterations of gravity which are due to the varying position of the moon. He explained the phenomena observed at the four stations mentioned above as due to the fact that they are situated either on the sea-coast or on islands, at places on the earth's surface at which the ebb and flow of the sea is very considerable. The ebb and flow of the sea acts secondarily on atmospheric pressure, especially by means of the alteration of surface, and give rise to corresponding increases and diminutions in that pressure. Paris, Prague, and Königsberg are, on the other hand, inland stations, at which the barometer cannot be affected by any variations in the level of the sea's surface.

STOCKHOLM.

Royal Academy of Sciences, March 14.—On the essential results of the mathematical paper for which M. Poincaré received the mathematical prize of the King of Sweden, by Prof. Mittag-Leffler.—Derivation of some independent expressions of the Bernouillian numbers, by Dr. A. Berger.—On the plane curves which may be rectified through Abel's integrals of the first kind, by Dr. J. Brodén.—On the conform delineation of a paraboloid on a plane, by Herr H. von Koch.—On some remarkable minerals formed at a later period in the primordial strata of Sweden, by Baron Nordenskiöld.—Sur la chaleur latente de vaporisation de l'eau et la chaleur spécifique de l'eau liquide, par Dr. N. Ekholm.—New observations on the variation of the shape of the first abdominal appendices of the female crawfish, by Dr. Bergendahl.—Contributions to the anatomy of the Trematode genus *Apoblema Dujardin*, by Herr H. Juel.—Ascomycetæ from the Isle of Öland and from Östergötland, by Herr C. Starbäck.—On some triazol derivatives, by Dr. J. A. Bladin.—On bisphenyl-methyl-triazol, by the same author.—On the molecular weight of maltose and some inulinoid carbohydrates, by Dr. Ekstrand and Herr R. Manzelius.—Annotations on some European Orthotricha, i., by Lector Grönvall.—Déterminations des éléments magnétiques dans la Suède méridionale, by Herr W. Carlheim-Gyllenskiöld.—Formulas and tables for calculation of the absolute perturbations of the planets, by Herr Masal.

AMSTERDAM.

Royal Academy of Sciences, March 30.—Dr. Van den Sanden in the chair.—M. Forster stated the results of some experiments made in his laboratory, by Mr. Hunder Stuart and Mr. Fraser Ewman, on the presence of bacteria in the intestines. Mr. Stuart found that ordinary, and Mr. Ewman that typhoid, bacteria, introduced into the stomach along with the food, are discovered ter only in the lowest part of the smaller intestine and further in the large intestine. Ordinary bacteria have, therefore, no influence on the digestive process.—By means of known properties of polar systems and of elementary reasoning about reality, M. Schoute proved geometrically that the co-variant of Hesse, belonging to a binary equation with real co-efficients, is negative for the values of the variable that correspond to the real roots of the equation, independently of the number of its real roots; this is an extension of Dr. F. Gerbaldi's theorem (compare *Rendiconti di Palermo*, tome iii. p. 22).—M. J. A. C. Oudemans read a paper on the present state of the methods for determining the parallaxes of fixed stars.

VIENNA.

Imperial Academy of Sciences, February 21.—The following papers were read:—On the specific brightness of colours, a contribution to the physiology of visual sensations, by F. Hillebrand.—On the law of the decreasing of the power of absorption at increasing thickness of absorbent layers, by W. Müller-Erzbach.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

E. Museo Lundii, Part 1 (Copenhagen, Hagerups).—The Bacteria in Asiatic Cholera: E. Klein (Macmillan).—Systematic Account of the Geology of Tasmania: R. M. Johnston (Tasmania).—Life; what it is sustained by, and Cognate Subjects: W. Boggett (Trübner).—Proceedings of the London Mathematical Society, vol. xix. (Hodgson).—Flora Orientalis, Supplementum: R. Buser (Geneva, Georg).—Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung, 1888 (Magdeburg).—Wild Life in a Southern County: new ed., R. Jefferies (Smith, Elder).—The Structure and Distribution of Coral Reefs, 3rd edition, with an Appendix by Prof. Bonney: C. Darwin (Smith, Elder).—Statics for Beginners: J. Greaves (Macmillan).—The Anatomy of Astrangia danæ, six lithographs from drawings by A. Sonrel; explanation of plates by J. W. Fewkes (Washington).—New Zealand Meteorological Report, 1885 (Wellington).—Proceedings of the Geologists' Association, February (Stanford).—Journal of the Institution of Electrical Engineers, No. 78 (Spon).—Quarterly Journal of Microscopical Science, April (Churchill).—Brain, No. 44 (Macmillan).—Botanische Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie, Zehnter Band, v. Heft, Elfter Band, i. Heft (Leipzig, Engelmann).—Bulletin of the American Geographical Society, vol. xxi. No. 1 (N.Y.).—Bulletin from the Laboratories of Natural History of the State University of Iowa, vol. i. No. 1 (Iowa).—Journal of the Royal Statistical Society, March (Stanford).—Journal of the Royal Microscopical Society, April (Williams and Norgate).

CONTENTS.

THURSDAY, APRIL 25, 1889.

THE SURFACE OF THE EARTH.

Das Antlitz der Erde. Von Eduard Suess. Mit Abbildungen und Kartenskizzen. Erste Abtheilung, 1883: Zweite Abtheilung (Schluss des I. Bandes), 1885. (Prag: F. Tempsky. Leipzig: G. Freytag.) Zweiter Band. Mit 42 Text-Abbildungen, 1 Tafel, und 2 Karten in Farbendruck. (Prag: Wien. Leipzig: F. Tempsky, 1888.)

THE varied attainments of geographers have enabled them to express emphatic views on the scope of their subject. Each in turn has augmented knowledge of the earth's surface till it has become difficult to distinguish the contributions of mathematician and astronomer, physicist and physical observer, geologist and naturalist. Many geologists have been eminent as geographers, and Lyell and Humboldt gave the subject an enduring scientific importance by teaching the effects of geological causation in shaping the earth's surface. Every geologist is aware how the light of geological structure illuminates the problems of mountain form, position, and relation to surrounding land; but never till now has an author attempted to narrate the geographical story and history of the earth's surface from a geological point of view. Prof. Suess has brought to the subject the qualifications of a great teacher, who realizes that science has a duty to make itself available to the unlearned, no less than to aid the researches which are yet to be made; and he has conceived of the earth's surface in a new, forcible way, which stimulates alike imagination and thought, and lays before the reader a wide knowledge of fact. This work, which we know has occupied the author for the past twelve years or more, can scarcely be judged of as a whole, because the third volume, whose subject gives a title to the treatise, is unpublished; but we may say that a more luminous and profound endeavour to place the elements of scientific geography before the general reader has not been made. We may perhaps think the subjects discussed need the aid of more figures to enable the reader to think as the author thinks, and attain a similar command of his facts. The aim of the work is to lead the reader through a consideration of the movements in the outer layers of the earth's crust which are manifested at the present day, and in the first half of the first volume the more striking phenomena are narrated, which are associated with volcanic disturbance and earth movements. The second part of the volume examines the structure and construction of mountain chains. The author naturally takes the Alpine system first, as nearest to the Austrian people, and then treats of the depressions, like the Adriatic and Mediterranean, associated with the prolongations of the Alpine system. Successive chapters tell the story of the mountain structure of Southern Africa and the Sahara, of Central Asia and the Malay Islands, and of the mountain systems of America and the West Indies. Thus, by raising the mountain chains, the author leads up, in the final chapter of the first volume, to a discussion of the nature and origin of continents, no less than of their relations to the seas from which they emerge.

The introduction is an introductory lecture, not written like a syllabus, for that is given in the table of contents, but designed to introduce the reader to conceptions of a large kind on which the scientific aspects of geography are based. The distribution and forms of land masses and depths of the oceans are shown to be effects of movements of the earth-rind which have varied through successive geological ages and so changed the distribution of life.

The work begins with an exposition of the Flood and ancient Babylonian cosmogonies, and the author states that the Deluge was connected with the lower Euphrates and flooding of the low land of Mesopotamia, owing probably to an earthquake combined with a cyclone from the south. The interest of the unlearned reader is thus insured. An excellent account is given of earthquake phenomena, followed by a chapter on "Dislocations," which are defined as resulting from decrease in the volume of the earth, and as comprising horizontal and vertical movements. The crumpling, contortion, and folding of the rocks of mountain masses is classified as consequent on tangential thrust, sinking, and the combined effects of these actions. Volcanic phenomena occupy the next chapter, and are regarded as dependent on the formation of radial fissures. These general studies completed, we turn in the second part of the first volume to the mountains of the earth. The Russian table-land is described as consisting of granite in Finland, on which rest Silurian and Devonian, and successively newer rocks stretching under the Carpathians; but it is uncertain whether the newer rocks of this plain extend under the Bug. The Sudetic Alps are described as closely linked in origin with the Russian table-land and the Carpathians. The Franconian-Suabian basin follows, and leads to a discussion of the system of the Alps, which is regarded as beginning with the Carpathians, as curving south with the Jura, being prolonged south-east with the Apennines, and then continued west through North Africa to the pillars of Hercules. The plain of Hungary is compared to the eastern half of the Mediterranean. The Adriatic basin is closely connected with the Plain of Lombardy, both being defined by the Apennines, which, though at one time independent of the Alps, have now become connected with them. These mountains rise in an unbroken, steep curve facing those depressions, and looking like an outer border of the Alps thrust up above the level of the deep-lying basins. The history of the Mediterranean Sea is illustrated by the newer Tertiary deposits and the life which they contain. At first the sea reached the central plateau of France, the valley of the Rhone, Styria, Switzerland, Hungary, and Transylvania, stretching east to the source of the Euphrates and Northern Persia. The second phase of the Mediterranean was excluded from Switzerland and the valley of the Danube, and extended over the Russian plateau to Kherson and into Asia Minor. Later still it filled the valley of the Rhone, and approximated to its present outline, being excluded from Asia Minor; but the Ægean Sea was a fresh-water lake. The distribution of these deposits in North Africa is shown by means of a map in the chapter on the Sahara and Egyptian deserts; and evidence of the antiquity of the fauna of the Nile is found in the distribution of its life in Syria and Arabia. The

author now passes to South Africa, the Indian peninsula, and Madagascar, which have the characters of a table-land which was once continuous, and from their geological history the sediments are derived which constitute the Cretaceous and Tertiary rocks of the Sahara and Arabia. The high land to the north of India is fully described, chain by chain, in the details of geological structure which govern their forms and continuity, leading to the volcanic regions of Java and Sumatra. There appear to be four great curves southward, seen in the Iranian chain, Hindu Kush, Himalayas, and the Malayan chain. The connection between the mountains of Central Asia and Europe is traced on the basis of the distribution of Tertiary strata : all the European mountains are regarded as continuations of chains which extend from the Thian Shan group. The continuity here sought to be established would appear to depend not less on the evidence of denudation than upon stratigraphical proofs of contemporary folding. The author, for example, carries the line of Cyprus through Crete into the Dinaric Alps, while the chain of the Caucasus passes through the Crimea and Balkans by Ovsova into the Carpathians ; but these curved lines seem to us rather the results of denudation of folded rocks than an indication of the directions in which the lines of folding were prolonged.

Three chapters are devoted to North and South America and the West Indies, treating of the rock-structure and folding of the earth-rind in the same way as in other parts of the globe. The rind appears to the author as more plastic or more symmetrically bent than we have been accustomed to regard it, insomuch that he everywhere finds the chains curved, whereas we often find them extending at angles to each other. The volcanoes of the West Indies are found to have the same relation to the main chain as have those of the Apennines and Carpathians, being on the inner side of the arch. And the Caribbean Sea is further compared with the western part of the Mediterranean.

The second volume is devoted to the oceans, and commences with an historical account of views held by successive investigators on the displacement of the shore-line. Three lines of investigation suggest themselves : first, the changing distribution of the seas in successive periods of time, which may be furnished by evidence of shore-lines and sea-margins ; secondly, by comparing the areas of deposition of sediments in past time, some idea is obtained of the ancient oceans ; and thirdly, by studying existing shores, evidence is found of oscillations in level. Beginning with the great waters of the present time, a detailed history is given of the Atlantic. Its shores are described, especially in the northern regions, and the mountains which extend towards the European coast are traced, and said to be paralleled by chains on the other side of the Atlantic, so that the old rocks of Ireland, Cornwall, and Brittany, appear to extend beneath the ocean. The west coast of Africa, and shores of Central and South America, are similarly described and compared in the light of their geological structure. The Serro do Mar in South America is regarded as comparable with the Appalachian chain, and hence it follows that the entire American continent is the consequence of a tangential thrust towards the Pacific Ocean. The history of the Pacific Ocean begins with the shores of New Zealand and Australia, but the author avails himself of the fossil floras and other evidences to indicate ancient relations of the strata with the corresponding rocks of Europe, India, and other localities. The line is followed on through New Ireland, New Caledonia, Borneo, Cochin China, Tonquin, the Philippines, and Japan, and so by the Kurile and Aleutian Islands to the west coast of America, everywhere dwelling on the light thrown by the geological structure of those countries upon the variations in extension of the sea. The Atlantic and Pacific are compared with each other, and found to have many points of structure in common. The foregoing evidences of change in ancient oceans furnished by the rocks seen on their borders, necessitate a history of geological changes in the Palæozoic, Mesozoic, and Tertiary periods of time. In a chapter on Palæozoic oceans, it is stated that they make us aware of two continents which now only remain in fragments. The first occupied the North Atlantic Ocean, furnished the ancient sediments of Europe and America, and its remains persist as Greenland. The second continent is first recognizable at the end of the Carboniferous period. Its relics persist in Africa, India, and Australia. As the former is known as Atlantis, so the latter is named Gondwanaland. No seas of the Mesozoic period have left sediments which indicate great depth of water. The chalk alone may be evidence of a deep ocean, which stretched from Europe towards the West Indies in a yet earlier time. Towards the close of the Cretaceous period the seas became smaller. In North America the prairie lands from Canada to Texas and Alabama emerged from the water, and similar up-heaval is seen in Europe, and especially evidenced in the freshwater strata of the Pyrenees and Southern France, which make a transition from the Cretaceous to the Tertiary, like that shown by the Purbeck beds in early strata.

These earliest shallow conditions in Tertiary Europe were succeeded by a central sea which reached far to the east, and is estimated to have spread from London to Khartoum, and from Kiew to the Indian Ocean. And the author sketches with a bold hand the succession of physical conditions changing the breadths of water which have resulted in the contours of existing shore-lines in America and the Old World.

Evidences of the changes which have occurred in the contour of existing shores are found in detailed study of the coasts of Norway, and the inland terraces, which mark earlier extent of the sea and glacial action. Other evidences of instability of the shore-line are recorded with similar detail on the coasts of Italy. And the history of the Baltic and North Sea emphasize the mutability of shores. The historical records of the Mediterranean shores supply, especially in its eastern parts, striking proofs of oscillation in Greece, Syria, Egypt, and Asia Minor. The northern shores of the world supply many proofs that at the close of the Glacial period the shore-line was more elevated than it is now. An examination of the shores of the equatorial and southern seas demonstrates the former wide spread of deposits in which the life is substantially the same as in the nearest oceans, though there is sometimes, as in South America, a larger number of European species.

Finally, a summary is given of the characteristics and geological history of the oceans which have been de-

scribed. The southward direction of peninsulas is attributed to folding of the earth's crust, and to depression of the ocean floor, which has caused the water to predominate towards the south, so that they are always in relation to areas of depression. It will thus be seen that, in simplicity of conception, largeness and continuity of the ideas dealt with, amplitude and detail of the knowledge and inductions brought together and correlated, this work promises to be one of the most valuable contributions to the history of the earth which we possess. From the time when Godwin-Austen planned his work on the ancient physical history of Europe which geology supplies, data have been accumulating with a rapidity which has made the task almost hopeless, of writing a history of the earth's surface which should be at once exact in details and large in ideas. But Prof. Suess does not so much trench on geological history, which can only be told intelligibly when supported by masses of technical facts; for his aim is to impart vitality to learning and teaching of those phenomena with which the geographer is concerned. It may be too much to say that he attempts to do for the surface of the earth what Darwin did for the distribution and classification of life, because so much had been previously contributed with which his own work is in perfect harmony; but we may say that henceforth no geographical teacher can neglect to place before his pupils the methods and results which the author's work brings to his hand. And we may anticipate that much as Lyell's treatise, the "Principles of Geology," has laid the firm foundations of geological thought and of scientific observation in geographical science, so this treatise appears likely to mark a similar epoch in the history of geography, becoming a guide to its principles for students and readers.

It is significant that it is the outcome of long experience, first as Professor of Palæontology, then as Professor of Geology, on the part of one who has given many of the best years of his life to the endeavour to make practical application of geological knowledge in improving water-supply and navigation of the earth's surface which surrounds Vienna. The same thoroughness and devotedness with which these earliest of his public works were done are seen in this latest contribution to education; and we cannot but see that geography, as Prof. Suess teaches it, is a science based upon the sciences which he has himself professed, though expanding in its ultimate developments to include that knowledge which the naturalist and the observer of Nature record. Every chapter is followed by a long bibliography, in which the reader finds the more important original sources of information with which the writer has refreshed his memory; and the beautiful drawings and engravings scattered through the volumes will be not less welcome to the earnest student as presenting typical examples of the geological foundations of geographical truths seen on the earth's surface.

H. G. SEELEY.

NATURAL INHERITANCE.

Natural Inheritance. By Francis Galton, F.R.S. (London: Macmillan and Co., 1889.)

IT is related that, when some boastful patriot was once describing the trees in his country as so high that a man could hardly see their tops, a stranger retorted: "That is nothing to the trees in my country, which are so high that two men are required to see the top of them; one man looks as far as he can, and the other begins where the first stops." A similar division of labour would be required in order to survey adequately the imposing scientific edifice which Mr. Galton has constructed; based as it is on a foundation of geometrical reasoning, and culminating in the clouds of biological hypothesis. The parts which are nearest to *terra firma* are most within our ken. The mathematical foundation and the structure which rests immediately thereupon appear to us solid and elegant. The author has re-stated the law of error in a form adapted to sociological investigations. He says truly and happily:—

"This part of the inquiry may be said to run along a road on a high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach."

Mr. Galton reads a useful lesson to statistical practitioners, when he complains that they limit their inquiries to averages, without taking account of those deviations from the average which are the subject of the theory of errors.

"Their souls seem as dull to the charm of variety as that of the native of one of our flat English counties, whose retrospect of Switzerland was that, if its mountains could be thrown into its lakes, two nuisances would be got rid of at once."

Mr. Galton is not dead to the charms of "normal variability." Statistical theory illustrated by him becomes in a high degree fascinating:

"Not harsh and crabbed as dull fools suppose."

He may well say:

"Some people hate the very name of statistics, but I find them full of beauty and interest."

Some of his riders on the law of error may be interesting even to physicists. The following problem is not so familiar to astronomers, but that Mr. Galton's solution of it may deserve attention. Given three or four observations relative to an unknown quantity; and again another small group of observations made on some other quantity by the same instrument or method of observation; and so on, each of the different little groups not in general comprising the same number of observations: find from the residuals, or apparent errors, presented by the respective groups, the true "probable error" incident to the method of observation. Mr. Galton gives four solutions of this problem, of which two involve data which are special to his subject; two may be described as general. Neither of the latter coincides with the theoretically best possible method; but the consilience of their results with each other and with the other two methods is interesting.

We have worded the problems in terms of *errors.* The form in which it presents itself to Mr. Galton relates rather to the deviations of individuals from their common type. He is determining the "probable error" or dispersion of the heights of brothers compared with their mean. It proves to be much less than the corresponding constant for the adult population generally. The question arises in the course of an inquiry whether the mean height of brothers and sisters deviates from the general average of adults less than the height of their parents. There is a little difficulty in stating the question

owing to the difference in the mean stature of the sexes. Mr. Galton gets over this difficulty by multiplying all his female data by a proper constant—pushing them up, so to speak, to the male standard. Upon this understanding, suppose that the mean height of the father and the "transmuted" mother—the stature of the "mid-parent" in Mr. Galton's phraseology—differs from the mean height of the general population by say three inches. Then the mean height of the sons and "transmuted," or pushed up, daughters, is most likely to be not three inches, but two inches. The constant of "regression" is determined with equal precision for other relationships. A general idea is obtained of the extent to which the peculiarities of an individual are likely to be shared by his kith and kin.

It is not easy in a few words, or perhaps in any number of words unaccompanied by symbols, to do justice to the cogency and precision of this anthropometrical reasoning. The manipulation to which Mr. Galton's materials have been subjected by one of his mathematical coadjutors, Mr. Hamilton Dickson, fully attests their consistency and strength. Some additional corroboration may be afforded by the following considerations. The probable error or dispersion for the statures of adult men, which Mr. Galton has extracted from the family records submitted to him, is identical with, or differs only by a fraction of an inch from, the constant furnished by many other sets of measurements. The value here obtained for this constant is 1·7 inch. The same value is obtainable from the measurements made by Mr. Galton for the British Association. The same value has been obtained by Signor Perozzo for the whole of Italy, and for each of its provinces. The agreement of observations made under such different circumstances is calculated to give us confidence in the higher theory of anthropometry. The result which has been thus verified may be used to confirm Mr. Galton's reasoning at several points. Any scruples which he may suggest as to the discrepancy in the values of mean stature determined from his different records are removed by a consideration of the error or diversity to be expected among these results. Again, consider those tables in which Mr. Galton compares the heights of a number of persons with the mean height of their children or brothers, in which it is shown, for instance, that men of the height 71·5 inches have brothers averaging 70·2. That all the entries point in the same direction—namely, that of "regression"—is in itself adequate evidence of that fact. But not only are the faggots strong in their union, but also each individually is possessed of considerable strength. Thus, the discrepancy which we have just noticed between the height of a man and the mean height of his brethren, namely, 1·3 inch, is founded on eighty-eight instances. The chances against this degree of divergence occurring by accident are some hundreds to one. The odds that the appearance of law which the tables present is not accidental are immensely increased by this consideration. We should be curious to know whether Mr. Galton's experiments on the "regression" of sweet peas would admit of this sort of corroboration.

The human stature is a subject particularly well adapted to Mr. Galton's exact methods of measurement. Length admits of more exact gradation than the so-called secondary qualities. To arrange in a regular scale the colours of eyes which are variously described as dark blue, blue-green, hazel, and so forth, is a delicate task. How far Mr. Galton has triumphed over this imperfection of his data, it must be for specialists to decide. The student of probabilities cannot doubt that the correspondences between his observations and his calculations are indicative of a real law. The coherence of the table in which he compares fact and theory as to the number of light-eye-coloured children born to parents of various eye-colours cannot possibly be accidental. Ill-adapted as eye-colour may be to exact measurement, it is a more satisfactory quality to deal with than "the artistic faculty." Can we suppose that the compilers of the different family records which Mr. Galton has analyzed have employed at all similar standards, when they applied the epithets "artistic" and "non-artistic" to their relations? Our misgivings increase when we go on to apply the calculus to the returns as to disease which are obtained from the family records. To arrange parents and children in a graduated scale of "consumptivity," upon the testimony of unprofessional relatives, seems precarious. The author himself abandons the use of the more delicate methods when he goes on to consider "good and bad temper." He has not, however, shrunk from dividing into five degrees or classes some sixty shades of temper ranging from "amiable" and "buoyant" to "surly," "uncertain," "vicious," and "vindictive."

We ascend into a region of hypothesis when we speculate on the causes of the phenomena which have been evidenced. The attention of biologists should be called to Mr. Galton's views on "particulate inheritance," "latent characteristics," and the stability of organic forms. The conceptions which he has formed as to the processes of heredity are placed by him in a variety of lights, and illustrated by many happy analogies. "Appropriate and clear conceptions," it has been well said, are essential conditions of science. Mr. Galton has done much to make his abstract ideas clear, but are they also appropriate? This is a question upon which, perhaps, only a few specialists are competent to advance an important opinion; and their authority is liable to be impaired by the prejudices incident to an exclusive line of research. We shall be slow to accept adverse criticism from any whose studies may not have qualified them to appreciate the support which Mr. Galton's theories receive from his masterly use of the calculus of probabilities.

F. Y. E.

NATURE'S HYGIENE.

Nature's Hygiene: A Systematic Manual of Natural Hygiene, containing a Detailed Account of the Chemistry and Hygiene of Eucalyptus, Pine, and Camphor Forests, and Industries connected therewith. By C. T. Kingsett, F.I.C., F.C.S. Third Edition. (London: Baillière, Tindall, and Cox, 1888.)

THIS book aims at being a systematic manual of natura[l] hygiene. The introductory chapters deal in a popular manner with chemical principles and chemical changes, leading up eventually to questions affecting the chemistry and hygiene of the atmosphere, of water, of sewage, and of

numerous other subjects supposed to be included within the domain of natural hygiene. The second part of the book is devoted to what are called the sanitary properties of Eucalyptus-trees, of pine-trees, and of camphor forests. At first sight the general character of the work impresses the reader favourably. He is disposed to read it for the sake of acquiring information on subjects which force themselves on public notice in every large town. These are: the purification of water; the relations existing between micro-organic life and the so-called infectious or contagious diseases; the measures to be adopted for the disposal and treatment of sewage, and the relative value of certain antiseptics and disinfectants. The author devotes to these subjects numerous pages of information more or less relevant to them. At p. 217, however, the author arrives at the matured conclusion that "the only disinfectant which, while possessing all these characters, also acts upon anaërobic and aërobic forms of life alike, of which I have knowledge, is that known as ——, for the existence of which I am proud to take credit." Besides this disinfectant, there is an equally good "fluid," an equally good "oil," and an equally good "powder," for all of which, no doubt, the writer has also pride in taking credit. There are in all some thirty references to these specifics.

The concluding chapters, in which *Eucalyptus globulus* and other species of Australian gum-trees are credited with wonderful powers as "fever-destroying trees" on account of "the aromatic vapours which emanate from the trees, and the preservative powers of the branches and leaves which fall on the ground," repeat a well-known but scarcely established doctrine of hygienists. It is probable that any fast-growing tree, suited for swampy districts, would produce exactly the same or similar results. But, granted for the moment that the essential oil given off in a vaporous condition from Eucalyptus or pine-trees is disinfectant in character and conducive to health, we fail entirely to see how this can apply also to camphor-trees. Yet we are told that "the natural history of camphor forests affords us another and remarkable feature of Nature's hygiene; . . . that atmospheric oxygen is constantly being absorbed by the essential oils that are continuously evolved into the air, and this simple process gives rise to the production of a number of active chemical substances, including peroxide of hydrogen and soluble camphor, all of which purify the air and enhance the healthful influences of the climate." Those who are at all acquainted with camphor-trees will admit that this is a very fanciful picture indeed. In common with most members of the *Laurineæ*, the emanations from camphor-trees are neither agreeable nor balsamic. The author brings no evidence whatever to establish his position, and we beg leave to doubt the healthful influences of camphor-trees on the grounds stated until we have something more tangible than the mere assertion of the author of this work.

OUR BOOK SHELF.

Elementary Inorganic Chemistry. By A. Humboldt Sexton, F.R.S.E., F.I.C., F.C.S. (London: Blackie and Son, 1889.)

THE chief part of this manual of 320 pages is specially prepared for students who are guided by the elementary division of the Syllabus of the Department of Science and Art. In addition there are twenty-five pages about the metals and their compounds, a chapter of nine pages on what is called "Organic Chemistry," twenty-three pages of "Experimental Illustrations," a chapter on "Chemical Arithmetic," a series of questions, an "Elementary Course of Qualitative Analysis" occupying thirty pages, and a few less important matters.

The main part of the book is pretty much what one is accustomed to in elementary treatises: it is clear and calculated to be useful; but the chapters on the metals and on organic chemistry are obviously meagre to a degree. The organic part deals with those substances mentioned in the alternative course of the above-mentioned Syllabus, but it would have been much better for the book and the students who use it if these few pages had been omitted. The analytical course refers to eight metals and four acids. It is not stated why these substances are specially favoured.

It is a pity that those who write on a subject like elementary inorganic chemistry, which has been so prolific of text-books that practically speaking no exertion or thought is needed in the selection of topics or the manner of their treatment, should not more often concentrate a little attention upon the exactness of their expressions. The statement, for example, that "If hydrogen and oxygen or air be mixed, and a light be applied, they will combine with a violent explosion," is open to much censure. Do hydrogen and air ever combine? Will a mixture of hydrogen and oxygen as a matter of course explode under the circumstances described? Again, the statement that "Potassium and sodium only expel one-half of the H from water" may be legitimately described as untrue. The equation "$Na + H_2SO_4 = NaHSO_4 + H$" is more likely to deceive than instruct the student. These are not isolated examples.

A Class-book of Geography. By C. B. Clarke, F.R.S. (London: Macmillan and Co., 1889.)

THIS is a new and revised edition of Mr. Clarke's well-known class-book of geography, which was first published in 1878. The populations of towns have been brought up to date, as also the political geography of Egypt, Turkey, &c. The names of places which have lately become of importance in consequence of commercial enterprises, such as Baku, have also been added. Perhaps the most important addition, however, is a chapter on astronomic geography, which is very clear, though necessarily not very detailed. An excellent outline of chartography has also been added. The particulars given relating to each country are of the usual character. They include an historical sketch of each country, manufactures, minerals, animals and plants, languages, religions, and forms of government. At the end of each section is a condensed statement of the principal features of each country. A short description of the different races of animals might have been given with advantage, as at present the student can only gatner the meanings of such terms as "Pachyderms" and "Chiroptera" from the examples quoted. The omission of the word "species" in such a sentence as: "England possesses one *dormouse* and one *squirrel*," is rather apt to make one inquire as to the location of those favoured animals.

There are eighteen excellent double-page maps, but of course they are not so full of detail as is necessary for a complete study of the subject. This, however, is no great drawback in these days of cheap atlases.

Travel-Tide. By W. St. Clair Baddeley. (London: Sampson Low, 1889.)

THE writer of this volume has visited many different parts of the world, and here he sums up the impressions produced upon him by the most remarkable of the scenes with which he has made himself familiar. There is

nothing of strictly scientific interest in the book; but we may say that Mr. Baddeley has the great merit of always trying to see things with his own eyes, that in his judgments of men and places there is no trace of any kind of British prejudice, and that his style is fresh and interesting. Among the subjects of which he has something to say are Bulgaria, Buenos Ayres, Constantinople, and Tunis.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Large Fireball.

ON the night of Monday, April 15 last, at 12h. 26m., a meteor of very exceptional proportions was observed from many parts of the country. The full moon was shining at the time, and near its meridian passage, but the brilliancy of the fireball was such that it vividly illuminated the sky and landscape with a flash which many people mistook for sheet lightning. Several observers describe the meteor as larger and considerably more brilliant than the moon, and at Swindon and Ramsbury a detonation was heard. At the former place the meteor "appeared so close that people thought it descending upon the town; it startled the rooks out of the trees, and suddenly illuminated the country round like the electric light."

At Worthing the meteor is described as falling almost perpendicularly from west-north-west to due north. At Clapham it seemed to take a slanting course from the south towards the west. At Bath the meteor was seen in the east moving horizontally, at a considerable altitude, from right to left. Probably therefore the body was situated over the region of Berks, but the data are altogether too imperfect to admit of trustworthy inferences either as to its position or height above the earth's surface.

It will be important if other observers can furnish accounts in which the position and direction of this fireball are more definitely given. The phenomenon was clearly one of uncommon character, but its apparition occurred at such a late hour that comparatively few persons must have witnessed it.

Bishopston, Bristol, April 20. W. F. DENNING.

Variable Stars and the Constitution of the Sun.

IN NATURE of March 21 (p. 492), Mr. A. Fowler has given an excellent account of my theory of the constitution of the sun, but he has not succeeded so well in describing my theory of the variable stars. I may here draw attention to some of the cardinal points not sufficiently noticed by Mr. Fowler:—

(1) The proof that the chemical combination at the external layers of the stars must be intermittent with regular intervals. This proof, pp. 8, 9, and 10, is mentioned as the very basis of my theory of the sun and stars.

(2) The fact that the intermittent eruptions of heat, if produced in this manner, cannot become visible through some perceptible increase of the heat of the star.

(3) The high probability that in the case of some red stars the vapours, noticed by the spectroscope in their external layers, are cooled to their dew-point, and so, with the smallest radiation of heat, are made ready to change into clouds, which suddenly withdraw the invariable inner light from our view.

(4) The high probability, too, that only those stars will be variable in which the external layers are cooled down to their dew-point, and that the intermittent eruptions of heat become visible because at intervals they cause the evaporation of the clouds, which surround the invariable inner light of the star during the minimum.

Therefore only those stars of Class III. will be variable, whose external vapours are cooled to their dew-point. The others also have their periodical eruptions of heat, but these are inperceptible to our eye.

The changes of the variable stars, therefore, are never associated with important changes of temperature. It would be possible for them to take place without the least change of temperature if the calories produced by an eruption of heat were entirely used for the evaporation of the clouds. And so even an increase of temperature of 1° would be sufficient to make a seemingly extinguished orb glitter again as a new star, whilst a similar decrease of temperature would suffice to restore the veil, which, steadily growing thicker, would make it invisible again, perhaps for centuries together.

Mr. Fowler is mistaken in saying that I do not seem to be aware that Algol is one of the hottest stars in the heavens, and that its spectrum is the same at maximum as at minimum. On the contrary, on p. 15, I have stated the exceptional case of Algol, and, seeing the impossibility of making it agree with my theory as in the case of those Algol-stars which are red at minimum, I ascribed the variability of Algol to the periodicity of its spots. Moreover, I added that this seeming contradiction to my theory was only a physical peculiarity of little importance. For the spots, too, are caused by periodical eruptions of heat in clouds. The only difference is that the clouds—I mean on the sun and Algol—are photospheric, and by vaporization cause dark spots which diminish the light, whilst in the cooler red stars, the clouds form a dark veil round the star, and therefore by vaporization increase its interior light.

In this defence of my theory, gradually passing from the variable stars by means of Algol to the sun, I must observe that my theory by no means suggests, as Mr. Fowler thinks, that the sun should have more spots in the Polar regions than near the equator. I only say that the spots must be found in parallel zones; of the breadth of those zones I say nothing. The spots can only be produced in places where the temperature and the chemical compositions work together to produce eruptions of heat. As the places of equal chemical composition and of equal temperature are only possible in the photosphere in two parallel zones of equal latitudes on opposites sides of the equator, it is plain that the spots must be produced there.

I conclude with an expression of gratitude to the Editor of NATURE and to Mr. Fowler for the trouble they have taken in noticing my theory. A. BRESTER, Jz.

Delft, April 1.

IN reply to Dr. Brester I have to remark, in the first place, that I made no attempt to give all the details of the theory, limitations of space not permitting. One of my principal arguments against the theory was that, if it were true, *all* cool stars should be variable, and I still see no reason to alter my views. The observations of the red stars by Dunér show that the spectra of some of the stars which are not variable are identical with some of those which are. For example, the spectra of 120 Schj., and D.M. + 47°·2291, which are not variable, are exactly like those of χ Cygni and R Leonis. The *compositions and temperatures* of the gaseous surroundings of these bodies are therefore similar, and there is no reason, from Dr. Brester's point of view, why one should be variable more than another, since, if they are cooling, they all start cooling under exactly equal conditions. (It may fairly be assumed that the spectra of the variables have been generally taken at maximum.) The cooling to dew-point is therefore not in question in the variable any more than in the apparently invariable stars.

The high probability that by far the greater number of variables are uncondensed meteor-swarms which are increasing in temperature, as demonstrated by Mr. Lockyer, is also obviously against any theory of variability which assumes a state of cooling.

The same objections which apply to the red stars apply also to the "unimportant" case of Algol; there are many other stars with identical spectra, and therefore temperatures, which exhibit no variability at all.

With regard to the sun, I remarked that the theory would suggest that spots should be most numerous at the poles, for the reason that it would be there where the atmosphere in the neighbourhood of the sun would be coolest, and where, therefore, chemical combinations would be most likely to take place. To this Dr. Brester replies that his theory only requires that spots should be formed in equal zones on opposite sides of the equator, and says nothing about the breadth of the zones. Of course, if it be assumed that the substances present in Polar regions are not such as to form combinations competent to produce spots, the difficulty is overcome, but an explanation depending upon such an assumption is far from satisfactory.

London, April 5. A. FOWLER.

Tertiary Chalk in Barbados.

IN a previous communication (NATURE, February 14, p. 367) we called attention to the series of oceanic deposits in Barbados, of which the well-known Radiolarian earth (or Polycistina marl) forms a part. We stated that these deposits had a wide extension, and were of variable composition, some being much more calcareous than others; and further that they formed an independent series, resting unconformably on the older clays and sandstones which are supposed to be of early Tertiary age.

Since the date of our former note, we have examined many sections along the outcrop of the deposits, and find that the varieties which we had noticed fall into a natural succession, the calcareous earths lying principally at the base, though in the northern part of the island there is a development of similar beds at the summit. The total maximum thickness is about 200 feet, and the series contains many interesting varieties of rock. We hope to describe these at length, and to lay our results before the Geological Society, but as some time must elapse before this can be done, we write at once to place on record the fact that some of the beds have all the essential characteristics of typical (Cretaceous) chalk.

We have samples which consist of from 80 to 90 per cent. of calcium carbonate, which give the usual white streak of chalk, which contain Foraminifera in abundance, and have a minute structure which can hardly be distinguished from that of certain portions of the English chalk. A thin slice examined under a 1-inch objective shows many Foraminifera distributed through a matrix which under this power appears to be amorphous; the Foraminifera are chiefly Globigerinæ of the thick-shelled type similar to that figured in Carpenter's "The Microscope and its Revelations," sixth edition. Examined under a higher power the matrix can be resolved into definite particles, among which can be distinguished many forms identical with the so-called crystalloids of the chalk. Our friend Mr. W. Hill, to whom we sent a specimen of this Barbadian chalk, says it presents a very close analogy to our English chalk. Other samples combine the characters of chalk and Radiolarian earth having a calcareo-siliceous matrix containing a mixture of Radiolaria and Foraminifera.

We believe that the Barbadian deposits were formed on the floor of the Atlantic previous to the upheaval of the Caribbean Islands, and this conclusion is strengthened by the fact that similar Radiolarian earths occur in Trinidad and Hayti. We find too that the late Dr. Carpenter remarked that if the modern oceanic oozes were uplifted they would form deposits similar to the Barbados earths ("The Microscope and its Revelations," sixth edition, p. 602).

We wish to correct one paragraph in our former letter, in which we tacitly assumed that the Caribbean Islands were originally part of the American continent, and were therefore continental islands; we are now disposed to regard Barbados at any rate as an oceanic island, and believe that it has never been connected with South America since its upheaval as an island. Colonel Feilden has collected some evidence on this point which we hope he will shortly publish. We may state that the nearest island to Barbados (St. Vincent) is one hundred miles to the west, and that the intervening sea is more than 1000 fathoms deep.

But whether classed as an oceanic or a continental island, the rocks of Barbados are equally interesting to the physical geologist, since they give proof of a complete interchange of continental and oceanic conditions in Tertiary times; for the underlying sandstones and shales imply the close proximity of a continent during their formation, while the chalky series proves the subsequent conversion of this shallow sea into an oceanic area. Moreover, the existence of both sets of rocks now at the surface is entirely antagonistic to the prevalent theories respecting both continental and oceanic islands.

April 22.

A. J. JUKES BROWNE.
J. B. HARRISON.

A New Mountain of the Bell.

I HAVE just returned from a journey of four weeks in the desert of Mount Sinai, made with the especial object of studying the *Jebel Nagous* in connection with the joint researches of Dr. Alexis A. Julien and myself on "musical sand." The "Mountain of the Bell" is situated on the Gulf of Suez, about four and a half hours from Tor by the roundabout camel route. It was first described by Seetzen in 1808, since which time it has been visited by Ehrenberg, Gray, Wellstedt, Rüppell, Ward, Newbold, and the late Prof. Palmer, as well as by large numbers of pilgrims. My observations confirm in the main their accounts of the acoustic phenomena heard, but my measurements differ widely from those of all the travellers save Prof. Palmer.

The name Jebel Nagous is given by the Bedouins to a mountain nearly three miles long and about 1200 feet high, composed of white sandstone bearing quartz, pebbles, and veins. On the western and northern sides are several large banks of blown sand, inclined at high angles. The sand on one of these slopes, at the north-west end of the mountain, has the property of yielding a deep resonance when it slides down the incline either from the force of the wind, or by the action of man. This bank of sand I distinguish from the others by calling it the *Bell Slope*. It is triangular in shape, and measures 260 feet across the base, 5 to 8 feet across the top, and is 391 feet high. It has the high inclination of 31° quite uniformly. It is bounded by vertical cliffs of sandstone, and is broken towards the base by projecting rocks of the same material. The sand is yellowish in colour, very fine, and possesses at this inclination a curious mobility which causes it to flow when disturbed, like treacle or soft pitch, the depression formed being filled in from above and advancing upward at the same time. The sand has none of the characteristics of sonorous sand found on beaches. When pulled downwards by the hands or pushed by the feet a strong vibration is felt, and a low note is plainly heard resembling the deep bass of an organ-pipe. The loudness and continuity of the note are related to the mass of sand moved, but I think that those who compare it to distant thunder exaggerate. The bordering rocky walls give a marked echo, which may have the effect of magnifying and prolonging the sounds, but which, as I afterwards demonstrated, is not essential. There are no cavities for the sand to fall into, as erroneously reported. The peak of Jebel Nagous rises above the Bell Slope to the height of 955 feet above the sea-level, as determined by a sensitive aneroid.

After studying the locality and phenomenon for several days, I formed the opinion that it could not be unique as hitherto supposed, and accordingly I tested every steep slope of blown sand met with on the caravan route northward to Suez. On April 6 I examined a steep sandbank on a hillock only 45 feet high, and was rewarded by the discovery of a second Nagous. This new Nagous is in the Wadi Werdan, only five minutes off the regular caravan route, and one and a half days, by camels, from Suez. The hillock is called by the Bedouins *Ramadan*, and forms the eastern end of a range of low hills about one quarter of a mile long; being the only hills in the Wadi, the locality can easily be found by travellers. The hills consist of conglomerate and sandstone, and to the west of gypsum; they slope up gradually from the north and end in bold cliffs on the south side. Sand blown by the north wind is carried over the cliffs, and rests on the steep face at two inclinations, 31° above, and 21°, or less, below. By applying the usual tests with the hands to the fine-grained sand, I found that wherever it lies at the requisite angle to produce mobility (31°), it yielded the bass note, though not so loud as on the Bell Slope of Jebel Nagous. In one instance, my friend and fellow traveller, Mr. Henry A. Sim, of the Madras Civil Service, who kindly aided me in my investigations, heard the sound while standing 100 feet distant. The Nagous sand occurs at intervals throughout the 500 yards of low cliffs; the main bank at the east end being 150 feet wide and 60 feet high measured on the incline. I stirred up the mobile sand pretty thoroughly on this slope, and the next day it failed to give the sounds, not having recovered its properties. The intervening night was very cold (53°). I feel confident that this phenomenon is not very rare in the desert, though the spontaneous production of sounds by sliding of the sand without man's agency, as at Jebel Nagous, may be. Whether the *Rig-i-Rawan*, north of Cabul, is caused by similar conditions remains to be determined, but I fear that the peculiar relations existing between England and Russia will prevent my visiting Northern Afghanistan. The Bedouins who accompanied us were greatly astounded at my discovery of a new Nagous, and I fear that their faith in a monastery hidden in the heart of Jabel Nagous has received a severe shock. It is interesting to note that the Nagous or modern gong is in daily use in the Monastery of St. Catherine, Mount Sinai.

I made photographs of Jebel Nagous and vicinity, as well as of the new Nagous, and collected specimens of the rocks, sand, &c. This communication must be regarded as a preliminary notice, full details being reserved for the work on "Musical Sand" in preparation by Dr. Julien and myself.

I shall be obliged if those who have opportunities of examining banks of dry and fine sand inclined at 31° will report through your columns whether they yield deep sounds when disturbed.

Cairo, April 10. H. CARRINGTON BOLTON.

AIR-TIGHT SUBDIVISIONS IN SHIPS.

THE last two months have been unfortunate ones for shipping generally, and more particularly for the navies of at least four of the great powers. France has lost two torpedo boats under such circumstances as to involve the condemnation of a whole class of vessels. Germany and the United States of America have each lost a small fleet in a hurricane of unusual violence. Besides the material loss of ships these three nations have to bemoan the loss of a considerable number of men. Only little more than a month ago one of the largest ships of the British Navy stranded in waters rightly assumed to be perfectly safe, and has become a total wreck. Fortunately in this case there was no loss of life. Another of her H.M. ships only just escaped the disaster which overwhelmed the German and American fleets at Samoa, and the circumstances attending her escape are worthy of a moment's attention.

The storm approached not without warning, and it is evident that the captains of all the ships set about making preparations for meeting it as best they might. They appear all to have got up steam, so as to ease their cables by steaming to their anchors, in case it should be impossible to get out. The only ship that did get out was H.M.S. *Calliope*, and without in any way detracting from the merits of her captain and those under his orders, it is evident, from the brief accounts to hand, that all would probably have been unavailing had she not been provided with very powerful machinery. In the Navy List her tonnage is given as 2770, and her horse power as 4020, or one and a half indicated horse power per ton of displacement. The most powerful of the other ships was the German corvette *Olga*, which apparently had considerably less than one horse power per ton of displacement.

The other ships, especially the American ones, were so deficient in power that they were unable to make any front to the storm at all. Even with her great power the *Calliope* was only able to attain an effective speed of half a knot per hour in the teeth of the storm. All praise is due to the men who were able to make such good use of this very meagre margin as to have saved a costly ship and many valuable lives for the further service of their country.

The Samoan disaster has thus, in a dramatic and even tragic way, shown the uses of steam power in saving a vessel by propelling her against a storm. Reflections on the loss of the *Sultan* lead us to ask if steam power cannot be made more useful in succouring and saving a ship after she has struck a rock, or in any other way received such damage to her hull as to render her loss by foundering imminent.

According to convention an engine is working at the rate of one horse power when it is lifting a weight of one ton against gravity at a velocity of 14·74 feet per minute. If, then, a ship is fitted with engines indicating one horse power per ton of displacement, these engines would, if their whole power could be usefully applied and directed against gravity, be able to keep the ship afloat so long as she did not sink at a greater rate than 14·74 feet per minute. The *Vanguard* took seventy-two minutes to sink. The practical question comes to be, How can the ship's power, of engines or men, be best applied so that the greatest proportion of it may be made available for keeping her from sinking?

Hitherto it has been usual to fit all ships with suction pumps, capable of being worked, some by steam and some by hand power. To use such pumps with effect it is necessary that they should be worked at such a rate as to throw overboard more water than can enter the ship in a given interval of time. The lower they bring the water in the hold of the damaged ship, the greater is the facility offered for the water to enter, and the harder becomes the work of lifting it. If the damage to the ship's hull is in any way serious, dealing in this way with its effect is almost always hopeless, unless it is possible to get at the leak and reduce its dimensions or close it altogether. The bottom of a ship at sea is very inaccessible. If she remains fast on the rock it is usually impossible to get at the leak either from the outside or from the inside. If she is afloat, and will keep afloat long enough, the leak can often be efficiently dealt with by passing a tarpaulin or sail under her bottom. But this is by no means a simple or easy operation, even when performed as a matter of drill with plenty of time, and in the absence of excitement or danger.

When a ship is sinking, she does so because water has got into her either from above or below, and has displaced the air with which she was charged. In order to stop her sinking and to raise her to her original level, it is necessary to reverse the operation and replace the water again by air. If the water has come in from above, by shipping seas, this can be effected by suction pumps, which throw it overboard again. If it has entered and is entering through a hole in the bottom of the vessel, it is necessary not only to remove the water which has entered, but to stop any further entry, and this is achieved by any means which enables us to thrust the water out again by the same way as that by which it entered.

If we consider a ship's hold, and assume that the deck covering it above, and the bulkheads shutting it off fore and aft, are all sufficiently strong and air-tight, then, if the whole bottom were allowed to drop out, her stability being otherwise assured, she would be very little the worse; the water would rise in her hold only until it had so far compressed the air that its tension exactly balanced the pressure of the column of water outside, and matters might safely remain in this condition of equilibrium almost indefinitely. Thus, by making the main deck of a modern ship, to which the water-tight bulkheads are carried up, air-tight, she would be practically proof against all risk of sinking from damage to her bottom.

I do not think that there would be any difficulty in making the compartments of a ship perfectly air-tight, or more properly, in fitting them so that the rise of tension quickly produced by the entry of water through a serious leak, would at once close any joints or small openings, in the same way as the door of the air lock giving entrance to a submarine caisson is kept closed and air-tight by the pressure of the air within. But inasmuch as the smallest leak of air, whether through the deck or through the bulkheads, would represent an equivalent of water entered and of buoyancy lost, it is necessary to be able to make good the loss by mechanical means. The more carefully the decks and bulkheads have been fitted in the first instance, the less will be the amount of air which will be required to be supplied by engine or man power in order to keep the water out in the event of serious damage to the ship's bottom.

Dealing with leaks in this way is equivalent to transferring the leak from the ship's bottom to her deck, and dealing with it there in the shape of an escape of air in place of an entrance of water.

In order to make successful use of this method it is necessary that the ship's deck and bulkheads should be not only air-tight, but also sufficiently strong to resist a pressure which, in the case of even the largest ships, would not exceed one atmosphere, or 15 pounds per square inch. Each compartment would have to be about as strong as an old low-pressure marine boiler.

Modern men-of-war are built in such a way that they require nothing but the air-tight hatches, and air-forcing

pumps to make them quite secure against the most extensive damage to their bottoms. Indeed, as regards the stoke-holds, they are already fitted with the air-tight hatches in order to be able to used forced draught for the furnaces. Modern merchant ships are built with an iron deck, so that there is no difficulty about providing the strength. Their hatchways are, however, always very large; but, on the other hand, there is little traffic through them, so that they could be treated in a more substantial way than the smaller hatchways of a man-of-war with her large complement of men. The bulkheads which subdivide the hold into compartments always profess to be water-tight, and to be able to resist the pressure exercised by the water filling the compartment. There should therefore be no difficulty about them. Indeed, if ships were built to withstand air pressure, a very simple method would be provided for testing the efficiency of the bulkheads without the disagreeable process of filling the compartment with water. It would be only necessary to close the legitimate openings and get the air in it up to a pressure equal to that of the ship's draught of water, and the result would be unequivocal. It is proper to observe that the construction of an air-tight bulkhead would differ slightly from that of a water-tight bulkhead, inasmuch as it will be exposed to the maximum pressure over its whole surface, whereas the water-tight bulkhead is exposed to a graduated pressure, being greatest at the keelson, and least under the deck.

A further advantage of fitting a ship with air-tight subdivisions is, that it not only gives her greater security against foundering, but it affords a means of largely insuring her against risks of fire. This has more especial reference to merchant ships. If the contents of a ship's hold catch fire, the easiest way of putting it out is to stop the supply of air, and this can be done if the hold is air-tight.

So far the damage to the ship is supposed to be a rent in the bottom. If it is not in the bottom, but somewhere above it, then the air can only expel the water down to the level of the breach, when the air will begin to escape through its uppermost part. It will now depend on the supply of forced air available, how large a hole can be kept continuously filled by a stream of air rushing out. The area so occupied is necessarily closed to the entrance of water, and if the machinery can supply air at a sufficient rate, the whole rent can be filled by a current of air, which, so long as it is kept up, is as efficient a leak stopper as a plate of iron would be, and meantime the bottom of the hold can be cleared by the ordinary bilge pumps.

Rents in a ship's side, such as are produced when she is run down, or rammed by another, are usually so extensive and serious that, unless the ship is protected by an inner skin, immediate destruction ensues before there is time to take any measures for rescuing her. But with an inner skin the damage may be so far reduced as to make it possible to deal with it as above indicated. The higher up on the ship's side is the damage the less suitable is the pneumatic method for dealing with it, if it is of a really extensive character; but, on the other hand, the more easy is it (given the time) to get at it, and deal with it from the outside. In all cases where the ship has been damaged by touching the ground, or by torpedo explosion under the bottom, and not involving the destruction of the ship, the pneumatic method affords the readiest means of combating the results.

It must be remembered that a ship's hold when filled with compressed air will be *habitable;* that is, if an air lock is provided, men can descend into it and repair the damage, just as they can descend into a caisson and dig out the foundations for the pier of a bridge.

The pneumatic method is however not only adapted for keeping damaged vessels afloat, it is also useful for raising sunken or stranded ships. For this purpose the salvage steamer must be provided with air-forcing pumps as well as the suction pumps which she usually carries. Having closed, and if necessary strengthened the deck, by means of divers if below water, she then pumps air into the holds of the ship, and at once restores a large proportion of her original buoyancy to her. If she does not rise, the other methods of salvage can be applied in addition, and with much increased chance of success.

The principle of this method is not new. A very old device in endeavouring to float, or to keep afloat, ships, is to fill as much of their damaged hold as possible with empty casks. A later modification of this method is to use inflatable india-rubber bags. It may be remembered that after the *Vanguard* sank Admiral Popoff of the Russian Navy sent a large apparatus of this kind in order to render assistance in trying to float her. Both these appliances are cumbersome. A ship's hold is seldom quite empty when she sinks, and even if it were, it is not easy to fill it under water with casks full of air, or even with inflatable air bags; and in any case it is difficult in this way to fill more than a fraction of the hold with air. The simple and efficient way of dealing with the matter is to treat the ship's hold itself as the vessel to be filled with air.

Compressed air is every day occupying a wider field as a means of transmitting power. It is already used as a substitute for gunpowder in the guns for firing shells with high explosives. It seems to me that if it can be used for largely increasing the safety of life and property at sea it is right that the fact should be brought as prominently forward as possible, in the hope that it may receive practical application in the hands of the shipbuilder and the engineer. J. Y. BUCHANAN.

NOTES ON STANLEY'S JOURNEY.

I HAVE watched every footstep of Stanley for the past twenty years, had constant intercourse with him during his short visits to this country, and have unbounded confidence in him as a pioneer, for I cannot but admire the noble efforts he has made to open up Africa to civilization. Wherever he has travelled he has left his mark behind him; others may follow his example without fear of being molested, and he has given us such vivid descriptions of the regions mapped by him that, for all practical purposes, no traveller need supervise his work. Some say he has been too high-handed with the natives, but I may be allowed to think that his power of influencing those over whom he holds command has proved him to be the most trusted and successful traveller of the age. If his explorations be quickly and judiciously followed up, the native inhabitants will feel security against all oppression, and the traders in slaves will be expelled from the country.

Brilliant is scarcely the name to give the exploit of Mr. Stanley, as given in his recently published letters. What instance in travel can excel such devotion? Is there a schoolboy who does not admire a man with his indomitable pluck and dogged perseverance? His latest journey to relieve Emin Pasha has outstripped, if possible, all his previous explorations in the "Dark Continent." Those 160 days of toil, from June 28 to December 12, 1888, through starvation, desertion, mutiny, savage dwarfs and cannibals, thorny thickets, darkness, and swamps, were enough to try the patience of any human being; but, thank God, his British pluck never failed him; on and on he pressed, while his native followers were in utter despair, and broke out into mutiny. He used every persuasion with them: all failed. What was he to do? He felt that his duty was to relieve Emin Pasha—his countrymen expected this—and, with his accustomed sense of what was just and right, the two ringleaders of the mutinous band

were hung in the presence of his camp followers. This wholesome example proved to be the saving of his expedition. He emerged from the poisoned atmosphere of the forest, and says that he was amply rewarded when his remaining native followers kissed his hands in grateful acknowledgment of being delivered from death.

The party proceeded on, moving with great glee across the grassy slope amidst villages and cultivation, soon standing upon the brink of the crags which overhang the western shores of the Albert Nyanza of Baker. Here fresh difficulties arose; the suspicious natives would give them no canoes, would hold no intercourse with him. Emin Pasha's steamer was not in sight, and, after consulting his officers, Stanley retired to an entrenched position, sent Stairs, R.E., for his English-built boat, and, terrible though this journey has been in every form, the heroic Stanley won his point, and shook hands with Emin Pasha on April 29, 1888, 465 days after leaving Charing Cross to his relief.

It is quite possible that he may return to England by the end of May, but there are several reasons which may delay him. The difficulty of providing for so large a party as ten thousand followers belonging to Emin Pasha—this is a most anxious charge. Again, Stanley's thirst to solve the problem of the unexplored country south of the Albert Lake may lead him there, and I really feel more anxious about him since the arrival of his letters than I felt before we heard of his safety, for he is so fearless, he never sees a difficulty.

The marvellous growth of vegetation upon Stanley's route is not to be wondered at, as we know that in similar latitudes, such as Uganda, Borneo, and the Amazon, the same density of undergrowth and forest exists. A band of moisture encompasses the world at the equator, extending three to four degrees of latitude on either side; the vertical rays of the sun beat down with great intensity, and vegetation is almost seen to grow. In Uganda I have seen the banana trees, after being felled, shoot up from their centres immediately after their stems had been cut across; the roots of the trees are surrounded by spongy soil laden with moisture from the daily fall of misty rain, and the powerful sun completes the formation of the great forests of banana trees, without the aid of cultivation, beyond the help of the decayed leaves. We see the same process in the great belt of forest called in India the "Terai," which extends along the bases of the southern spurs of the Himalayas. Here the rains which fall upon these spurs, ooze out over the lands of the "Terai" and feed the roots of the magnificent forest trees, forming food and shelter for the wild elephant, boar, and swamp-loving creatures; but the atmosphere is almost certain death to all human beings except the inhabitants. We cannot, therefore, feel any surprise that Stanley and all his party suffered from sickness, and wonder how any of them escaped alive.

"Ugarrowa or Ulede Balyuz, a tent-boy of Speke's," an "Arab slave-dealer," is constantly mentioned in Stanley's interesting narrative. I am able to give some information about this person if he be the same "Ulede," one of "Speke's faithfuls," represented in the *Illustrated London News* of July 4, 1863, as "Ulede Senior," in a photograph taken by Royer in Cairo. He told me that he was a native of Uhiao, was captured by the Watuta in infancy, and sold as a slave to a Zanzibar trader. He was engaged by Speke as a load carrier, and became my valet, which he continued to be till our arrival in Cairo. He was thoroughly trustworthy, as many of his race are, and more intelligent than most of our men. He could name accurately every march in our journey, most of the trees and plants, and could tell a capital story. His career has been deservedly successful, and though from circumstances he has become a well-known dealer in slaves, I might ask what career is open to any young man of African origin who has never received the slightest education. Ulede Balyuz (*i.e.* the Consul's boy) has done good service in sheltering Stanley's sick, and in transmitting the graphic despatches which we have all read with profound interest, therefore he ought not to be condemned too hastily, but rather be utilized by the Congo Free State Government as the head of a district.

The dwarfs mentioned by Stanley must be very numerous, as he came upon one hundred and fifty villages of them. One specimen alone was seen by Speke and myself in Unyoro, and at least one perfect skeleton has been received from Emin Pasha by Prof. Flower. They seem very proficient in hunting, and used every conceivable device to poison the men of Stanley's party by placing staked pitfalls on the path, in the manner they would trap an elephant or antelope, and it appears they were only too successful.

We must wait for Stanley's return to hear more of the race of Manyema. I believe this race to be the Nyam-Nyam described thirty years ago by Mr. Petherick, but without knowing their tribal marks and arms, this cannot be decided. Meantime, these daring cruel savages have shot down poor Major Barttelot, and are engaged by the slave-dealers of Zanzibar to plunder, capture, and kill the inhabitants, and reduce the country to a wilderness; so that, through Stanley's brave deeds, we have our work of civilization before us.

J. A. GRANT.

FURTHER NOTES ON THE GEOLOGY OF THE EASTERN COAST OF CHINA AND THE ADJACENT ISLANDS.

TWO years ago some notes were published in NATURE (vol. xxxvi. p. 163) on the geology of a portion of the coast of China, compiled from a report forwarded by Surgeon P. W. Bassett-Smith, R.N., of H.M.S. *Rambler*, to the Hydrographical Department of the Admiralty. Since then Mr. Bassett-Smith has extended the area of his investigations both to the north and south of the coast-line dealt with in this report, so as to embrace the whole eastern coast from Shanghai and Hong Kong; and has embodied his observations in two further reports to the Hydrographical Department. These documents, with the specimens referred to in them, having been submitted by the Hydrographer to the Director-General of the Geological Survey, Dr. Hatch, of the Petrographical Department of the Survey, has drawn up the following abstract of the reports and notes regarding the specimens:—

Speaking generally, the whole coast between Shanghai and Hong Kong consists of granite; the high mountain-ranges, especially in the south, present chiefly this rock. Flanking the granite on various parts of the coast are vast masses of crystalline schists (gneiss, mica-schist, &c.), parts of which are rich in metallic ores, even auriferous quartz occurring, as at Chinsan, and more plentifully in the Shangtung province, where it is profitably worked by the Chinese. A curious conglomerate, found at Sharp Point Islands, River Min, at Davis Island, Yangtse-kiang, and also in the Shangtung province, is overlain by slates, probably of Cambrian age, but for the most part unfossiliferous, although some fish-remains and Algæ have been found in the Shangtung province.

In the northern part of the coast (Chusan to Shanghai) there are many traces of ancient volcanic activity. The older volcanic rocks consist of porphyritic felsites (Chinsan Island, Davis Island, Elliot Island, Bonham Island, and Side Saddle Island) and basalts (Changtau), both of which are intrusive in the granite and crystalline schists. More recent volcanic tuffs and breccias were obtained in a quarry near Ningpo.

The Chusan Archipelago.—Of this group of islands, situated at the mouth of Hang-chow Bay, south of Shanghai, the northern members have a marked vol-

canic character, the rocks composing them being volcanic conglomerates, breccias and tuffs, together with felsitic, trachytic and basaltic lavas, the more acid types of which show well-marked flow-structures. The vents from which these lavas were erupted are situated chiefly in the large island of Chusan; another focus of emission is probably represented by Changtau Island.

One of the most noticeable features of the group is presented by the vast stretches of land that have been rescued from the sea. Many islands formerly isolated have been united; and broad plains of rich alluvial ground have been reclaimed, are now highly cultivated, and support a dense population. This has been chiefly brought about by the construction of strong embankments and sea-walls from point to point across the bays, after the latter had been allowed to become partly silted up by the mud brought down from the Yangtsze River and Hang-chow Bay. This difficult work testifies to the marvellous energy and industry of the Chinese.

Details are given of the geology of the following islands of the Archipelago:—

Video Island, the outermost of a long chain of islands, extending in a west-south-west direction, has a conical shape, with steep cliffs, and consists of a pinkish quartz-trachyte, penetrated by numerous dykes of basalt.

Tripod Island, an elongated island, about 600 feet high, sloping moderately to the west, but descending on the east almo t perpendicularly into the sea, is composed of a volcanic breccia, frequently penetrated by dykes of basalt.

Keusan Island, a high island of irregular elongated shape, separated from Changtan Island by a narrow channel of 5-7 fathoms, with a good anchorage, presents, at its north-eastern end (Radstock Point), a coarse volcanic breccia ("trachyte-conglomerate"), with which are associated well-banded acid lavas (trachyte). In other parts of the island a greenish tuff occurs, which is abundantly penetrated by an interlacing system of basaltic dykes.

Changtau Island, a rugged island with a double-peaked summit, shows, along its west coast, cliffs consisting of a stratified green tuff and trachyte-breccia, with dykes of basalt and flows of a well-banded trachytic lava.

Taeshan Island, a series of high hills attaining to a height of 700 feet, connected by broad alluvial plains, consists on its north-east coast of a grey quartz-porphyry, weathering blood-red, and salmon-coloured felsites, penetrated by numerous dykes of basalt.

Show Island is formed entirely of a coarse trachyte-breccia, containing large angular fragments. This rock is much quarried, the stone being conveyed away in junks.

Volcano Island, the most westerly of the chain, is composed of the same volcanic breccia, associated here with felsitic lavas.

North-East Islet, off Chusan Island, and Nine-Pin Rocks are composed of a compact dark-coloured felsite, with a marked bedded character. In places the rock shows distinct flow-structure.

Poo too Island consists of a high peak, separated from a number of smaller ones by deep gullies, filled with blown sand. The summit of the hill is formed of a compact white trachyte, which has been erupted through the granite forming the base of the hill.

Chusan Island, the largest of the group, being twenty-two miles long and ten miles wide, consists of a long range of mountains, many peaks of which are over 1000 feet high. Between the numerous spurs given off from these mountains lie tracts of highly fertile land, the lower parts of which have been recently reclaimed, and are protected by a series of embankments. Outside the outermost of these the mud-flats are used for the collection of salt, to obtain which the mud is scraped up, filtered, and the brine evaporated in wooden trays. The old cliff-line now stands far back from the present coast; and former islands appear now as isolated hills. This island is less bleak than the smaller ones of the group, owing to the protective influence of the small fir-trees that are encouraged to grow on the hill-sides. Other trees here met with are the camphor, tallow, maple, and numerous evergreens in the neighbourhood of the villages. The rocks are quartz-porphyries and felsites.

Lateo Island consists of a coarse volcanic breccia, containing large angular fragments of quartz-felsite. This stone is extensively quarried.

Ketsu Island.—A small rugged double island off Chusan, consisting of dark-banded felsite with small porphyritic crystals of red felspar.

Blackwall Island.—A large well-cultivated island, with hills of dark-coloured felspar-porphyry and felsite. Volcanic breccia also occurs, penetrated here and there by basalt dykes.

Kintang Island.—A large island near the mainland, presenting a fine, pointed summit of red felspar-porphyry. Along its cliffs are highly contorted volcanic breccias and felsites.

Taoutse Island.—A small narrow island of red felspar-porphyry (red felsitic ground-mass embedding small bright red crystals of felspar).

Changpih Island.—A large island with much reclaimed land; red felspar-porphyry.

Chinhai Island.—A small rock in the mouth of the Ningpo River, composed of the same red porphyry.

Rambler Island, Hang-chow Bay.—A rounded mass with steep smooth sides, composed of volcanic breccia and brown felspar-porphyry.

Mr. Bassett-Smith adds that no traces now remain on the China coast of the volcanic activity that gave rise to the enormous accumulations of lava and tuff referred to in the above notes, with the exception of a few scattered hot springs. He is of opinion that after the eruptions ceased, a subsidence must have taken place, but that the ground is now probably rising.

WHICH ARE THE HIGHEST BUTTERFLIES?

THE following extracts from a letter received a few weeks back from Mr. W. H. Edwards, of Coalburgh, touch on this question, and may be of interest to lepidopterists. Having now for many years ceased to give attention to this subject, I cannot express any opinion, but I think Mr. Edwards's facts are very curious, and the conclusion expressed in his last paragraph not far from the truth.

ALFRED R. WALLACE.

"In a recent part of my vol. iii. I have figured one of the high Alpine Colorado Erebias, *E. Magdalena*, found on the extreme summits, among nothing but rocks. I have also succeeded in breeding another of the Alpine Erebias, *E. epipsodea*, from egg to imago, and have a full set of drawings for plate. Have also had *Chionobas chryxus* (also Colorado) and imago, and have all the drawings there. Connected with these Alpine species is a matter I talked over with you, and of which I now write. There must be many genera of Satyridæ in which the larvæ are thick-bodied, inert creatures, very much like many of the Noctuidæ. I have twice raised *Arge Galatea* from egg to imago. This larva is remarkably like a Noctuid in shape, inertness, in the manner it lies on the ground—curled up so that head touches tail, in a ring, or like a *d*. The pupa is so like a Eudamus, that when I sent one to Mr. Scudder to ask what it was, he replied, 'Some Hesperid probably, very near to *E. tityrus*.' It is made loose on the ground or

in the sod, there being no outer case, and no attachment. The usual hooks of the cremaster are not bent, but straight out and few. Now the *Erebia epipsodea*, and the three Chionobas which have been bred in this country, *C. chryxus*, *semidea*, and *jutta*, are like the *A. Galàtea* in larval habits and appearance, and the pupa is unattached, and has actually no hooks at all. I read in Buckler, that *Satyrus Semele* actually makes a case underground (like some of the Sphingidæ), and is inside that like a Hesperid. It is to be supposed that many genera of the Satyridæ pupate unattached, or in cocoons. Mr. Scudder says the eggs of Satyridæ are very like the Hesperidæ, and has to admit the resemblances I have spoken of in the other two stages. But he passes over all this as a mere trifle, and insists that 'in the prime features,' as he calls it, of the imago, the Satyridæ 'out-rank all others.' Now what are the 'prime features' he tells about? They are two: one is that the pupa hangs by the tail, and that there is a regular progression from the Hesperid style of attachment through the Papilionidæ, the Lycænidæ and the Satyridæ; and that the flat ventral side of pupæ in what he calls the higher families, the Suspensi, is an evidence that once they or their ancestors were attached by a girdle, like the Papilionidæ. The other is the atrophied condition of the fore-legs, which is more extreme in the Satyridæ than in any other family, and reaches the last degree in Chionobas. He, in his 'Butterflies of New England,' now issuing, puts *Chionobas semidea* at the head of the North American butterflies, the top rung of the ladder, beyond which we can go no farther! This is what I call your attention to.

"When we used to study 'Euclid,' we sometimes proceeded by an apparently correct mode of demonstration, till we came to 'which is absurd,' and I hold that this conclusion of Mr. Scudder is absurd on its face. Here is a butterfly on the top of the White Mountains of New England. Its species is found nowhere else than in Labrador and in Colorado, in the latter on the loftiest summits. There is no difference between the three butterflies from the three regions, and yet they cannot have had any communication for untold ages. It is considered as a relic of pre-glacial times in the White Mountains. This butterfly lives in a semi-torpid condition through its short season, lies about on the rocks, has but a trifling power of flight, and dodges the high winds in crevices of rocks. To say that an insect which for perhaps 50,000 years has lived this sort of life, and has not changed in all that time, is the most advanced in the scale of North American butterflies, and so of all the world, is absurd and ridiculous! The wonder is that it has not lost the use of its wings. Therefore the argument is wrong somewhere that leads to such a conclusion. If the premisses are allowed to be correct, then the reasoning has a flaw.

"I do not believe there ever was any derivative progression from one family of butterflies to another. And we cannot say that the Papilionidæ are derived from the Hesperidæ (either because of six legs, or the epiphysis, or any other reason), or the Papilionidæ from the Lycænidæ, or the four-legged families from the six-legged. There is not in the rocks a particle of evidence of such a progression, and the whole thing is the merest fancy. Any differences between families are not owing to derivation, but to the development of each independently, like the rays of a fan. "W. H. EDWARDS."

NOTES.

WE regret to have to record the death of Mr. Warren De la Rue, F.R.S. He was born in 1815, and died on Good Friday, after a short illness, from pneumonia. Mr. De la Rue was a most devoted observer and munificent patron of astronomy, and in him and Balfour Stewart solar physics has lost its chief founders.

THE death is announced of Dr. Paul du Bois-Reymond, Professor of Mathematics at the Technical High School of Berlin, and formerly at the Universities of Freiburg and Tübingen. He was the author of two well-known mathematical works, and brother of the eminent physiologist of the same name. He was born on December 2, 1831, and died at Freiburg in Baden, on April 7.

THE Rev. J. H. Thomson, Vicar of Cradley, whose death is announced, had made what is described as an extensive and valuable collection of European plants, and it is understood that he has bequeathed them to the Worcestershire Naturalists' Museum.

THE National Union of Elementary Teachers has been holding its twentieth annual conference this week at Birmingham. The conference was opened in the Town Hall on Monday afternoon, when an address was delivered by Mr. R. Wild, the President-elect, on the report of the Education Commission, and on the latest edition of the Code. The defects of the existing system of national education were discussed at a crowded meeting on Tuesday evening. Mr. Chamberlain, in addressing this meeting, spoke of payment by results, in the sense in which the expression is now used, as a method which everyone condemns. "We want you," he said, "to show us a better way, and it is through such conferences as those which are now being held that Parliament and the Government may hope to find, tested by your practical experience, a substitute for a system which we desire to alter."

THE picture of Sir William Bowman, by Mr. Ouless, R.A., has, by special permission, been exhibited to subscribers in the Marsden Library of King's College, and has now been sent to the Royal Academy. The list of subscribers numbers 420, and will be closed on June 1.

SIR ROBERT BALL, the Royal Astronomer of Ireland, has just been elected an Honorary Fellow of the Royal Society of Edinburgh.

PROF. CORFIELD, M.D., has been elected a Corresponding Member of the Italian Association "Dei Benemeriti," and awarded a gold medal for his contributions to hygiene.

DR. SCHWEINFURTH arrived at Aden on March 23, on his return from a three months' stay in Central South Arabia. He has started for Europe, bringing a very interesting botanical collection with him.

THE *Japan Weekly Mail* says that Mr. W. Gowland, who has occupied a prominent place in the Imperial Mint at Osaka, has retired from the Japanese service. In 1872, Mr. Gowland was selected by Dr. Percy, of the Royal School of Mines, London, as Chemist and Metallurgist to the Japanese Government. His first task in that country was the organization of the metallurgical department of the Copper Mint and the establishment of chemical and metallurgical laboratories. He subsequently filled the posts of Technical Adviser and Assayer, and as such was directly responsible for the accuracy of the coinage. Amongst other reforms at the Imperial Mint he introduced a novel process by which crude copper could be converted into bronze coinage bars at one operation, and also elaborated processes for the coinage conversion. His investigations into the effect of bismuth on the ductility of silver are well known. He made many interesting discoveries amongst the tumuli and shell-heaps in the interior. The Emperor conferred several distinctions on him before his departure.

THE Berlin Academy of Sciences has lately been presenting various sums of money to promote scientific research. Dr. Franz-Stuhlmann, assistant at the Würzburg Zoological Institution, has received £50 (Mk. 1000) to enable him to proceed with his

investigation of the fauna of Zanzibar, where he has been since last spring. A sum of £60 (Mk. 1200) has been sent to Dr. Gustav Weigand to help him in his linguistic and ethnographical researches in the Balkan peninsula; and Dr. Pomtow has received £25 (Mk. 500) towards the publication of his work on Delphi.

A CORRESPONDENT in Paris writes to us that the British Section of the Paris Exhibition is nearly ready, and is much in advance of most other parts of the Exhibition.

THE following are the dates of some of the Exhibition Congresses which are to be held in Paris:—Technical Education, July 8 to 12; Bibliography of the Exact Sciences, July 16 to 26; Chemistry, July 29 to August 3; Ballooning, July 31 to August 3; Pigeons, July 31 to August 3; Hygiene, August 4 to 11; Higher Education, August 5 to 10; Physiological Psychology, August 5 to 10; Geography, August 6 to 11; Photography, August 10 to 17; Criminal Anthropology, August 10 to 17; Primary Education, August 11 to 19; Horticulture, August 16 to 21; Prehistoric Man and Remains, August 19 to 26; Electricity, August 24 to 31; Chronometry, September 2 to 9; Mines and Metallurgy, September 2 to 11; Applied Mechanics, September 16 to 21; Meteorology, September 19 to 25; River Utilization, September 22 to 27; Commerce and Industry, September 22 to 28; and Hydrology and Climatology, September 30 to October 15.

LECTURES will be delivered in Gresham College, on April 30, and May 1, 2, and 3, by Dr. E. Symes Thompson, on the medical aspects of life assurance.

AT a recent meeting, the Liverpool Geological Society appointed a Committee to report on the boulders occurring in the glacial deposits of Liverpool and district. This Committee, after full discussion, has reported that the investigations would be better carried out if extended over the whole of Lancashire and Cheshire, and by a larger Committee, consisting of representatives from all parts of the two counties. The objects of the enlarged Committee would be to examine and record the occurrence, nature, and facts bearing on the mode of transport, of erratics in the glacial deposits of Lancashire and Cheshire. If this suggestion meets with general approval, a meeting of all interested in the question will be held at some convenient centre at an early date.

IN a paper reprinted in NATURE (vol. xxxiv. pp. 220, 239), M. A. Blytt gave his views on variations of climate in the course of time. Believing that periodical variations of climate are to be attributed to changes in the strength of ocean-currents, he finally traced back their origin to the precession of the equinoxes and the eccentricity of the earth's orbit. He has now issued a paper, with two supplementary notes, in which he seeks to explain the displacement or alteration of beach-lines, by changes in the tidal-wave, caused by variations in the eccentricity of the earth's orbit. Referring to his former conclusions, that dry periods should be marked by chemical deposits, and rainy periods by mechanically-formed sediment, he passes on to consider the character of the Tertiary strata in the Paris and Hampshire Basins. In the author's opinion, the alternation of sediments of different nature indicates periodical variations in climate; and he endeavours to correlate these variations with particular phases in the eccentricity of the earth's orbit, the period or duration of which has been calculated in years. The author also believes that the lengthening of the sidereal day has had much influence on the form of the globe. This lengthening has been caused mainly by the tidal wave, and as the centrifugal force diminishes under such circumstances, strain accumulates in the solid earth, until the limit of resistance is reached. Hence, in the author's opinion, arose vertical displacements of beach-lines.

A REPORT from Sumatra states that the volcanic crater on the west coast of the island, which has been quiet for several centuries, was active during the middle of February.

SEVERE shocks of earthquake, lasting for eight seconds, were noticed at Zvornik in Bosnia, on April 2. The direction was from south to east.

WE have received from the Deutsche Seewarte, the second part of the *Deutsche Ueberseeische Meteorologische Beobachtungen* (see NATURE, vol. xxxvii. p. 444) containing complete observations made at six stations in Labrador during the year 1885, and at one station in Walfisch Bay (south-west coast of Africa) during the year 1887. Both sets of observations are carried on under considerable difficulties; in Labrador the rain-gauges are frequently inaccessible on account of drifting snow-storms, and the ordinary hygrometers are unmanageable during very low temperatures. In Walfisch Bay the thermometers are often choked by sand, or are liable to be affected by the intense radiation from the ground. Nevertheless the work supplies very valuable materials from remote regions where little is yet known about the peculiarities of the climate.

A CARBOHYDRATE of the empirical composition $C_6H_{10}O_5$, and possessing properties very closely resembling those of the arabin of "gum arabic," has been artificially prepared by Prof. Ballo, of Buda-Pesth. This achievement is the outcome of an attempt to reproduce the conditions under which the acids of the vegetable world are reduced by chlorophyll. It was assumed that the iron of chlorophyll is present in the ferrous state, and tartaric was the acid upon which operations were commenced. About equal quantities of tartaric acid and ferrous sulphate were dissolved in a minimum bulk of water, and the solution was warmed upon a water-bath. In a short time a greyish-yellow precipitate began to separate. The whole was then evaporated until it completely solidified on cooling. The cold mass was next extracted with alcohol and the extract again evaporated. The residue thus left by volatilization of the alcohol was neutralized with milk of lime, and the filtered solution again placed on the water-bath. It was now noticed that as the water was gradually expelled the contents of the evaporating dish became more and more viscid, until, finally, a sticky mass was left, reminding one most forcibly of gum arabic. Knowing that this familiar article of commerce chiefly consisted of the calcium and potassium compounds of arabin, the likeness was felt to be somewhat indicative of the formation of an arabin-like substance. On allowing the concentrated syrup to cool, a calcium salt readily crystallized out, yielding on analysis numbers pointing to the formula $(C_6H_9O_5)_2Ca + 9H_2O$. From this the free carbohydrate was obtained in two ways, either by precipitation of the solution in water with lead acetate and subsequent decomposition of the lead salt with sulphuretted hydrogen, or by addition of the calculated quantity of oxalic acid. The syrup of "iso-arabin," as it is provisionally termed, was further purified by repeated treatment with alcohol and ether and subsequent re-evaporation. It was then allowed to stand over sulphuric acid, some specimens for a month and others so long as a whole year. Each of these specimens, on combustion, yielded numbers indicating the empirical formula $C_6H_{10}O_5$. Iso-arabin is an almost colourless syrup, readily mixing with water. It does not reduce Fehling's solution, but rotates the plane of polarization to the right. It behaves, in short, exactly like the carbohydrates of the $(C_6H_{10}O_5)_n$ group. The potassium salt, obtained by decomposing the calcium salt with potassium carbonate, also crystallizes well in large anhydrous crystals. In addition to iso-arabin itself, a small quantity of its hydrate, $C_6H_{10}O_5 + H_2O$, is also formed by the action of ferrous sulphate upon tartaric acid, and separates out in crystals from the alcoholic washings of the crude iso-arabin.

Natural arabin itself forms a similar hydrate, the precipitate formed by addition of hydrochloric acid and alcohol to a solution of gum-arabic, when dried at 100° C., possessing this composition.

MR. FLETCHER, of Warrington, has recently put in the market a new and very cheap sensitive flame. It consists of a special arrangement of the small Argand bunsen, and differs from other sensitive flames inasmuch as any considerable sound completely extinguishes it. An interesting point in connection with it is that its sensitiveness is not constant, but apparently varies with the atmospheric conditions. The circular issued with the burner particularly states that the transparency of the flame renders it unsuitable for lecture experiments.

MR. W. A. HOLLIS writes to us from Brighton, describing a dream in which rooms and things in his house seemed to be in their relative positions, but transposed. The displacement extended itself to a servant, who appeared to be dusting some furniture; the cloth she was using she held in her left hand. This experience suggests to Mr. Hollis the question, "Is it possible that in dream-land we see things as in a looking-glass, like our little friend Alice?"

PISCICULTURISTS will read with interest M. Albert Le Play's pamphlet on the rearing of carps. It is entitled "La Carpe," and contains many new facts and suggestions concerning this branch of pisciculture.

AMONG foreign works which have been translated into French during the last two years, and the translations of which have been considered worthy of a reward, we notice two English books. The one is Mr. Green's "Short History of the English People"; the other Darwin's "Life and Correspondence," translated by M. H. de Varigny. The two other works to which, with the preceding, the Prix Langlois has been awarded, are Janssen's "Germany under the Reformation," and Nordenskiöld's "Travels." It is interesting that a book on Darwin should have been rewarded by the French Academy. M. Renan and M. Taine were on the jury, and did much to bring about the result.

WE have received the first three numbers (January to March 1889) of the *Bulletin international de l'Académie des Sciences de Cracovie*. The ordinary transactions of the Cracow Academy, being published in Polish, are not accessible to scientific students unfamiliar with the Slavonic languages. To remedy this inconvenience the Academy will henceforth issue a monthly *Bulletin* containing extracts in French and German of its regular proceedings, as well as summaries of all important memoirs in one or other of these languages at the option of the authors. Besides several historical, philological, and antiquarian papers, the present numbers contain contributions by Prof. Kreutz on the granites of Volhynia containing tourmaline and garnets; by M. Olszewsky, on an improved method for liquefying and solidifying the permanent gases and for studying their spectra; by M. Krzyzanowski, on the liquefaction and solidification of hydrogen in M. Pictet's experiments; by M. Olearski, on the elasticity of zinc and copper alloys; and by M. Sawicki, on the influence exercised by the physical and chemical agents on the electric properties of the nerves.

THE removal of tattoo-marks is a matter of no little difficulty, and many different methods have been tried (blistering, suction, thermo-cautery, counter-tattooing with white powder or milk, &c.). Criminals sometimes pour vitriol on their arms or hands, and letting it act for a few seconds, plunge the limb in water. The following method is recommended by M. Variot (in the *Revue Scientifique*):—The skin is first covered with a concentrated solution of tannin, and re-tattooed with this in the parts to be cleared. Then an ordinary nitrate of silver crayon is rubbed over these parts, which become black by formation of tannate of silver in the superficial layer of the dermis. Tannin powder is sprinkled on the surface several times a day for some days to dry it. A dark crust forms, which loses colour in three or four days, and, in a fortnight or so, comes away, leaving a reddish scar, free of tattoo-marks, and, in a few months, little noticeable. It is well to do the work in patches about the size of a five-franc piece at a time. The person can then go on with his usual occupation.

FROM a report of the Belgian Consul-General in the Congo State, it appears that the efforts made to introduce European vegetables and fruits in that district have been rewarded with very great success. The Government has imported tobacco-seed from Havana and Sumatra, which is cultivated in conjunction with native tobacco. The natives cultivate tobacco badly, but efforts are being made by the Government to teach them better methods. The inhabitants of the Lower Congo have been very successful in cultivating not only the usual African products, such as manioc, sweet potato, &c., but also sorghum, maize, and the "wandu" haricot, called "Boma" by the natives. The cotton-plant grows in its wild state, and the natives manufacture from it hats, wallets, &c. No effort has yet been made to cultivate it for trade purposes.

THE American Commercial Agent at Limoges, in a report on the result of the sanitary investigation as to the effect of plastered wines—that is, wines to which sulphate of lime has been added—says that the practice is very ancient, and one about the evil effects of which the highest hygienic authorities have differed. The Academy of Medicine has held special meetings and discussed the subject at great length. The advantages claimed for the practice are that fermentation is increased very much; that it is more rapid and complete; that the wine keeps longer when it has been plastered, and that the colour is richer and more lasting. It is now settled, however, that plastered wines have occasioned functional troubles, as, for instance, in the Department of Aveyron, where, the doctors report, those who consumed plastered wines suffered from an unquenchable thirst, an insupportable dryness of the throat, and various other troublesome symptoms. The action of sulphate of lime on the bitartrate of potash in ordinary wine produces an acid sulphate of potash; and in wine treated with lime, sulphuric acid in a free state is formed, and sulphate of magnesia; and these combined act as a purgative and sometimes as a caustic. M. Marty, who was appointed by the Academy of Medicine to report on the practice of plastering, examines all the arguments adduced in favour of the process, and, on his recommendation, the Academy condemns the custom as being detrimental to health.

THE British Consul-General in Algeria, in his report to the Foreign Office, says that hitherto the Government have been in the dark as to the habits and natural history of the locusts which now and then work such ravages in Algeria. Last year, however, a distinguished naturalist, M. J. Künckel d'Herculais, President of the Entomological Society of France, was sent to study the question on the spot, and he has published two reports on the subject. The species of locust which has ravaged the country since 1885 is not the same as that which invaded the same district in 1876–77. The former is *Stauronitis maroccanus*, the latter *Acridium peregrinum*. The former is found in most of the countries bordering on the Mediterranean, especially in Asia Minor and Cyprus. Morocco is, however, its original home, where it was observed in 1845, and again in 1867. Specimens of it collected in both those years still exist. Hitherto the accepted theory has been that they are brought from the desert by a strong southerly wind, but M. Künckel says that none have ever been observed in the Sahara, and he believes

them to have their origin in the mountainous regions of Hodna. Fortunately the exact moment of their appearance may be predicted, and steps can be taken to destroy them. With this object M. Künckel has made charts of the localities where they laid their eggs last autumn, and has arranged a methodical system of campaign. The destruction of eggs is an uncertain and expensive process, he thinks, whereas one man can destroy a million young insects in a day.

A CHINESE native paper published recently a collection of some zoological myths of that country, a few of which are worth noting. In Shan-si there is a bird, which can divest itself of its feathers and become a woman. At Twan-sin-chow dwells the Wan-mu Niao (mother of mosquitoes), a fish-eating bird, from whose mouth issue swarms of mosquitoes when it cries. Yung-chow has its stone-swallow, which flies during wind and rain, and in fine weather turns to stone again. Another bird when killed gives much oil to the hunter, and when the skin is thrown into the water it becomes a living bird again. With regard to animals, few are so useful as the "Jih-kih" ox, found in Kansuh, from which large pieces of flesh are cut for meat and grow again in a single day. The merman of the Southern Seas can weave a kind of silky fabric which keeps a house cool in summer if hung up in one of the rooms. The tears of this merman are pearls. A large hermit-crab is attended by a little shrimp which lives in the stomach of its master; if the shrimp is successful in its depredations the crab flourishes, but the latter dies if the shrimp does not return from his daily excursions. The "Ho-lo" is a fish having one head and ten bodies. The myths about snakes are the strangest of all. Thus the square snake of Kwangsi has the power of throwing an inky fluid when attacked, which kills its assailants at once. Another snake can divide itself up into twelve pieces, and each piece if touched by a man will instantly generate a head and fangs at each end. The calling snake asks a traveller "Where are you from, and whither are you bound?" If he answers, the snake follows him for miles, and entering the hotel where he is sleeping, raises a fearful stench. The hotel-proprietor, however, guards against this by putting a centipede in a box under the pillow, and when the snake gives forth the evil odour, the centipede is let out, and, flying at the snake, instantly kills him with a bite. The fat of this snake, which grows to a great size, makes oil for lamps and produces a flame which cannot be blown out. In Burmah and Cochin-China is a snake which has, in the female sex, a face like a pretty girl, with two feet growing under the neck, each with five fingers, exactly like the fingers of a human hand. The male is green in colour, and has a long beard; it will kill a tiger, but a fox is more than a match for it.

A SERIES of regulations with regard to patents and designs has just been issued in Japan. All inventors, whose discoveries are beneficial or are calculated to improve existing processes of manufacture, may apply for letters patent. No patents, however, will be granted in the case of articles of food or drink, or in case of medicines. Inventors who do not receive letters patent are powerless to sue in respect of piracy of their inventions. In order to register an invention, application must be made to the Patents Bureau, and if the officials are satisfied as to the genuineness of the invention, it is registered, on certain forms being complied with and certain fees paid. A curious omission occurs in the regulations, but it is not plain whether it is intentional or not. Nothing whatever is said as to the rights of a foreigner to patent an invention, but it is presumed that he will not be able to do so. Nor has any provision been made for advertising applications for letters patent. The Patents Bureau is to be the sole judge of all cases submitted to it, and from its decision there is no appeal; but, in certain cases, two judges sit with the Bureau and assist in deciding whether a patent should be granted or not. The duration of a patent is to be five, ten, or fifteen years, according to the amount paid in fees. The patent, of course, passes by assignment *inter vivos*, or to the patentee's heir, but nothing is provided for the cases of bankruptcy or marriage.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Miss Caroline Newton; a Leopard (*Felis pardus* ♂), a Lesser Koodoo (*Strepsiceros imberbis* ♂) Malindi, East Africa, two White-crested Touracous (*Corythaix albocristata*) from South Africa, presented by Mr. G. S. Mackenzie; a Common Squirrel (*Sciurus vulgaris*) British, presented by Mrs. Arthur Faulkner; an Indian Wolf (*Canis pallidus*) from Afghanistan, five Chaplain Crows (*Corvus capellanus*), an Indian Python (*Python molurus*) from Fao, Persian Gulf, presented by Mr. B. T. Ffinch, C.M.Z.S.; two Slender-billed Cockatoos (*Cacatua tenuirostris*) from Australia, presented respectively by Mr. Walter Bird and Mrs. Hunt; an Eagle (*Aquila* sp. inc.) from Foochow, China, presented by Messrs. J. de la Touche and George Siemosen; two Alligators (*Alligator mississippiensis*) from Florida, deposited; a Wanderoo Monkey *Macacus silenus* ♀) from the Malabar Coast of India, an Indian White Crane (*Grus leucogeranos*), six Rose-coloured Pastors (*Pastor roseus*) from India, three Elliot's Pheasants (*Phasianus ellioti* ♂ ♀ ♀), thre Amherst's Pheasants (*Thaumalea amherstiæ* ♂ ♀ ♀) from China, two Swinhoe's Pheasants (*Euplocamus swinhoii* ♂ ♀) from Formosa, two Vulturine Guinea Fowls (*Numida vulturina* ♂ ♀) from East Africa, two Crested Screamers (*Chauna chavaria*) from Buenos Ayres, two Pochards (*Fuligula ferina* ♂ ♂), European, purchased; two Viscachas (*Lagostomus trichodactylus*), a Vulpine Phalanger (*Phalangista vulpina* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE CONSTITUTION OF CELESTIAL SPACE.—M. G. A. Hirn has recently published an able and interesting work, entitled "Constitution de l'Espace Céleste," in which he inquires into the nature of the medium or agent which establishes and carries on the relationships of the celestial bodies. For all of these, from the most enormous sun to the most infinitesimal meteorite, are in constant relationship to each other, continually attracting each other, continually radiating and receiving light and heat. Newton long ago regarded it as the greatest of absurdities to imagine "that one body might act upon another at a distance, through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another. Gravity," he added, "must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."

This question, left unsolved by Newton, Hirn answers as follows:—"The thorough analysis of the most diverse facts revealed to us by science to-day allows us to reply to the first question by the most absolute negation. That which fills space and which establishes relations between the celestial bodies is not diffuse matter."

That "ponderable matter in the state of a diffuse gas" does not fill interplanetary and interstellar space M. Hirn seeks to prove by inquiring what effect such a medium would have on the various members of the solar system, and particularly upon their movements. Many of his conclusions are exceedingly striking, and if accepted certainly prove his main proposition given above. Perhaps the most remarkable is that relating to the secular acceleration of the moon. To explain a secular acceleration of $0''\cdot5$ in the mean motion of the moon it would be sufficient if 1 kilogramme of gas were distributed over 975,000 cubic kilometres of space; a rarefaction one million times greater than that of a Crookes vacuum of the millionth of an atmosphere. But the effect of the shock of the particles of this rarefied gas against a body like the moon as it moved forward in its orbit would be to raise the gas to a temperature of

38,000° C., and inconceivably attenuated as this interplanetary atmosphere would be, the moon would yet come into contact with 600 kilogrammes of it in each minute of time. On a body like the earth, surrounded by an atmosphere, the inevitable result of this unceasing collision with the interplanetary atmosphere would be the stripping away of the terrestrial atmosphere layer by layer. Arriving at results of a similar unacceptable character from the consideration of the action of a diffuse interplanetary gas on the other members of the solar system, M. Hirn decides that matter exists only in a sporadic state in space; only in the state of distinct bodies—stars, satellites, meteorites, and the like. It exists in a state of extreme diffusion only in nebulæ, but elsewhere space is perfectly empty, or, at least, whatever remains cannot suffice to explain the relations of stars to stars.

COMETS 1888 *e* AND *f* (BARNARD, SEPTEMBER 2 AND OCTOBER 30).—The following ephemerides for these objects are in continuation of those given in NATURE of April 4, p. 546, and are for Berlin midnight:—

	Comet 1888 *e*. R.A.	Decl.	Comet 1888 *f*. R.A.	Decl.
	h. m. s.	° ′	h. m. s.	° ′
April 30	23 25 51	1 28·1 N.	9 31 10	37 39·4 N.
May 4	23 23 34	1 41·6	9 33 48	37 35·8
8	23 20 54	1 54·1	9 36 36	37 30·4
12	23 17 50	2 5·5	9 39 38	37 23·7
16	23 14 18	2 15·7	9 42 50	37 15·8
20	23 10 15	2 24·5	9 46 12	37 6·7
24	23 5 37	2 31·6 N.	9 49 43	36 56·8 N.

α URSÆ MAJORIS.—Mr. Burnham reports from the Lick Observatory that he has discovered this star to be a close double. He gives the following measures of the companion:—

			Mag.
1889·142	P = 327°·0	D = 0″·96	11
1889·151	= 325·9	= 0·83	11

Mr. Burnham was not able to see the companion with the 12-inch telescope, and concludes that it is too difficult for such an aperture, the difference in magnitude between the two components being so great.

THE WHITE SPOT ON SATURN'S RING.—M. Terby, writing to the *Astronomische Nachrichten*, reports that he has not been able to see the white spot again which he observed on March 6 and 12 (NATURE, vol. xxxix. p. 497). MM. Knorre, Knopf, Lamp, Struve, and Schiaparelli have likewise failed to detect it. On the other hand, Prof. McLeod, of Montreal, and Mr. Brooks, of Smith Observatory, Geneva, U.S.A., both state that they have seen it; and the latter reports it variable. If it be a real spot, and not a mere effect of contrast with the shadow of the planet, it evidently would only occasionally be seen in the place where it was first discovered, but would be observed from time to time in other parts of the ring, for it would be carried round with it in its rotation.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 APRIL 28—MAY 4.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on April 28

Sun rises, 4h. 39m.; souths, 11h. 57m. 20·7s.; sets, 19h. 16m.: right asc. on meridian, 2h. 23·9m.; decl. 14° 18′ N. Sidereal Time at Sunset, 9h. 44m.

Moon (New on April 30, 2h.) rises, 4h. 36m.; souths, 10h. 54m.; sets, 17h. 25m.: right asc. on meridian, 1h. 20·3m.; decl. 3° 3′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	4 46	12 12	19 38	2 38·6	15 42 N.
Venus.....	4 9	12 7	20 5	2 33·2	20 42 N.
Mars......	5 8	12 51	20 34	3 17·9	18 30 N.
Jupiter....	0 15	4 11	8 7	18 35·9	22 55 S.
Saturn....	10 59	18 38	2 17*	9 5·7	17 51 N.
Uranus...	17 15	22 43	4 11*	13 11·7	6 54 S.
Neptune..	5 44	13 30	21 16	3 57·3	18 49 N.

* Indicates that the setting is that of the following morning.

April.	h.	
29	23	Venus in conjunction with and 10° 15′ north of the Moon.
30	12	Mercury in conjunction with and 5° 8′ north of the Moon.
May.		
1	2	Venus in inferior conjunction with the Sun.
1	3	Mars in conjunction with and 4° 21′ north of the Moon.
1	21	Mercury at least distance from the Sun.

Variable Stars.

Star.	R.A.	Decl.		
	h. m.	° ′		h. m.
U Cephei	0 52·5	81 17 N.	May 1,	2 12 *m*
U Monocerotis	7 25·5	9 33 S.	,, 3,	*m*
δ Libræ	14 55·1	8 5 S.	Apr. 30,	0 17 *m*
U Ophiuchi	17 10·9	1 20 N.	May 1,	1 44 *m*
			,, 1,	21 52 *m*
β Lyræ	18 46·0	33 14 N.	,, 2,	0 0 *M*
U Aquilæ	19 23·4	7 16 S.	,, 4,	0 0 *M*
η Aquilæ	19 46·8	0 43 N.	,, 1,	20 0 *M*
S Sagittæ	19 51·0	16 20 N.	,, 3,	22 0 *m*
R Sagittæ	20 9·0	16 23 N.	,, 3,	*m*
T Vulpeculæ	20 46·8	27 50 N.	,, 3,	2 0 *m*
			,, 4,	4 0 *M*
δ Cephei	22 25·1	57 51 N.	Apr. 28,	4 0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ζ Ursæ Majoris	206	57° N.	Slow; bright.
,, α Serpentis	234	10 N.	Swift.
,, υ Herculis	239	46 N.	May 1. Swift; short.
,, η Aquarii	337	2 S.	Swift; very long.

THE CORROSION AND FOULING OF STEEL AND IRON SHIPS.[1]

THE difficulty of obtaining adequate experimental data, and the fact that nearly everyone who has worked at the subject has had a composition of his own to bring before the public, has so hampered and restrained the free discussion and interchange of ideas on this most important question, that at the present time we have made but scant progress beyond the point reached twenty years ago, and my object in bringing this paper before you is more to excite you to discussion, and to show you the known facts of the case, than to tell you of any very new or startling discoveries.

Corrosion generally precedes fouling on exposed metal surfaces, and it is therefore this portion of the subject that will be considered first, together with the means which have been taken to prevent it and to protect the plates of our vessels from decay.

In a paper which I had the honour to bring before you two years ago, I pointed out that in all processes of rusting carbonic acid gas and moisture played an important part, the iron uniting with the carbonic acid and oxygen of the water to form ferrous carbonate whilst the hydrogen was set free, and that the ferrous carbonate then took up oxygen dissolved in the water, or present in the atmosphere as the case may be, and was decomposed into ferric oxide (rust) and carbonic acid, which being liberated in actual contact with the moist surface of the iron carried on the process of "rusting."

This view of the case was confirmed by a paper read by Prof. Crum Brown before the Iron and Steel Institute, at Edinburgh, last autumn, and is generally accepted as the true explanation of the corrosion taking place on iron or steel surfaces exposed to moist air or fresh water; but the rusting of the metal in sea water has by many chemists been ascribed to a more complex action, in which the salt present plays an important part by first forming oxychloride of iron.

This preliminary stage of corrosion in sea water is, I am inclined to think, a myth. When iron filings or turnings are exposed to the action of sea water, hydrogen gas is evolved, and ferrous oxide and carbonate are formed, and this changes, as in air or fresh water, into ferric oxide, by taking up dissolved oxygen present in the water. At no time have I been able to

[1] A Paper read at the thirtieth session of the Institution of Naval Architects, by Prof. V. B. Lewes, F.C.S., F.I.C., Royal Naval College Associate, on April 12, 1889.

detect the presence of oxychloride, and from the fact that a few drops of alkali added to the sea water stop the corrosion, I am of opinion that the simple rusting of iron in sea water is due to the same cause as in fresh—*i.e.*, the decomposition of the water by the iron in presence of carbonic acid.

The saline constituents of sea water, however, do undoubtedly play an important part in a more active form of corrosion, by helping to excite galvanic action between the iron in the plates and any foreign metal or impurities present, an action which is also materially aided by want of homogeneity in the metal, by particles of rust, by mill scale, by wrought and cast iron or steel in contact with each other; or even by the different amount of work, such as hammering or bending, undergone by different parts of the same plate; and in all of these cases the galvanic action set up causes rapid oxidation of the iron at the expense of the oxygen of the water, hydrogen being evolved.

We may therefore consider that on the skin of a ship two processes of rusting are going on, the simple corrosion on exposed surfaces of the metal, due to the presence of moisture, carbonic acid and free oxygen, which forms a fairly uniform coating of rust on the metal, and the more local corrosion due to galvanic action, which results in pitting and uneven eating away of the plates.

As I pointed out in a previous paper, rust cones are due to the most local form of galvanic action, caused by the presence of a spec of deposited copper, lead, or other foreign metal, or even a small particle of rust, or mill scale, left on the surface of the iron, and covered by the compositions used as protectives and antifoulers; as soon as the sea water penetrates to these, galvanic action is set up, water is decomposed, rust formed, and the escaping hydrogen pushes up the composition, forming a blister, the hydrogen leaks out, the water leaks in, the action becoming more and more rapid, and the blister gradually filling with the result of the action—rust. The blister bursts, but the cone of rust has by this time set fairly hard, and continues to grow from the base, the layers of rust being perfectly visible in a well-formed cone, and when the rust cone is detached, the pitting of the metal at the base of the cone is, as a rule, found to be of considerable depth.

The speck of foreign matter which has caused this destructive action generally clings to the surface of the iron, and, being at the bottom of the pitting, escapes detection and removal, and when the vessel, newly coated with fresh compositions, again goes to sea, the corrosion will again probably be set up in the same spot.

The corrosion of the plates in the interior of a vessel is a subject quite equal in importance to the external action of sea water and dissolved gases on the metal; and from the fact that certain portions of the interior plates, from their position, escape the frequent examination and attention bestowed upon the exterior, it becomes a still greater source of danger.

Corrosion, like all other forms of chemical action, is much accelerated by increase of temperature; and in the bottom of a ship, near the furnace-room and boilers, this has a considerable effect in increasing the rapidity of rusting. Also in the coal bunkers, the mere contact of moist coal with the iron plates sets up galvanic action, carbon being electro-negative to iron, and the coal dust which sifts down into the double bottom lends its aid to the destruction of the plates; whilst, if the coal contains any "pyrites," which is nearly always the case, these double sulphides of iron and copper are gradually oxidised into soluble sulphates of the metals, and these, washing down into the bilge water, would at once cause most serious corrosion, should they come in contact with any bare portion of the plates. Repairs to any portion of the inside plates will loosen rust and mill scale, which, finding its way into the bottom, tends to set up galvanic action: whilst the scale of oxide of copper from copper and brass fittings and pipes is another great cause of danger, as the bilge water would gradually convert it into soluble salts, which will deposit their copper upon the iron wherever a crack or abrasion enables them to come in contact with it; and finally, leakages from stores and cargo are in many cases of a character highly injurious to iron.

In addition to all these sources of danger, we must remember that the interior of the vessel is the part most liable to abrasion from shifting and moving of cargo, coals, &c.

The protection of the outsides of the bottoms of our ships from the destructive agencies of sea water and dissolved gases may be said to have been attempted in two ways, by metallic and by non-metallic coatings.

So far, all attempts at metallic coatings have proved failures, and, as far as it is possible to judge, there is but small likelihood of their ever being made to succeed, because if zinc is used in order to protect the iron of the ship there must be galvanic action, and this action must take place evenly all over the surface of the iron plates, which means that the sheathing must be in uniform metallic contact with the iron, in which case the wasting of the sheathing would be so rapid that it would have to be renewed frequently, which, even leaving out the question of cost, is in many cases impossible.

Zinc is practically the only metal which could be used for this purpose, in order to place the plates of the ship in an electro-negative condition, and it is, therefore, to zinc that inventors have turned from time to to time, the chief novelties introduced being the method of attachment. As far back as the year 1835, I believe Mr. Peacock tried zinc plates on the bottom of H.M.S. *Medea*, and in 1867 Mr. T. B. Daft again brought the subject forward; Sir Nathaniel Barnaby, Mr. McIntyre, and others, also suggesting various plans of attachment, whilst as late as last year Mr. C. F. Henwood read a paper at the United Service Institute strongly advocating zinc sheathing as attached by his system.

Where the galvanic contact has been but small there the sheathing has had a certain life, but has afforded but little protection to the iron, and has gradually decayed away in a very uneven fashion; whilst in those cases where galvanic contact has been successfully made the ship has on several occasions returned from her voyage minus a considerable portion of the sheathing.

Another drawback to the use of zinc sheathing is one which was found when it was used to coat wooden ships, and that is that zinc when in sheets, like every other metal, is by no means homogeneous, and that for this reason the action of sea water upon it, leaving out of consideration galvanic action, is very unevenly carried on, the sheathing showing a strong tendency to be eaten away in patches, whilst the metal itself undergoes some physical change, and rapidly becomes brittle.

Attempts have been made to galvanize the iron before the building of a ship; but Mr. Mallet showed as early as 1843 that this coating was useless when exposed to sea water, as in from two to three months the whole of the zinc was converted into chloride and oxide, and that, when, therefore, galvanising is used, care must be taken to protect the thin coating of zinc. This does not, of course, apply to fresh water, in which galvanised iron would answer very well, the rapid action being due to the salts in the sea water; but even in this case the galvanising would have to be done after the plates had been riveted together, as any breaking of the surface would set up rapid wasting of the zinc, and it could, therefore, only be used on small craft.

Copper, tin and lead have been proposed for coating ships, but these metals are electro-negative to iron, and would rapidly destroy the hull, should any abrasion of the coating or damage to the insulating material take place.

The non-metallic coatings which are intended to do away with corrosion have been almost endless. At the present moment there are upwards of thirty in the market; whilst the patent list of the last fifty years contains an enormous number which were practically still-born.

They may be divided for convenience into—

(*a*) Oil paints.

(*b*) Pitch, asphalt, tar, or waxes.

(*c*) Varnishes, consisting of resins and gums dissolved in volatile solvents.

(*d*) Varnishes, containing substances to give them body.

(*e*) Coatings of cement.

And, before going into these in detail, it is necessary to consider the condition of the surfaces to which they will have to be applied, and the effect this will have upon them.

Air has the power of holding water vapour in suspension, the amount so held being regulated by the temperature; the higher the temperature the more can the air hold as vapour, whilst any cooling of the air saturated at the particular temperature causes a deposition of the surplus moisture. When a ship is scraped down to the bare iron in the dry dock, we have a huge surface of metal which varies in temperature much more rapidly than the surrounding air, and cools much more rapidly than the stone walls of the dock; as it cools, so it chills the layer of air in immediate contact with it, and causes a deposition of the surplus moisture on its surface—a phenomenon known as the "sweating

of iron"—and on to this moist surface the protective composition has to be painted. If now a rapidly-drying varnish is put on, the rapid evaporation of the volatile solvent causes again another sudden fall of temperature—evaporation being always accompanied by loss of heat—and this fall of temperature again causes a deposition of moisture, this time on the surface of the protective, so that the coating is sandwiched between two layers of moisture, both of them probably acting deleteriously upon the resin or gum in the varnish, whilst the moisture on the iron also prevents adherence of the varnish to the metal. If, instead of a quick-drying varnish, the old-fashioned red lead and linseed oil protector had been used, the second deposition would not have taken place, but the sweating of the iron would have prevented cohesion, and, when dry, any rubbing of the coating would bring it off in strips.

The condition of the outer skin of a ship, when she is being coated with her protective composition, is one of the prime factors in the discrepancies found in the way in which compositions act. It being a very usual thing for a composition to give most satisfactory results on several occasions, and then, apparently under exactly similar circumstances, to utterly break down, and to refuse even to keep on. Too much stress cannot be laid upon the condition of the plates at the time of coating, and it is absolutely essential either to have a perfectly dry ship or else a composition which is not affected by water.

When an old ship is broken up, you will often see on the backs of the plates the numbers which had been painted on them with white lead and linseed oil before the ship was built, and, under the paint, the iron in a perfect state of preservation, the secret being that the paint was put on while the plates were hot and dry.

Boiled linseed oil, mixed with red or white lead, is amongst the oldest of the protective compositions in use, but of late years has been but little employed, since it was proved by M. Jouvin, of the French Navy, and also in this country, that compounds of lead, when exposed by the wasting of the vehicle to the action of sea water, are converted into chloride of lead, and this is rapidly acted on by the iron, depositing metallic lead and forming chloride of iron, the deposited lead carrying on the corrosion of the iron by rapid galvanic action. The drying of boiled linseed oil is due to the fact that it has in it a certain quantity of an organic compound of lead, and the drying properties are given to it by boiling it with litharge (oxide of lead), so that, even when red or white lead is not mixed with it, still lead compounds are present, and this action will go on to a lesser extent. When the boiled oil drys, it does so by absorbing oxygen from the air, and becomes converted into a sort of resin, the acid properties of which also have a bad effect upon iron, so that protectives containing boiled oil are open to objection. Within the last two months a good example of the action of sea water on the bottom of an iron ship, coated with red lead, has been afforded by H.M.S. *Nile*, which, after being painted over with coats of red lead, was allowed to remain for some months in Milford Haven, with the result that her bottom is very seriously corroded, and, on examination of specimens of rust taken from her, the crystals of metallic lead are in many cases easily identified.

If red lead is used, it can only form a ground-work for an anti-fouling composition which has to protect the red lead as well as the iron of the ship from the action of sea water, and when the anti-fouling composition and the vehicle perish, then serious corrosion must ensue.

The second class of protectives, consisting of tar and tar products, such as pitch, black varnish, and also asphalt and mineral waxes, are amongst the best protectives, the waxes especially not being affected by the sweating of the plates, and forming admirable coatings for the plates. Certain precautions, however, must be taken in the case of tar and tar products, both of which are liable to contain small quantities of acid and of ammonia salts; but if care be taken to eliminate these, and if it could be managed to apply this class of protectives hot to warm plates, the question of protection would be practically solved, bituminous and asphaltic substances forming an enamel on the surface of the iron which is free from the objections to be raised against all other protectives, that is, that being microscopically porous they are pervious to sea water.

The third class of protectives consists of varnishes formed by dissolving gums or resins in volatile solvents, such as spirit, turpentine, naphtha, fusel oil, &c., and such varnishes are open to several objections—in the first place, they are acted upon by moisture, which causes a deposition of the resins or gums as a non-coherent powder and destroys tenacity of the varnish. The amount of action which moisture has on such a spirit-varnish depends to a considerable extent upon the proportion of resin or gum to spirit, when the solvent is present in large quantities, and the resin in comparatively small; then the moisture has apparently little action; but it must be remembered that the drying of such protectives means the rapid evaporation of the solvent and concentration of the resin or gum, whilst the rapid volatilization which is going on cools the hull of the ship, and causes deposition of moisture on the drying varnish with most disastrous results.

Another point which must be borne in mind is that no such varnish is impervious to gases and liquids. We are apt to think of a coating of varnish as being perfectly homogeneous; but, on examining it through a microscope, it is seen to be full of minute capillary tubes, which become gradually enlarged by the action of water, and finally result in the destruction of the varnish, whilst moisture and dissolved gases find their way to the metal, and carry on corrosion. The application of several coats of varnish tends to diminish this evil, as in many cases the holes in the first coat will not correspond with the holes in the second, and so each succeeding coat will tend to make the protective more and more impervious. In using such varnishes, they must only be applied in favourable weather, and must be allowed to thoroughly harden before being brought in contact with the water.

In the fourth class we have varnishes of this kind to which body has been given by the addition of foreign constituents, generally mineral oxides; and this class is far preferable to the last, if the solvent used is not too rapid in its evaporation, and if care has been taken to select substances which do not themselves act injuriously upon iron or upon the gums or resins which are to bind them together, and are also free from any impurities which could do so.

At present the favourite substance used to give colour and body to such varnishes is the red oxide of iron, the colour of which effectually cloaks any rusting which may be going on under it. In using the red oxide for this purpose, care should be taken that it contains no free sulphuric acid or soluble sulphates, as these are common impurities, and are extremely injurious, tending to greatly increase the rate of corrosion. The finest coloured oxides are, as a rule, the worst offenders in this respect, as they are made by heating green vitriol (sulphate of iron), and in most cases the whole of the sulphuric acid is not driven off as the heat necessary impairs the colour; this acid is often neutralized by washing the oxide with dilute soda solution, but very little trouble, as a rule, is taken to wash it free from the resulting sulphate of soda, which is left in the oxide.

A sample of exceptionally good colour intended for using in protective compositions was sent me a few weeks ago for analysis, and proved to contain no less than 15·3 per cent. of sulphate of soda.

The best form of oxide of iron to use for this purpose is obtained by calcining a good specimen of hæmatite iron ore at a high temperature. When prepared in this way, it contains no sulphates, but from 8 to 40 per cent. of clay; if the percentage does not, however, exceed 12 to 18 per cent. it is perfectly harmless.

Composition manufacturers can easily test their red oxides for themselves, to see if it contains soluble sulphates, by warming a little of it with pure water, filtering through blotting paper, and adding to the clear solution a few drops of hydrochloric acid, and a little solution of chloride of barium (easily obtained at any druggist's). If a white sediment forms in the solution, the sample should be rejected.

In a previous paper[1] on the corrosion and protection of iron and steel ships, I pointed out that when such a varnish perished, the oxide of iron being left in contact with the iron plates, increased the corrosion going on at the surface of the metal, all oxides being electro-negative to the metals from which they are produced, and on that occasion I advocated the use of finely divided metallic zinc, which can be obtained as an impalpable powder, in place of the oxide of iron, pointing out that such a composition would last as long as any varnish of this class, and that, when the varnish perished, as it must do after long exposure to sea water, then the metallic zinc would, on coming in contact with the iron, set up galvanic action; but that, instead of being electro-negative, as in the case of oxide of iron, and causing corrosion of the plates, it would be electro-positive, and in consequence would protect them, being itself slowly oxidized, and so would give a fresh period of protection.

[1] Transactions of the Institution of Naval Architects, vol. xxviii.

I hoped at the time that I had made it perfectly clear that the zinc would in no way act until both the anti-fouling and protective varnish had perished, and had become spongy and porous, and that the idea was a prolongation of the period of protection, the great point which has now to be aimed at; but the remarks made afterwards in several journals which were kind enough to notice my paper showed me that they had mistaken my intention, and supposed that the zinc was put in to at once create galvanic action, and predicted that if by any chance it did act, the hydrogen generated would blow the composition into blisters, and defeat its own purpose. I need hardly point out that nothing was farther from my intention, as zinc in fine powder will be acted on more rapidly than the dense metal in plates, and I have already pointed out that this is destroyed too rapidly by galvanic action to render it of practical use as a protective *per se*.

As to the hydrogen blowing off the composition, no gas could be generated until both the anti-fouling and the protective coatings had been perished and rendered perfectly porous by the action of the sea water, a condition which would have permitted the free escape of the generated hydrogen, which, it must be remembered, will permeate through openings which other gases cannot pass through.

One of the largest firms of composition manufacturers had enough curiosity to try the effect of zinc *versus* oxide of iron, and painted a patch of it upon a ship coated with his compositions, and after a long voyage she returned with her protectives in perfectly good order, and had it not been for the patch containing zinc having had its position fixed by careful measurements, its whereabouts could not have been discovered. This is exactly what one would have expected; as long as the varnish remains intact oxide of iron, zinc, or, indeed, any substance which will not damage the varnish, does perfectly well; but had the vessel been allowed to continue until the varnishes had perished, then I venture to say that the patch containing the zinc would have shown better protection than those parts containing the oxide of iron. My ideas have undergone considerable modification during the past two years, but I still consider the views I put forward in my last paper were perfectly sound, and I am every day more and more convinced that the great object the composition-maker has to aim at is the prolongation of the life and effectiveness of compositions, and not the multiplication of short-lived devices, however admirable in their action.

In the fifth class of protectives we have cement coatings; but these, together with such schemes as the covering the hull of the vessels with vitreous glazes, glass, &c., have of late years, as far as I know, entirely been abandoned. The action of cement on iron, however, must later on be discussed in its important bearing on the protection of the interior portions of the hull, for which it is largely employed, its weight and the difficulty of attachment rendering it unfitted for outside work.

In selecting a protective composition for the bottom of a vessel, one of the second or fourth class should be chosen, attention being given to the points I have indicated, which are that in the bituminous and asphaltic compositions all the original acids must be eliminated, and that in the varnishes of the fourth group quickly evaporating solvents should be avoided, and, if possible, zinc substituted for oxide of iron.

The vessel should have her plates as dry as possible during the application of the protective, and, if feasible, days on which the air is fairly dry should be chosen. The protective should not be too thick, as, if it is, it does not readily fill into inequalities in the plates; and, if in this way any air is inclosed, change of temperature will cause it to expand or contract, thus causing a blister to form, which will fill with sea water and set up rapid corrosion. The composition must either be elastic or else have the same rate of expansion and contraction as the iron; for, if not, the change of temperature will cause cracking and tearing of the composition with disastrous results. The vessel, if she has to be scraped down to the bare metal, must be scrubbed free from all traces of rust, and where a well-adhering coating of composition exists, it should be painted over and not disturbed. In the case of a new ship, she must be pickled with dilute acid, to get rid of every trace of mill scale, and then washed down with some slightly alkaline liquid to neutralize every trace of acidity, the alkali in turn being removed by clean water. Under these conditions, and given a composition with good adhering properties, but little apprehension need be felt as to the ravages of corrosion on the metal of a ship's bottom, the chief risk being from abrasion and other mechanical injury to the composition, coupled with improper constituents in the anti-fouling compositions. The protection of the interior portion of the vessels, where the plates are exposed to the corroding action of bilge water, rendered more active by a high temperature, leakage from cargo, acids and sulphates from wet coals, and the presence of such electro-negative factors as coal dust, scale, and rust, is a matter of quite as great importance as the exterior protection; whilst the great chance of mechanical abrasion during coaling and shifting of cargo, as well as the difficulty of getting at the lower portions of the hold to examine the condition of the plates, renders it a question of the gravest consideration. The corrosion found in the portions underneath the engine-seats, the bunkers, and the water-ballast chambers, especially near the engine-room, is often very serious, and needs most careful watching, which, from the position of these parts of the vessel, it is very hard to bestow upon it.

It must also be remembered that the bilge water in a vessel is in constant motion, and that the air in these parts of the vessel may be expected to be exceptionally rich in carbonic acid gas, which, as I have before shown, is the most important factor in corrosion. Under these conditions any abraded portion would probably be continually washed over, and then exposed to the foul air, a condition of things most conducive to rapid rusting. There are three main classes of protectives for the interior of a ship—

(1) Cements.
(2) Bituminous coatings.
(3) Paints.

The first of these, the cement coatings, have many good points to recommend them, but they also have many serious drawbacks.

The rigidity, firmness of adherence and endurance, are all of them points of the greatest importance, and there is no doubt but that the silicates present in the cement in time, not only bind the cement into a mass of wonderful hardness, but also bind that cement to the iron. A point to which I should like to draw your attention, however, is that a thin coating of Portland cement is highly porous, and that it can be permeated by liquids and gases. Suppose, now, that some copper scale from the interior fittings had fallen into the bottom of the vessel, and had been converted into soluble salts of copper by the saline bilge water, this solution would soak through the capillary orifices in the cement, until it came in contact with the iron below, when the copper would be deposited on the iron, and rapid galvanic action set up, the cement being loosened, and to a certain extent lifted, by the formation of rust, whilst corrosion would gradually extend under the cement, giving on the outside of the coating but little sign of damage taking place below it.

Also the hardness and rigidity of the cement gives it a tendency to crack away from the metal when any strain is thrown upon the plates, or during any expansion or contraction of the metal; whilst any repairs on the outside of the ship, such as making a boring to test the thickness of plate, replacement of rivets, &c., would undoubtedly cause a loosening of the cement coating within, and, wherever a loosening takes place, the space between the cement and the plate will quickly be found to become a starting-point for corrosion, which quickly spreads and loosens the cement, and will only be discovered by chance.

It is for this reason that I consider bituminous or asphaltic varnishes, freed from any trace of acid, and applied hot, or sound tough paint, preferable to cement; as, although they are not so hard, yet if serious corrosion should be set up, it is easily discovered and stopped before much damage results, whilst, being impervious to moisture, deleterious solutions, either from the coal bunkers or cargo, would be prevented from acting upon the skin of the ship.

In approaching the subject of fouling, one is impressed with the apparent hopelessness of obtaining any reliable information from the successes or failures registered by the bottoms of the vessels, in the Service, or in the Mercantile Marine. Hundreds of ships may be examined, and their condition and the nature of the compositions used upon them registered, and just as one begins to feel that the key to the mystery is within one's grasp, a whole series of results so abnormal suddenly comes to light that it seems impossible to reconcile them with one's previous experience. A ship may sail half a dozen times to the same waters, coated with the same composition—on four occasions she will come home clean and in good condition, whilst on the other two voyages she may accumulate an amount of weed and animal life sufficient to knock down her speed from nine knots to five. Moreover, if the compositions with which she was coated be examined, and scrapings taken from her on her return, no cause will present itself that

can in any way explain the great difference in her condition. After several years' close observation, however, certain factors begin to make themselves apparent. Ships at sea from March to August show a worse average than those afloat from August to March; one also begins to realize that the amount of fouling increases enormously if the ship has been long at anchor—ships which have been lying at the mouths of rivers, although quite clean in the brackish water, foul much more rapidly on going to sea than the vessels which have been cruising, or even at anchor for the same time in salt water; and finally, certain ports and certain seas seem to exercise a deleterious effect, both as regards corrosion and fouling, which is not to be found elsewhere.

Turning back to the naval history of the past, we find that fouling is no new trouble born with the advent of our present iron monsters; but that it has been the one trouble that the combined engineering and scientific skill of many centuries has been unable to overcome.

With our wooden ships, metallic copper sheathing, if it were of the best kind, answered the purpose fairly well; but then the copper wasted so fast the inferior kinds and alloys were substituted to prevent the rapid loss, and, with the slowing down of the destruction of the copper, at once the trouble of fouling returned.

When iron ships began to replace the wooden ones, as was only natural, attempts were made to utilize the metal which had before given relief; but it was quickly found that the effect of the galvanic action set up by the copper was fatal to the iron plates of the ship, and attempts were then made to sheathe the ship with copper plates in such a way that they should be insulated from the iron of the vessel, a condition almost impossible to attain and attended with great risk, should any accidental injury to the insulation take place.[1] Early in the history of iron shipbuilding the idea was started of using coatings of paint, so prepared as to fulfil the same functions as the copper plates had done; but from 1840, when the first paint of this kind was patented, down to the present day, when there are upwards of thirty-two different compositions in the market, very little progress has been made in their manufacture, and the best of their compounds cannot be relied upon for keeping a ship's bottom even fairly free from fouling for periods extending beyond nine months, and I am personally convinced that the reason of this is to be found in the fact that a start was originally made in the wrong direction.

The idea which originally led to the present class of anti-fouling compositions was that the copper salts formed by the action of the sea water on the metallic sheathing owed a considerable portion of their value as anti-foulers to the poisonous action they exerted upon marine animal and vegetable growths; but, when an observer comes to study the natural history of these lower forms of animal life and vegetation, it is gradually forced upon one that it is only in the early stages of their growth—the germ period—that metallic poisons can affect them. Seaweeds do not take in the constituents they require for their growth by means of their roots, as is, to a certain extent, the case with ordinary plants, but absorb them by means of their pores from the water itself, the root only serving to attach them to the solid they choose for their resting-place; it is also well known that when once a marine plant which has passed the first stages of existence is dislodged or torn from its support, it cannot reattach itself to anything else, whilst most of the mineral poisons have little or no effect upon their life and growth.

In the same way we find that, with the animal life found on a ship's bottom, the under side is used to cling on with only, and not as an extractor of nourishment, and that, therefore, after the seeds and germs have once obtained a foothold on the side of the vessel, no amount of poison which can be put into a composition will have any effect upon them. Metallic poisons undoubtedly do exert an influence upon the germs in their earliest stages; but after that they are perfectly useless as anti-foulers, and only imperil the plates of the vessel.

The germs of both kinds of growth are of necessity more abundant in the surface water near shore than in deep water, and therefore the period when the ship is in port is the time when the germs are most likely to make good their attachment, after which their further development is, unless other methods of getting rid of them are employed, merely a matter of time.

On examining the conditions under which a vessel is placed when coated with a composition which relies for its anti-fouling powers on metallic poisons only, we at once see the reasons which must make such a coating of little or no avail. In the composition we have drastic mineral poisons, probably salts of copper, mercury, or arsenic, which have been worked into a paint by admixture with varnishes of varying composition, and each particle of poison is protected from the action of the sea water by being entirely coated by this vehicle: that this must be so is evident, or the composition would not have sufficient cohesive power to stick on the ship. As a rule, care is taken to select fairly good varnishes, which will resist the action of sea water for, perhaps, two or three months, before they get sufficiently disintegrated to allow the sea water to dissolve any of the poison; whilst, even with the accidental or intentional use of inferior varnishes, three or four weeks will pass before any solution can take place, and any poison be liberated to attack the germs. A ship is dry docked, cleaned, and, her anti-fouling composition having been put on, she goes probably into the basin to take in cargo. Here she is at rest, and, with no skin friction or other disturbing causes to prevent it, a slimy deposit of dirt from the water takes place, and this, as a rule, is rich in the ova and germs of all kinds of growth, whilst the poisons in her coating are locked up in their restraining varnish, and are rendered inactive at the only period during which they could be of any use. After a more or less protracted period, the ship puts to sea, and, the perishing of the varnish being aided by the friction of the water, the poisonous salts begin to dissolve or wash out of the composition; but the germs have already got a foothold, and with a vessel sweeping at a rate of, say 10 to 12 knots through the water, the amount of poison which can come in contact with their breathing and absorbing organs is evidently so infinitesimally minute that it would be impossible to imagine it having any effect whatever upon their growth. If the poison is soluble, it is at once washed away as it dissolves; if it is insoluble, then it is also washed away, but there is just a chance that a grain or two may become entangled in the organs of some of the forms of life, and cause them discomfort. As the surface varnish perishes, the impact of the water during the rapid passage of the vessel through the water quickly dissolves out or washes out the poisonous salts, and leaves a perished and porous, but still cohesive, coating of resinous matter, which forms an admirable lodgment for anything which can cling to it; and by the time the vessel lays-to in foreign waters, teeming with every kind of life, the poison which would now again have been of some use is probably all washed away, and a fresh crop of germs are acquired, to be developed on the homeward voyage, and a "bad ship" is reported by the person who looks after her docking. It is evident that a poison, even if it had the power of killing animal and vegetable life in all stages, could only act with the vessel at rest, unless it were of so actively corrosive a nature as to burn off the roots and attachments of the life rooted to it, and if it did this, what, may I ask, would become of the protective composition and the plates of the vessel? And I think it is also evident that any poison so used must be under conditions in which it is very unlikely to be in a position to act when it might do good.

The lamentable failure of composition after composition of this kind has gradually reduced them in number to some ten or twelve at the present time, and in most cases it is low price alone which keeps them in the market.

The practical proof, given by experience, that poisons alone are unable to secure a clean bottom, soon led many inquirers to the conviction that it was the exfoliation in the case of copper which had acted in giving fairly good results, and in many compositions the attempt has been made to provide a coating which shall slowly wash off, and, by losing its original surface, shall at the same time clear away germs and partly developed growths, and so expose a continually renewed surface, in this way keeping the bottom of the vessel free from life. There is no doubt that, when this is successfully done, a most valuable composition will result, but the practical difficulties which beset this class of anti-foulers must not be overlooked. In order to secure success, the composition must waste at a fairly uniform rate, when the ship is at rest, and also when she is rushing through the water; and this is the more important in the case of Service vessels, as in many cases they spend a large percentage of their existence at anchor, or in the basins of our big dockyards. If a composition is made to waste so rapidly that it will keep a vessel clean for months in a basin, then you have a good composition *for that purpose*; but send the vessel to sea, and under conditions where you have a higher temperature, and the enormous friction caused by her passage through the water exerting its influence upon the composition, and you will find that the coating, which did its work well for six months at rest in the basin, will, in the course of one month under these altered conditions, be all washed

[1] Some copper sheathed vessels still exist, and its revival has been lately warmly advocated in America.

away, and fouling will be set up. Noting this result, the manufacturer renders his composition more insoluble—less wasting—and so obtains a coating which, when the vessel is in motion, scales just fast enough to prevent fouling, and good results at once follow; the composition is then put on the same or other vessels, and they take a spell of rest in the basin, and, bereft of the aid of the higher temperatures and the friction of the water, the composition ceases to waste fast enough, and bad results at once have to be recorded.

There is no doubt that this is the true explanation of the wide discrepancies which are found between the compositions in the Navy and in the Mercantile Marine: take any of the big lines, their steamers are running at a fairly uniform rate of speed, and the periods of inaction are as short as the desire not to waste the charge on the capital they represent can make them, and under these conditions, by varying the constituents in the varnishes used for anti-fouling purposes, it is fairly easy, given the necessary data, to so constitute a composition as to secure admirable results; but when you come to apply this same coating to an ironclad running at various speeds, and as often at rest as in motion, then you at once find that the composition you before imagined to be all that could be desired fails just as lamentably as the tribe of anti-foulers which preceded it. It is not so very long ago that I had the honour to serve on an Admiralty Committee under the able guidance of Admiral Colomb, and, after inspecting many vessels in the Mercantile Marine, and watching all the dockings of Service vessels over a considerable space of time, we were forced to the conviction that it was only in very rare cases that the condition of the bottoms of Her Majesty's ships at all approached the freedom from fouling to be found in the ships belonging to the big companies, with the result that some of the most successful of the compositions in the Mercantile Marine were brought into use in the Navy, and I believe the reports of the dockings since they have been adopted will amply prove the existence of the difficulties I have mentioned.

Another factor which is often overlooked, and which tends to give misleading results, is the action of brackish water, which, in many cases, seems to exert a special action in keeping the bottom of a vessel clean, the fresh water having a tendency to disagree with certain forms of marine growth, whilst the salt water is apparently equally unpalatable to the fresh-water forms of fouling.

In most of the compositions now in use, attempts are made to combine strongly poisonous substances with exfoliating and wasting coatings, and this is done by either using metallic soaps, the basis of which is, as a rule, copper, or else by charging a perishable and easily washed-off varnish with poisonous salts, consisting, as usual, of compounds of either copper, mercury, or arsenic, and in some cases all three.

As I have before pointed out, I do not think the presence of these substances exerts any deterrent action upon the fouling, save perhaps when the vessel is at rest; but they exert undoubtedly an important influence upon the rate of exfoliation, as when the perishing of the varnish exposes them they dissolve, or are washed out, and in this way tend to disintegrate and clear away the surface more rapidly—an important and decidedly useful function, but one which might be more cheaply performed by substances other than high-priced metallic poisons.

The use of metallic poisons of the character indicated throws an increased burden upon the protective composition, as, should the latter become abraded by friction of chain cables, barges alongside, or any other cause, the iron of the vessel will be attacked by the metallic salts, either present in the soluble form in the anti-fouling composition, or rendered so by the solvent action of the saline constituents of the sea water, the action of the metallic salts being to rapidly dissolve portions of the iron, and to deposit the metal which they contain upon the surface of the plates, and these deposits, exciting energetic galvanic action, cause corrosion and pitting to go on with alarming rapidity. Both mercury and copper salts are offenders in this way, but copper is by far the most objectionable, from the fact that the salts formed by the action of the sea water upon the compounds used in the compositions are far more soluble than the corresponding salts of mercury, and are therefore liable to be present in much larger quantity, and so exert comparatively a much more injurious action on the plates.

As an illustration of this, two equal portions of sea water were saturated, the one with copper chloride, the other with mercuric chloride, and into each a piece of steel, planed upon one side, and of about equal weight and size, was placed, and left for four days. At the end of this period the two plates were removed, and, after being cleaned and dried, were again weighed, when it was found that the one exposed to the copper-saturated sea water had lost 22·2 per cent. in weight, while the plate exposed to the mercurial solution had only lost 3·6 per cent., this being due to the much larger amount of the copper salt soluble in the sea water.

On now placing these plates in clean sea water, corrosion went on in each case with extreme rapidity, and after being exposed for a month, they had both wasted to about the same extent—that is to say, when once deposited on the iron, mercury is practically as injurious as copper.

I am quite aware that this experiment is not at all likely to be carried out in practice, and none can have a greater conviction of the inutility of small laboratory experiments than I have, as they lack all the factors of mass of material and atmospheric influence which play so important a part in a question like the present; but such an experiment gives one a definite and fairly correct idea of the relative rate of action of the two poisons upon the plates.

All the time the ship is in motion, the wash of the sea water will prevent the metallic poisons doing the plates or the marine growths much harm, but there is one phase of this question which I think has been overlooked. I need not point out that in certain ports there is a fashion in compositions, and that most of the homes of the Mercantile Marine have some pet local composition which is largely used at the particular port. If, now, many ships are laying in a basin, taking in and discharging cargo, and if the prevalent compositions contain copper, it is evident that a certain quantity will go into solution in the water, which often does not undergo frequent or rapid change, and under these conditions every ship in the basin will be exposed to the same danger, and wherever an abrasion has taken place in the protectives, there copper will be deposited on the iron, causing corrosion and destruction of the plates; and it must be remembered that when the vessel is next docked and coated no amount of scraping will remove the fine particles of copper deposited in the pitted and corroded portions of the plate, and so finely divided as to be invisible to the eye, but that they will remain and carry on the destructive work under the new coatings of protective.

It is, I think, a well-recognized fact that, when a vessel coated with a copper compound has become corroded from failure of her protective, or from abrasion, even an entire change of composition does little or no good in stemming the tide of corrosion, until after some considerable period has elapsed, a result which is due to the same cause; and, inasmuch as copper compositions are a source of danger, not only to the ships coated with them, but to any others which may be at rest in the same basin, I do strongly urge upon the manufacturers to abandon the use of these deleterious compounds, and to use others equally efficacious and free from the grave objections I have enumerated.

At the present time, 15 out of 32 principal compositions rely upon copper in some form or other as the basis of their anti-fouling composition, and in one which has enjoyed considerable favour finely divided metallic copper itself is used, and should vessel coated with it, after the varnishes had commenced to disintegrate, be moored alongside an iron ship by a chain cable, or even by a wet hawser, a big galvanic couple would be formed at the expense of serious damage to any exposed iron.

In the history of anti-fouling many attempts have been made to obtain highly glazed and glass like-surfaces which it was hoped would withstand the action of sea water, and afford no lodgment to marine growths; but even glass itself is slowly acted upon by sea water, and, when once roughened on the surface, will foul, whilst the rigidity of such coatings, and the straining and cracking consequent on unequal expansion and contraction of the plates and their coating, offers a serious obstacle to any such scheme.

In concluding this long paper, I wish to point out that in the present phase of the anti-fouling question, and until some new principle for preventing marine growth has been advanced and successfully adopted, satisfactory results can only be insured by an intelligent use of the existing compositions.

The protective composition is the important composition, and care must be taken to obtain the best in the market, as, if the protection is good, the plates remain uninjured even if fouling take place. The anti-fouling composition to be used with it must either be elastic, or have the same rate of contraction and expansion as the protective, and must—at any rate in the Navy—be chosen to suit the work to be done, such as contain copper compounds being carefully rejected, whilst preference should be given to those which rely on exfoliation rather than mineral poisons.

If a vessel is to remain at rest for a considerable period, an anti-fouling composition which exfoliates rapidly, and which also contains poisons known to act on germ life, must be used, the amount of such poison depending on the seasons and the waters in which the ship is to be; whilst if a vessel is to be continually running, then a slowly exfoliating composition must be employed, and a very small percentage of poison is all that is required, as skin friction and the comparative absence of the germs and spores in deep water will do the rest.

Our ships represent an enormous capital, and any trouble or care which will prolong their existence is well worth taking and will be amply repaid, and at the present time a heavily corroded and foul vessel means either ignorance or negligence on the part of those who have the responsibility of deciding on the compositions to be used; and, finally, it must be clearly borne in mind that there is no anti-fouling composition which ever has been made, or probably ever will be made, that will answer for all cases, and that, until this is clearly recognized, the present unsatisfactory condition of the question will exist.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 28.—"The Diurnal Variation of Terrestrial Magnetism." By Arthur Schuster, F.R.S., Professor of Physics; with an Appendix by H. Lamb, F.R.S., Professor of Mathematics, Owens College, Manchester.

In the year 1839, Gauss published his celebrated memoir on "Terrestrial Magnetism," in which the potential on the earth's surface was calculated to twenty-four terms of a series of surface harmonics. It was proved in this memoir that if the horizontal components of magnetic force were known all over the earth the surface potential could be derived without the help of the vertical forces, and it is well known now how these latter can be used to separate the terms of the potential which depend on internal from those which depend on external sources.

The use of harmonic analysis to separate internal from external causes has never been put to a practical test, but it seems to me to be specially well adapted to inquiries on the causes of the periodic oscillations of the magnetic needle.

If the magnetic effects can be fairly represented by a single term in the series of harmonics as far as the horizontal forces are concerned, there should be no doubt as to the location of the disturbing cause, for the vertical force should be in the opposite direction if the origin is outside from what it should be if the origin is inside the earth.

In any case, the differences between the two results will be of the same order of magnitude as the vertical force itself. If it were then a question simply of deciding whether the cause is outside or inside, without taking into account a possible combination of both causes, the result should not be doubtful even if we have only an approximate knowledge of the vertical forces.

Two years ago I showed that the leading features of the horizontal components for diurnal variation could be approximately represented by the surface harmonic of the second degree and first type, and that the vertical variation agreed in direction and phase with the calculation on the assumption that the seat of the force is outside the earth. The agreement seemed to me to be sufficiently good to justify the conclusion that the greater part of the variation is due to causes outside the earth's surface. Nevertheless, it seemed advisable to enter more fully into the matter, as in the first approximate treatment of the subject a number of important questions had to be left untouched. I now publish the results of an investigation which has been carried out as far as the observations at my disposal have allowed me to do. My original conclusions have been fully confirmed, and some further information has been obtained, which I believe to be of importance.

I have made use of the observations taken at Bombay, Lisbon, Greenwich, and St. Petersburg. The horizontal components of the diurnal variation during the year 1870 were in the first place reduced to the same system of co-ordinates and to the same units. If we remember that experience has shown the diurnal variation to be very nearly the same for places in the same latitude, except near the magnetic pole, and also that it is symmetrical north and south of the equator, we may for a given time of day assume the horizontal components known over eight circles of latitude, four of which are north and four south of the equator.

From the horizontal components, the potential was calculated in terms of a series of surface harmonics. It was found that in order to represent both the summer and the winter effect with sufficient accuracy thirty-eight terms were necessary. In this calculation the vertical forces were not made use of at all.

From the potential, as calculated from the horizontal components, we can deduce the vertical force, either on the assumption that the variation is due to an outside cause, or that it is due to an inside cause; and compare the vertical forces thus found with the vertical forces as actually observed.

If we put both into the form

$$r_n \cos n(t - t_n),$$

we can obtain an idea of the agreement as regards amplitude and phase for each harmonic term. The following tables give the results for $n = 1$ and $n = 2$—that is, for the diurnal and the semi-diurnal variation:—

TABLE I.

Observed and calculated Values of the Coefficients t_1 and t_2 of Vertical Force, when expressed in the form $r_1 \cos (t - t_1) + r_2 \cos 2(t - t_2)$, on the supposition that the Disturbing Force is *inside* the Earth.

	t_1.			t_2.		
	Calc.	Obs.	Diff.	Calc.	Obs.	Diff.
	h. m.	h. m.	h. m.	h. m.	h. m.	h. m.
Bombay	23 02	11 13	+11 49	9 55	4 23	+ 5 32
Lisbon	22 35	10 40	+11 55	11 42	5 50	+ 5 52
Greenwich	22 06	8 42	−11 54	11 32	5 56	+ 5 36
St. Petersburg, 1870	21 16	3 10	− 5 54	10 48	7 05	+ 3 43
,, 1878	...	7 05	− 9 49	...	6 12	+ 4 36

TABLE II.

Observed and calculated Values of the Coefficients t and t_2 when expressed in the form $r_1 \cos (t - t_1) + r_2 \cos 2(t - t_2)$, on the supposition that the Disturbing Force is *outside* the Earth.

	t_1.			t_2.		
	Calc.	Obs.	Diff.	Calc.	Obs.	Diff.
	h. m.	h. m.	h. m.	h. m.	h. m.	h m.
Bombay	11 10	11 13	+ 0 03	3 47	4 23	− 0 36
Lisbon	10 37	10 40	− 0 03	5 46	5 50	− 0 04
Greenwich	10 03	8 42	+ 1 21	5 38	5 56	− 0 18
St. Petersburg, 1870	8 52	3 10	+ 5 42	4 38	7 05	− 2 27
,, 1878	...	7 05	− 1 47	...	6 12	− 1 34

In Table I. the comparison of the observed phases is made with the values calculated on the assumption that the disturbing force is inside the earth. In Table II. the same comparison is made on the alternative hypothesis. There is complete disagreement in Table I. between the observed and calculated values, and nearly complete agreement in Table II. It is seen how both at Lisbon and Bombay the time of maximum displacement agrees within three minutes of time for the diurnal variation, and at Lisbon within four minutes of time also for the semi-diurnal variation. Considering that Lisbon is the most important station, not only on account of its geographical position, but also because the observed vertical forces apply to the same year as the calculated ones, the result is strikingly in favour of the outside force. The results for Greenwich argue in the same direction. As regards St. Petersburg, the results for 1870 neither agree with one nor with the other hypothesis. The observations for 1870 are, however, doubtful, but the results for 1878 agree well with the hypothesis of an outside disturbing force.

The observed amplitudes are found in all cases to be considerably smaller than the calculated ones.

If we then take it as proved that the primary cause of this variation comes to us from outside the earth's surface, we are led to consider that a varying magnetic potential must cause induced currents within the earth, if that body is a sufficiently good conductor. These induced currents might be the cause of the apparent reduction in amplitude. As my colleague, Prof. Lamb, has given considerable attention to the problem of currents in a conducting sphere, I consulted him, and he

gave me the formulæ by means of which the induced currents can be calculated. This investigation is given in an appendix to the paper. The result is very interesting. If the earth is treated as a conducting sphere, the observed reduction in amplitude is accounted for, but that reduction should be accompanied by a change of phase which is not given by observation. We can reconcile all facts if we assume, as suggested by Prof. Lamb, the average conductivity of the outer layers of the earth to be very small, so that the reduction in amplitude is chiefly due to currents induced in the inner layers. If the conductivity inside is sufficiently large, a considerable reduction in amplitude would not be accompanied by a sensible change of phase. We have arrived, therefore, at the following result:—

The vertical forces of the diurnal variation can be accounted for if we assume an outside cause of the variation, which induces currents in the earth, and if the earth's conductivity is greater in the lower strata than near the surface.

Prof. Balfour Stewart's suggestion that convection currents in the atmosphere moving across the lines of the earth's magnetic forces are the causes of the daily variation, gains much in probability by this investigation. If the daily variation of the barometer is accompanied by a horizontal current in the atmosphere similar to the tangential motion in waves propagated in shallow canals, and if the conductivity of the air is sufficiently good, the effects on our magnetic needles would be very similar to those actually observed. The difficulty as to the conductivity of the air is partly met by the author's investigation of the behaviour of gases through which electric discharges are passing.

It will be interesting to follow out the investigation, especially with a view of examining the influence of sun-spot variation. The question of magnetic disturbances is more complicated, but as magnetic observatories are being established in many countries, the time may not be far distant when we shall be able to bring the irregular disturbances within the reach of calculation.

The author acknowledges the help he has received from Mr. William Ellis in some of the reductions; he has also to thank his assistant, Mr. A. Stanton, for much labour bestowed on making and checking numerical calculations.

Royal Meteorological Society, April 17.—Dr. W. Marcet, F.R.S., President, in the chair.—The following papers were read:—On the deaths caused by lightning in England and Wales from 1852 to 1880, as recorded in the returns of the Registrar-General, by Inspector-General R. Lawson. The total number of deaths from lightning during the twenty-nine years amounted to 546, of which 442 were of males, and 104 of females. In consequence of their greater exposure, the inhabitants of rural districts suffer more from lightning than those of towns. It appears also that vicinity to the west and south coasts reduces the chances of injury by lightning, and that distance from the coast and highland seems to increase them.—The diurnal range of the barometer in Great Britain and Ireland, by Mr. F. C. Bayard. The author has reduced the hourly records of the barometer at the nine Observatories, Aberdeen, Armagh, Bidston, Falmouth, Glasgow, Greenwich, Kew, Stonyhurst, and Valencia, during the years 1876–80. The curves of inland places are smoother than those of places on the sea-coast, and the curves of places to the westward are more irregular than those of places to the eastward. As we go from south to north the general tendency of the curve is to get flatter with a lessened diurnal range.—Note on a working model of the Gulf Stream, by Mr. A. W. Clayden. The author showed this interesting model at work; it has been constructed to illustrate the formation of ocean currents in general and of the Gulf Stream in particular.—On the rime-frost of January 6 and 7, 1889, by Mr. C. B. Plowright. The author gives an account of the very heavy rime which occurred in the neighbourhood of King's Lynn on these days, when the fringe of crystals upon twigs and branches of trees was about 2 inches in length. The weight was so great that nearly all the telegraph wires were snapped and an immense number of branches of trees broken off.

Zoological Society, April 16.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary exhibited a pair of a fine large Buprestine Beetle of the genus *Julodis* (*Julodis ffinchi*), obtained by Mr. B. T. Ffinch near Karachi; and a Mole-cricket (*Gryllotalpa vulgaris*), sent by Mrs. Talbot from Bagdad.—Mr. Sclater made some remarks on the animals he had noticed during a recent visit to the Zoological Gardens of Rotterdam, Amsterdam, and Antwerp.—A communication was read from Mr. A. H. Everett, containing remarks on the zoo-geographical relationships of the Island of Palawan and some adjacent islands. In this paper it was contended that Palawan and the other islands intervening between Borneo and Mindoro form an integral portion of the Bornean group, and do not naturally belong to the Philippine Archipelago, with which they have hitherto been treated. The writer founded his contention upon the grounds (1) that the islands in question are connected with Borneo by a shallow submarine bank, while they are separated from the Philippines by a sea of over 500 feet depth; and (2) that a comparison of the Bornean and Philippine elements in the fauna of Palawan, so far as it is known, shows a marked preponderance of the former over the latter element; while the Philippine forms are also more largely and more profoundly modified than the Bornean species. This fact indicated that they had been longer isolated, and consequently that the fauna of Palawan was originally derived from Borneo, and not from the Philippines, though a considerable subsequent invasion of species from the latter group had taken place.—A communication was read from Mr. Oldfield Thomas, containing an account of the mammals of Kina Balu, North Borneo, from the collections made on that mountain by Mr. John Whitehead in 1887 and 1888. The species represented in Mr. Whitehead's collection were 21 in number, of which six had proved to be new to science.—Mr. G. A. Boulenger read the second of his communications on the fishes obtained by Surgeon-Major A. S. G. Jayakar at Muscat, on the east coast of Arabia. The two collections recently received from Mr. Jayakar contained examples of 80 species not included in Mr. Boulenger's former list.

PARIS.

Academy of Sciences, April 8.—M. Des Cloizeaux, President, in the chair.—Fixation of nitrogen by vegetable soil with or without the aid of leguminous plants, by M. Berthelot. The paper deals with a fresh series of sixty-four methodic experiments carried out during the year 1888, and fully described in the April number of the *Annales de Chimie et de Physique*. They form a sequel to the systematic researches begun by the author in 1883, and tend fully to confirm the views already announced by him on the fixation of free nitrogen in the ground effected either with or without the co-operation of luzern, vetches, and other leguminous plants. He considers the fixation now fully established, and finds in this fact the true interpretation of a multitude of phenomena highly important to agriculture.—Experiments on putrefaction and the formation of manures, by M. J. Reiset. The more recent experiments here described fully confirm the results of those undertaken by the author so far back as 1854, and show that, in the process of organic decomposition, nitrogen is not fixed, but liberated.—On the identity of erysipelas and acute lymphangitis, by MM. Verneuil and Clado. The researches of the authors in the Hospital de la Pitié show that these are not two distinct disorders, as is often assumed, but merely two forms of the same contagious, infectious, and parasitic disease, due to a special microbe easily recognized, isolated, cultivated, and inoculated in animals. This microbe, hitherto discovered in erysipelas alone, has now also been detected in acute lymphangitis with all its characters and biological properties.—On the influence of refraction in the reduction of the observations of a meridian transit, by M. G. Rayet. The conditions already described by the author in his communication on the influence of refraction in the reduction of the observations of the circumpolar stars (*Comptes rendus*, March 11, 1889), are here shown to be equally applicable to the reduction of the observations of transits at any declination.—Direct determination of the compressibility of glass, crystal, and metals, up to 2000 atmospheres, by M. E. H. Amagat. By direct determination is here meant a determination effected without employing any formula. The results already communicated in recent notes were for slight pressures only; hence these further experiments have been undertaken for the purpose of ascertaining whether, under very high pressures, the compressibility of glass, crystal, &c., undergoes any considerable diminution. The process employed is that adopted by Mr. Buchanan, and afterwards by Prof. Tait in their researches.—On the intensity of telephonic effects, by M. E. Mercadier. During his researches on the theory of the telephone, the author has been led to study the causes to which is due the varying intensity of the effects produced by this instrument. Here he studies more particularly the influence of the thickness of the diaphragm for a telephone of well-defined form, and for a like variation of the magnetic

field. Some experiments are described with iron diaphragms, and it is generally inferred that for all telephones of a given magnetic field there is a given thickness of the iron diaphragm which yields a maximum effect.—On the solubility of salts, by M. H. W. Bakhuis Roozeboom. This is a reply to M. Le Chatelier's critical remarks (*Comptes rendus*, March 18, 1889) on the work recently published by the author on the conditions of equilibrium between the solid and liquid combinations of water with salts, more particularly with calcium chloride.—On methylacetanilide, by M. H. Giraud. It is pointed out that the scientific name of ortho-methylacetanilide given to the *exalgine* recently prepared by M. Brigonnet, can only be applied to the substance described by Beilstein and Kuhlberg under the name of aceto-orthotoluide. It is further shown that M. Brigonnet's preparation is not new, that it was described by Hofmann in 1874, and that its true name is methylacetanilide.

Berlin.

Physiological Society, March 27.—Prof. du Bois-Reymond, President, in the chair.—Dr. Klemperer spoke on the proteid needs of the animal economy in health and in certain pathological conditions. Voit's teaching, that the human body in health requires daily from 100 to 120 grammes of proteid in order to supply its nitrogenous needs, has been recently contested from many sides; and even if the experiments on which the attacks were based were not altogether free from some defects, they still sufficed to cast a good deal of doubt on Voit's theory. The speaker had endeavoured, working from the clinical point of view, to decide the question whether an increased proteid metabolism can be prevented or diminished by an increased ingestion of carbohydrates or fats. He carried out experiments on the nutrition of two healthy persons, in which the daily dose of proteids was very considerably diminished, even down to 40 grammes, while in compensation for the lessened proteids larger quantities of fats, sugar, and easily absorbed and oxidizable alcohol were administered. The nitrogen excreted in the urine was constantly less in amount than that taken in the food, thus showing that healthy, active men can be fed with largely diminished amounts of proteid without the occurrence of any destructive metabolism of their tissue-proteids. He next proceeded to investigate whether, in diseases which are characterized by an abnormally large breaking down of tissue-proteids, this increased nitrogenous metabolism could be lessened by the ingestion of an increased quantity of non-nitrogenous food. An increased nitrogenous metabolism occurs in dyspnœa, fever, anæmia, cancer, tuberculosis, diabetes, and Addison's disease. For dyspnœa, experiments were made on animals; while for anæmia, cancer, diabetes, and Addison's disease, observations were made on the human subject, and results were obtained which corresponded to the supposition under which the experiments were started. A very considerable reduction of the nitrogen excreted in the urine was observed when only moderate quantities of proteid were given, while at the same time increased amounts of carbohydrates, fats, and alcohol, were administered. It is impossible to enter here into the interesting details of these experiments, which were all carried out by very precise methods, or into a discussion of the hypotheses which were advanced in explanation of the phenomena which had been observed.—Prof. Rosenthal, of Erlangen, gave an account of calorimetric experiments with which he had been busied for the last few years. He employed in these an air-calorimeter of special construction. It consisted of a copper vessel, of easy ventilation, in which the animal was placed; this was surrounded by an air-tight envelope, filled with air and constituting the reservoir of an air-thermometer; external to this was a covering to shield the whole apparatus from any changes in the temperature of the surrounding atmosphere. When the animal gives up to the envelope of air, per unit of time, exactly the same amount of heat as the whole apparatus radiates into the surroundings, the temperature of the air in the envelope remains constant, as also its *pressure*: hence the heat produced and given off by the animal during any known time could be measured by means of a manometer. Notwithstanding that the dog used in the experiments was fed in exactly the same way at each meal, the quantities of heat produced varied very largely, and any considerable uniformity is only obtained by taking the mean of a long series of observations. Up to about the third hour after the meal the heat-production diminishes, then rises rapidly to a maximum, and from this point, at about the eighth hour, it begins to fall again slowly and with irregularities, until the next meal. Over the whole twenty-four hours the heat-production is more uniform during the second period of twelve hours than in the first; about 20 per cent. more heat is produced during the first than during the second half of the whole day. When an excess of food was given the heat produced was always less than that calculated out from the oxidation of the food itself; but with a uniformly constant diet the mean value of the heat produced corresponded to the heat calculated for the oxidation of the food. The amount of carbonic acid gas given off by the animal was found to correspond to the heat given off during the same period only in cases where prolonged intervals of time were taken into account. When the surrounding temperature varied between 5° and 25° C., all other conditions remaining the same, a minimal production of heat was observed at 15° C.: from this point it increased uniformly in both directions, not only when the temperature fell to 5° C., but also when it rose to 25° C.—Prof. Schweigger demonstrated several pieces of apparatus, which by the use of small incandescent electric lamps, could take the place of the ophthalmoscope, and even render a binocular examination possible. They also made the measurement of refraction in the eye both simple and exact.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Useful Native Plants of Australia (including Tasmania): J. H. Maiden (Trübner).—The Psychic Life of Micro-organisms: A. Binet; translated by T. MacCormack (Chicago).—The Elements of Vital Statistics: Dr. A. Newsholme (Sonnenschein).—Catalogue of the Fossil Fishes in the British Museum (Natural History). Part 1: A. Smith Woodward (London)—Richtigstellung der in bisheriger Fassung unrichtigen Mechanischen Wärmetheorie und Grundzüge einer Allg. Theorie der Aetherbewegungen: A. R. von Miller-Hauenfels (Wien).—The Land of Manfred: J. Ross (Murray).—Bulletins de la Société d'Anthropologie de Paris, tome xi., 4e fasc. (Paris).—Mémoires de la Société d'Anthropologie de Paris, tome iv., fasc. 1 (Paris).—Journal of Anatomy and Physiology, April (Williams and Norgate).—Zeitschrift für wissenschaftliche Zoologie, xlviii. Band, 1 Heft (Leipzig).

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